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SCIENCE PROGRESS

ON THE STRUCTURE OF THE UNIVERSE

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It may be said with a good deal of truth that the ultimate object of stellar astronomy is to discover the solution to the problem of the structure and the evolution of the universe. The many recent researches upon the distances and distribution of stars, upon the relative distances of stars of different types, upon the numbers of stars of different magnitudes, and upon allied topics, have all contributed in throwing some light on this problem, which, because of its complexity and baffling nature, because also it is the most far-reaching with which the mind has to deal, has fascinated and engaged the attention of men almost from the dawn of civilisation. The Greek philosophers supposed the world on which they lived to be the centre of the universe, and that the sun, the planets, and the stars all rotated around it, and they devised ingenious theories to account for the observed motions of the planets. Our knowledge has advanced a great distance since their time, yet, although many facts have been accumulated, we have so far taken only the first few steps towards a solution. This is not surprising when it is remembered that for much of our knowledge we are dependent upon photography, the application of which to astronomy is still of quite a recent date. Except for the nearest stars, the refined measurements necessary for determining stellar distances have only been made possible by the development of photographic methods. The problem of the present structure of the universe is indeed, as Kapteyn has pointed out, the problem of the distances. If the distance of each star were known in addition to its position in the sky, its position in space would be determined, and our knowledge of the present structure would be complete. To fix the change in the structure, it would then

only be necessary to know the motion of each star. For the brighter stars we have fairly accurate observations of their positions which date back 160 years, and from comparisons with modern observations their proper motions—*i.e.* their apparent motions in the sky—can be determined with great accuracy. For the majority of the fainter stars we have not, however, sufficiently distant observations for determining accurately their proper motions. The measurement of the velocities of stars in the line of sight is also one of the most recent developments of astronomy, so that our knowledge of these is still in its early stages. As we cannot hope for some time to obtain sufficient information thus to solve the problem directly, it becomes necessary to get such indirect information as we can, and bound up with the problem of the present structure is the additional and more difficult problem of the evolution of the universe—more difficult because to mark its changes we must measure not by hundreds but by millions of years.

The questions which even a very cursory examination of the problem raises are very numerous. Is our universe finite or infinite in extent? Can we, with the aid of our telescopes, penetrate to its extremities, and number the stars? If it is finite, are there other stellar universes existing outside our own, and if so, in what relation does ours stand to them? How has our universe been evolved, what will be its end, and how long its duration? What is its form and where is its centre? To some of these questions we can give answers with more or less certainty, but to others of them we cannot yet reply.

In all these investigations the Milky Way holds a position of fundamental importance. Some part of the Milky Way may be seen on any clear night in the year, but it is seen best in early winter, when it passes near our zenith in the evening. It is a broad, luminous stream of faint stars, with many branches and dark rifts, but on the whole lying very nearly in a plane which is inclined at a few degrees to the ecliptic, and which intersects the celestial equator in the constellations of Aquila and Monoceros. It is important as being the plane of symmetry of the stellar universe, and the coordinates which express the position of a star relative to it are called its galactic longitude and latitude, the longitude being measured eastward from the point of its intersection with the celestial equator in Aquila.

It might not unnaturally be expected that the bright stars are, on the average, comparatively near to us. We should therefore expect them to be distributed more or less uniformly in neighbouring space, and to exhibit no decided concentration towards the galaxy; for, if the near stars showed such a concentration, it would indicate a conical structure of the universe, with our solar system at the apex of the cone, and it is very improbable that our solar system occupies such an exceptional position. The following table exhibits the distribution of stars down to various magnitude limits in nine zones of galactic latitude, each 20° wide:

TABLE I

Galactic Latitude Zones	Densities of stars down to various limits of magnitude.		
	6·0 m.	9·0 m.	14·0 m.
+ 90° to + 70°	0·113	2·78	107
+ 70 „ + 50	0·122	3·03	154
+ 50 „ + 30	0·124	3·54	281
+ 30 „ + 10	0·145	5·32	560
+ 10 „ − 10	0·160	8·17	2,019
− 10 „ − 30	0·154	6·07	672
− 30 „ − 50	0·129	3·71	261
− 50 „ − 70	0.124	3·21	154
− 70 „ − 90	0·125	3·14	111

The second column gives the density per square degree of stars down to the visual limit of 6·0 m. as calculated by Houzeau. The third column gives the number of stars in the *Bonner Durchmusterung* of Argelander and Schönfeld, down to a limit of 9·0 m., as corrected by Seeliger to allow for the fact that the brightnesses were overestimated in regions containing only a few stars, and underestimated in the denser regions. The fourth column contains the densities derived from the Herschel star gauges—counts of stars visible in a definite area in the field of Herschel's 18-inch reflector. These include all stars down to a limit of about 14·0 m. From the above table it will be seen that the naked-eye stars exhibit a slight but perfectly definite preference for the galaxy, indicating that a proportion of the stars which are visible in the Milky Way are either really situated in, and form part of it, or are in some way definitely connected with it. That this is so is supported by the fact that some of the brightest stars have parallaxes too small to measure.

From an examination of the near stars—which, on the average, are those of large proper motion—Kapteyn concludes, as might be expected, that these are distributed uniformly, and have no connection with the Milky Way. Referring again to the table, it is seen that the stars down to the ninth magnitude are nearly three times as dense in the Milky Way as at the galactic poles, whilst those in Herschel's star gauges are nearly twenty times as dense, this large ratio being due to the fact that the star clouds in the Milky Way contain an immense number of very faint stars. This latter result is apparently contradicted by some recent star-counts made by Messrs. Chapman and Melotte at the Royal Observatory, Greenwich, based upon a series of photographs of the whole sky taken by the late John Franklin-Adams. This investigation indicated that the galactic concentration did not vary greatly over the range 5·0 m.–17·0 m. of photographic magnitudes. An explanation of this discrepancy has been suggested by Prof. H. H. Turner—viz. that the galactic regions may contain a very large number of faint red stars visible in Herschel's telescope, but too red to affect the photographic plate. A similar effect would be caused if, as is more than probable, there is absorbing matter in space, which scatters the light of short wave-length, and makes the distant stars appear reddish, similar to lights seen through a fog, and so incapable of affecting the photographic plate. In any case, however, it is certain that there is a very marked flattening of the system as a whole towards the Milky Way.

A comparison of any of the above north galactic zones with the corresponding south zones shows that in each case the density in the southern zone is greater than that in the northern; the explanation of this fact (which is generally accepted) is that our solar system lies somewhat to the north of the galactic plane, and a close study of the course of the Milky Way in the heavens lends support to this view. Struve found that it does not trace out a great circle on the celestial sphere whose centre is our solar system, but a small circle at a distance of about 88° from the south galactic pole.

Of still greater interest is an examination of the galactic concentration of stars of different types. It is believed that the stage in its evolution at which a star has arrived can be told with considerable certainty from its spectrum. The classes in the supposed order of evolution are denoted by the letters B, A,

F, G, K, M, N. The spectra of stars of class B are characterised by strong helium lines, and those of class A by strong hydrogen lines, other lines being comparatively weak. In classes F, G, K the hydrogen lines become progressively weaker and the metallic lines more and more prominent. Stars of class G have spectra similar to our sun, which is itself a G star, and which contain numerous fine metallic lines. In classes M, N flutings due to carbon compounds make their appearance. The stars of early type are supposed to be the hottest and gradually to cool as they get older.

The spectral types of an immense number of stars have been determined at Harvard, and Prof. Pickering has tabulated the numbers of different types, brighter than the visual limit 6·5 m, in eight zones of galactic latitude, chosen so as all to have practically an equal area. His results are given in Table II., in which in each column after the first are given the number of stars of the type at the head of the column, for the eight zones whose average galactic latitude is given in the first column.

TABLE II

Latitude.	B.	A	F.	G.	K	M.	Total
+ 62·3°	8	189	79	61	176	56	569
+ 41·3	28	184	58	69	174	49	562
+ 21·0	69	263	83	70	212	57	754
+ 9·2	206	323	96	99	266	77	1,067
− 7·0	161	382	116	84	239	45	1,027
− 22·2	158	276	117	100	247	69	967
− 38·2	57	161	94	59	203	59	633
− 62·3	29	107	77	67	202	45	527

If the stars of each type were distributed uniformly over the sky, the numbers in each vertical column should be approximately equal. It will be seen that in no case is this so, and that whilst the stars of type B exhibit a very strongly marked crowding towards the galaxy, the distribution in the various zones becomes more uniform with successive spectral types, until with class M there is little or no indication that the density in the galaxy is greater than that at the galactic poles. The most remarkable concentration is, however, shown by a small class of stars known as Wolf-Rayet stars, whose spectra apparently indicate a transition stage between nebulæ and B-type stars, and which therefore

are to be regarded as preceding the B stars in the order of evolution. Of the ninety-one known Wolf-Rayet stars, seventy are in the Milky Way and all the remainder are in the Magellanic Clouds. A similar concentration is shown by the gaseous nebulæ: Hertzsprung found for the pole of their plane of concentration the position R. A. 192°·7, December + 28°·1, in close agreement with the position of the galactic pole derived by Pickering—R. A. 190°·0, December + 28°·0.

These facts must be considered in conjunction with a remarkable result first announced by Prof. W. W. Campbell, from an analysis of the line-of-sight velocities of stars measured at the Lick Observatory—viz, that the average velocity of the stars of given spectral type increases with progression of the type. This is illustrated by the following table, summarising Campbell's results:

TABLE III

Spectral class.	Number of stars.	Average radial velocities in km. per sec.
O and B	141	8·99
A	133	9·94
F	159	13·90
G and K	529	15·15
M	72	16·55

The same result has been found from a discussion of the proper motions by Boss and by the present author. The interpretation of this phenomenon is by no means easy. Although all stars are not of equal age, some having condensed from nebulous matter at a much later date than others, yet it is natural to suppose that gravitational forces have acted upon the matter composing every star for the same length of time. On the other hand, the fact that the average velocity of a star increases with its age, and that the velocities of the early-type stars are very small, seems to suggest that these gravitational forces due to the stellar system as a whole do not become effective until the nebulous matter has condensed into a star. It may possibly be that the resultant velocity of the materials which go to form the star is practically zero. However, the facts which we have gathered together would seem to indicate that the birth of stars occurs mainly in the galactic plane. The stars then acquire

velocities under the general attraction of the whole stellar system —the law of attraction being doubtless somewhere between an attraction varying directly as the distance (the law of attraction due to a uniform distribution of matter) and an attraction varying inversely as the square of the distance (the law of attraction when the attracting mass is concentrated at one point—as in the motion of the earth around the sun). The stars of early types O and B, owing to the comparatively small time which has elapsed since their birth, have small velocities and are strongly condensed in the galactic plane. The stars of later types having been born earlier have had time to acquire larger velocities and to stray further from the galactic plane, so that we find the type M stars very uniformly distributed. If this be the true explanation, it is not free from difficulties. If the stars are, as we have supposed, born in the galactic plane, the attraction under which they move will be in the same plane, so that it is not easy to see how they can move out of it. To avoid this difficulty, Prof. Eddington has conjectured that the stars of types K and M may have been formed originally in a more spherical and uniform distribution than the stars of early type, and that possibly, for some reason which we do not know, the birth of stars was retarded in the galactic plane. It is a well-known fact that the stars of early type do on the whole move parallel to the galactic plane, as we should expect, and this fact has been used by Prof. Kapteyn to predict their parallaxes with a considerable degree of accuracy. Another alternative explanation which Prof. Eddington has suggested is that in the galactic regions, richly supplied with star-forming material, the tendency has been to form large stars which have developed very slowly, whilst away from the galaxy small stars have been formed, which have run through their course of development quickly. This would account for the predominance of early-type stars in the galactic plane; but it raises other difficulties, and does not appear to explain the increase of average velocity with the progression in type. One feels that one is treading on uncertain ground, and that these hypotheses are merely speculative, with little solid foundation. The facts, nevertheless, must be of fundamental importance in any theory of the structure of the stellar universe.

A few further facts must be considered before we can form a general idea of the structure of our sidereal system. So far nothing has been said as to the average distances of stars of

various types. Many investigations have been made to determine this, and they all agree in their main results. Briefly stated, these are that the most distant stars are those of very early type, and that the average distance gradually decreases with advancing type, until the solar type is reached. The stars of this type are on the average the nearest to us. Those of still later types are more distant, and the red or M stars are the most distant of all, after the B stars. The great average distance of the helium or early-type stars is accounted for by the fact that they are mostly in the distant galactic star clouds. We may thus picture our sidereal system as consisting of a central region containing stars of various types, but chiefly the later ones, more or less uniformly distributed about their centre; outside this central region lies the belt of the Milky Way at such an immense distance from our solar system that it has not yet been measured with any certainty, constituted mainly of early-type stars, and containing many nebulæ and nebulous clouds. An influence of the galactic belt on the inner region is found in the tendency of its stars to concentrate towards the galactic plane, more particularly in the case of the earlier types—although we are not yet able to assign definitely the cause of this, nor the way in which the system has arrived at its present form.

The further question arises as to whether our sidereal universe is finite or infinite in extent. It is a well-known fact that when the number of stars of magnitude $m + 1$ is compared with the number of magnitude m, the former is found to be between three and four times the greater. Now by the definition of the magnitude of a star, a star of magnitude m gives 2·512 times as much light as a star of magnitude $m + 1$ (log. $2{\cdot}512 = 0{\cdot}4$, so that a ratio of 100:1 in apparent brightness corresponds to a difference of 5 magnitudes). It follows from these facts that all the stars of magnitude $m + 1$ give together more light than all the stars of magnitude m. This result can only hold for moderate values of m, because otherwise the heavens would shine with a blaze of light greater than the noonday sun, due to the innumerable faint stars. It follows that at some stage the number of stars of a given magnitude must reach a maximum and afterwards begin to decrease, and this leads one to expect that the total number of the stars may be finite in amount. That this is so is indicated by the counts

of stars of different magnitudes recently made by Messrs. Chapman and Melotte. The results of this count are shown in the annexed table, in which the second column gives the *total* number of stars down to the limit of magnitude (photographic) in the first column.

TABLE IV

Magnitude.	Number of stars.	Magnitude.	Number of stars.
2	38	11	698,000
3	111	12	1,659,000
4	300	13	7,646,000
5	950	15	15,470,000
6	3,150	16	29,510,000
7	9,810	17	54,900,000
8	32,360	18	(91,200,000)[1]
9	97,400	19	(144,000,000)[1]
10	271,800		

It will be seen from this table that after the 15th magnitude, the ratio of the number of stars of any given magnitude to the number of the next brighter magnitude is less than two. By extrapolation from these results Chapman and Melotte estimate that the number of stars down to a magnitude of about 22·0 is half the total number of stars, which, they conclude, "is not less than one hundred thousand millions, and cannot much exceed twice this amount." This result is dependent upon the assumption that there is no scattering nor absorption of light in space; but although there is considerable evidence that such an absorption does occur, the most reliable estimates of its magnitude indicate that its amount is so slight that it cannot very greatly modify the general conclusion. It follows that we must regard our stellar universe as finite in extent—although its dimensions are so vast as to stagger the mind—and contemplate the possibility of the existence of other, and independent universes, outside it.

It has been shown above that our universe consists of a central mass more or less globular in shape, and that outside this lies the Milky Way, stretching out to immense distances and containing a large proportion of faint stars. This has suggested that the system may really be a spiral nebula, and

[1] These are extrapolations.

that the other spiral nebulæ may be separate universes. Astronomers divide nebulæ into three classes. These are:

(1) The irregular nebulæ of which the Great Nebula in Orion is the most conspicuous example. This class comprises nebulæ of many varied shapes, whose names are often given from a more or less striking resemblance to some terrestrial object, such as the Dumbbell Nebula, the Crab Nebula, the North America Nebula, the Keyhole Nebula, and many others. To the same category belong the nebulous backgrounds, obviously associated with stars, such as the nebulosity around the Pleiades, and in the constellation of Taurus. These irregular nebulæ occur mainly in the neighbourhood of the galaxy, and undoubtedly belong to our system. In many cases they show undeniable connection with certain stars.

(2) The second class is known as the Planetary or Gaseous nebulæ. They were first classified as such by Sir William Herschel, but he did not originally recognise their nebulous nature. "We can hardly suppose them to be nebulæ," he says; "their light is so uniform as well as so vivid, their diameters so small and well defined, as to make it almost improbable that they should belong to that species of bodies." He considered that they might be planets attached to distant suns, but recognised that this supposition was untenable. Their spectra present many analogies to the spectra of the Wolf-Rayet or gaseous stars, and, like the latter, they occur almost exclusively in the Milky Way. This apparent connection seemed to indicate that nebulæ passed, in the ordinary course of evolution, into the gaseous or early-type stars. This hypothesis received a rude shock when measurements of the line-of-sight velocities of these objects became possible. If by evolution they passed into early-type stars, their average radial velocity should be very small, of the order of 5 or 6 km. per second. The first few results obtained indicated that this was far from being the case, and enough measurements have now been made to assert definitely that, so far from their velocities being small, their average velocity is considerably greater than that of the late M-type stars—the average line-of-sight velocity of the latter being about 17 km. per second, and that of the planetary nebulæ of the order of 40 km. per second. Several of them have been found to possess extremely high velocities, even as great as 200 km. per second. How the planetary nebulæ are to be

fitted into the general scheme of stellar evolution remains at present one of the unsolved problems of astronomy. The suggestion has been made that they have been formed from stars through collisions with other stars. One would naturally expect such collisions to occur most frequently in the Milky Way, where the stars are densest, but their huge velocities are not thus accounted for.

(3) The third class of nebulæ, and the class which is by far the most numerous, is that of the Spiral nebulæ. Their discovery was the one striking achievement of the great Parsonstown reflector, constructed by Lord Rosse. Fath has estimated that there are at least 160,000 nebulæ of all kinds, so that the number of spirals must be very great. By far the most conspicuous object of this class is the great nebula in Andromeda, easily visible to the naked eye as a small blurred patch, very different from a star in appearance. It was the only nebula discovered before the invention of the telescope. Photographs show it to consist of a bright central nucleus, with long, spiral, nebulous arms wreathing around it. The spiral nebulæ that we know are placed at all inclinations to our solar system. The Andromeda nebula is seen obliquely. Many, like the Whirlpool Nebula, are seen perpendicular to the plane of the spiral arms; in this case, and in others, the two arms are clearly seen starting out from opposite edges of the central nucleus. Some, again, are viewed edge on, and in these the spiral arms are seen as a narrow line, evidence that they lie in one plane. This line is seen dark where it crosses in front of the central nucleus, owing to the scattering which the light from the latter undergoes in passing through the arms. This is the kind of nebula to which our sidereal system has been compared. From a careful consideration of the structure of the Milky Way, obtained by combining all the available photographs, C. Easton has recently given a hypothetical representation of it in the form of a spiral, in which account has been taken of all its prominent features. This attempt to account for the structure of the Milky Way on the spiral hypothesis is very interesting and instructive.

Now if the known spiral nebulæ are distinct from our sidereal system, if they are indeed "island universes" in space, one would expect them to show a uniform distribution relative to the galactic plane. If, on the other hand, they were found to show, as do the planetary and irregular nebulæ, a marked

predominance in the galaxy, then the theory of their independence would fall to the ground. As a matter of fact, their distribution is quite different from that of the other two classes. Instead of being found chiefly in the Milky Way, they appear to avoid it, and largely preponderate in the neighbourhood of the galactic poles, although a few are found in the Milky Way. This distribution might be supposed to indicate some connection with our system, but this is not necessarily the case. If they lie right outside it, it is quite conceivable that, in the direction of the Milky Way, none are seen owing to the light which they emit being scattered and cut off by the far-extending stars and star clouds of the Milky Way. Further research will be necessary before the question can be regarded as at all settled, but at present the evidence, on the whole, seems in favour of it, and it has the advantage of forming a connecting link between some of our facts and of giving a coherent picture for our minds. It may perhaps be mentioned that the very large line-of-sight velocities that have recently been found for some of the spiral nebulæ may possibly indicate independence of our system.

One always associates a spiral with the thought of rotation, and it is undoubted that some at least of the nebulæ are in rotation. Attempts have been made to find whether our system shows any evidence of rotation about an axis perpendicular to the Milky Way. The problem is a very difficult one, involving an accurate knowledge of the precession constant, and of the magnitude and direction of the solar motion, in addition to which it is complicated by the effects of star-streaming. It is therefore not surprising that, up to the present, no concordant results have been obtained beyond the proof that the rotation—if it exists—must be very small in amount. The search for a great central sun—the hub of the universe—about which the whole system is turning is one that appeals strongly to man's imagination, and several attempts have been made to discover such. Mädler decided upon Alcyone, the brightest star in the Pleiades, but this supposition is untenable. If such a central sun exists there is little doubt but that it must be situated in the galactic plane, whereas Alcyone lies far outside this plane. Easton, on the other hand, decided upon a centre situated in the constellation of Cygnus, a rich galactic region containing many nebulæ. It has been mentioned above that, as a result of the study of the distribution of stars in galactic latitude, it has been

concluded that our solar system lies slightly to the north of the galaxy, so that Easton's conclusion cannot be admitted.

A more recent discussion by O. W. Walkey indicates that Canopus may be the sidereal centre. Although further evidence is necessary before this can be definitely asserted, yet this supposition appears more reasonable than any previous one. Canopus is the second brightest star in the heavens, its magnitude being 0·86. In general, it is safe to assert that the bright stars are the near ones, but this is certainly not the case with Canopus, whose parallax was investigated by Sir David Gill. Using eighth-magnitude stars as comparison stars, Gill found for it a zero relative parallax, and this careful determination therefore indicates that its parallax is the same as that of the comparison stars—*i.e.* of the order of a few thousandths of a second of arc. It follows from this that Canopus is probably from ten to one hundred thousand times as luminous as the sun. One feels that such a star, one of the greatest, if not the greatest sun of which we have any knowledge, has a claim to our consideration, as being very suitable for the sidereal centre.

The position of our sun relative to the galactic plane can be fixed, as far as its galactic latitude is concerned, with a considerable degree of accuracy from the counts of stars in various regions of galactic latitude. The determination of our lateral displacement relative to the plane is not nearly so easy. Were the Milky Way a band of stars of uniform density the matter would be comparatively simple, for it is obvious that the number of stars in the Milky Way included within a range, say, of 5° of galactic longitude would reach a maximum for the 5° which included the centre of the Milky Way, and it would simply be necessary to determine the direction in which the stars of the Milky Way are the densest. The problem is complicated, however, and the results obtained rendered uncertain by the local irregularities of the Milky Way—the occurrence of regions of exceptionally great star density, and the numerous branches leading off from the main track. Fairly concordant results have nevertheless been obtained by Walkey who has discussed the question, using counts of stars down to various limits of magnitude and of various types. He concludes that the sidereal centre lies approximately on the 230° galactic meridian. Further investigations will be necessary to confirm this result, but at present it may be taken as a first approximation. From this result, by

estimating the relative vertical and lateral displacements of our sun relative to the sidereal centre, Walkey concludes that the latter lies on the 230° galactic meridian in about 30° S. galactic latitude, which gives a direction passing very close to Canopus, and that its distance from our sun is about four hundred light-years, although much weight cannot be attached to this estimate. In support of the contention that Canopus is the sidereal centre, it is further necessary to show that it is situated in the plane of the galaxy, and that its distance, as derived from the distances of the opposite galactic streams, agrees with its measured distance.

This cannot be done at present, because it is impossible to measure the immense distances concerned with any accuracy. The Milky Way may be at any distance from 1,000 parsecs upwards (one parsec being a distance corresponding to a parallax of one second of arc; it is equal to 3·26 light years), and although it is certain that the parallax of Canopus is less than 0·01″, yet a change in the assigned value by a few thousandths of a second of arc one way or the other will halve or double its distance. But although the theory cannot, at the present time, be either conclusively proved or disproved, yet some support can be obtained for it. From a statistical study of the measured line-of-sight velocities of stars, the velocity of the sun in space can be deduced, and also the direction of its motion, though this latter can be derived more accurately from a study of the proper motions. This velocity being known in magnitude and direction, any observed line-of-sight velocity can be freed from the effects of solar motion and the absolute velocity of the star in the line-of-sight obtained. When this is done for Canopus, it is found that, within the limits of experimental error, its line-of-sight velocity is zero. It must here be remembered that the observed stellar velocities are purely relative: the deduced solar velocity is a velocity relative to the mean of the stars or to the mass centre of the system, and in fact different values are obtained from stars of different types. Our whole sidereal universe may itself have a motion of its own through space, just as our own solar system has amongst the stars of our stellar universe, but such a motion can only be detected by means of observations of bodies lying outside our system. If the view that the spiral nebulæ fulfil this condition be supported, then when the radial velocities of an adequate number of them have been measured,

we may be able to find whether our system as a whole has any motion.

Remembering this, it follows that the sidereal centre must be at rest relative to the mean of the stars, and the fact that Canopus has no motion in the radial direction, whilst not proving that it is the sidereal centre, is a link in the chain of evidence. If now we assume that it actually does occupy this position, it follows that it can have no velocity in a direction perpendicular to the line of sight, and therefore its observed annual proper motion of ·0184″ must be parallactic in origin, being an apparent motion due in reality to the motion of our own sun. The known speed of this motion provides a means of calculating the parallax of Canopus, upon the above assumption, and the resulting value is ·0067″, corresponding to a distance of 149 parsecs. This is the order of magnitude of the quantity to which Gill's direct determination led. This distance, being assumed correct, provides a base line from which to measure other distances in our system, and the resulting distance of the galaxy is found to be about 2,000 parsecs, or about 6,500 light-years. We can only say, in reference to this value, that it is of the order of magnitude usually assigned. There for the time being we must leave this theory. The evidence, so far as it goes, is concordant, but it must be left to the future, when, perhaps, some new and indirect means of determining accurately these immense distances may be evolved, to establish it firmly or to disprove it. The idea that the centre of our system is occupied by an immense sun, many thousands of times larger and more glorious than our own sun, and that round about it are millions of lesser suns of various sizes, together forming the nucleus of an immense spiral nebula, of which the spiral arms coiling around the nucleus appear to us as the Milky Way, and that this to us immense system is but one, and perhaps a comparatively small, island universe amongst thousands or millions of other island universes in space, is an idea which by its magnificence appeals to the mind of man. Upon what basis of truth this conception is founded I have endeavoured as briefly as possible to elucidate in the present article. So brief a discussion of so great a theme is necessarily, both from limitations of space and from present incompleteness of knowledge, very inadequate, and many important and related phenomena, such, for example, as that of star-streaming, or Russell's theory of "giant" and "dwarf" stars, which changes

the commonly accepted order of evolution, have not been touched upon at all. Meanwhile almost daily new knowledge is being gained, and new light being thrown upon these problems, and although in some cases the new facts seem to find no place ready for them in the theory, and although in others they lead to a recasting of many of our ideas, yet progress is continuous, and there can be no doubt that in course of time many of the most puzzling facts will find a natural explanation as the theory developes.

LOGIC: A REJOINDER TO MISS STEBBING

By CHARLES MERCIER, M.D., F.R.C.P.

Miss Stebbing's reply to my charges against logic does not seem to me successful; but as it does seem successful to some people, a rejoinder may be permitted. She says, "It is a commonplace amongst present-day logicians that the excessive claims made on behalf of the syllogism by *e.g.* Whately, and no less by J. S. Mill, must be rejected." I have given some logicians the credit, not in the article in Science Progress, but in another place, for expressing a half-hearted doubt whether, after all, the syllogism is entitled to all the powers claimed for it; but I said, and it is true, that this half-hearted admission has no real influence in leavening their doctrines. They have been compelled, as Galileo was compelled, though by different methods, to make the admission in words, but they have not altered their teaching in the slightest.

The logician is an elusive and eel-like person. When you try to pin him down to some flagrant absurdity that is taught in all the books, and ask him if it is consistent with common sense and manifest fact, he puts you off airily with this evasion. "That," he says, "is no longer taught. No logician would teach that now." "But it is in all the books?" At this he smiles a superior, not to say a supercilious smile, and tells you that you must not go by the books, and leads you to infer that the true doctrine is to be heard in his class-room only. Now I maintain that in order to discover the actual state of any science at any time, what doctrines are accepted and prevail, the only method is to take as a guide the text-books that are most widely used by students in preparing for examination in that subject. These books will make only a passing reference, or none at all, to doctrines that are held so lightly and by so few that they have not yet been incorporated into the accepted scheme of doctrine, but they dare not exclude any doctrine, however novel,

that has any considerable body of support, or that is in the air, for these are just the topics that examiners love to set questions upon, and if the text-book did not prepare students to answer them, it would very speedily drop out of use and be superseded by another. I take, therefore, five well-known books, whose vogue is proved by the number of editions and impressions that have been issued. The most popular book on Logic is, I suppose, that of Dr. Fowler, now in its tenth edition. It devotes 54 pages to the syllogism and none to any other form of mediate inference. Prof. Carveth Read's *Logic* is in its fourth edition, and has been reprinted ten times; it has 52 pages on the syllogism, and none on any other form of mediate inference. Mr. Welton's *Manual of Logic* is now in the fifth impression of the second edition; it devotes 134 pages to the syllogism, and less than 3 pages to all the other forms of mediate inference put together. The *Formal Logic* of Dr. Keynes, now in its fourth edition, gives 146 pages to the syllogism, and 2½ pages to other forms of mediate inference. Dr. Mellone's *Text-book of Logic*, of which my copy is of the third edition, gives 75 pages to the syllogism, and none to any other form of mediate inference. I submit, therefore, that my assertion that "the central doctrine of logic is that there is only one mode of reasoning or inference, only one way in which we can reason from premisses to a conclusion, and that this is by means of the syllogism," cannot be seriously impugned.

"Nevertheless," says Miss Stebbing, "many of the arguments which he does adduce are really syllogisms in disguise—that is to say, they are arguments only if there be *assumed* premisses the production of which suffices to turn the argument into a correct syllogism." I draw attention to the sign of quantity, "many," a sign, by the way, which Miss Stebbing has no business to use, for it is unknown to logic; but it appears that Miss Stebbing, like other logicians, cannot conduct an argument within the boundaries of logic, but must go outside of them into the world of common sense. As a logician she should have said "some of the arguments," and although you never know in logic whether "some" may not be all, for logicians have a rooted and irrational aversion to the use of "some only," yet it is noteworthy that Miss Stebbing does pointedly abstain from asserting that all my arguments can be expressed in syllogisms. That some of them can be so expressed I admit, and I was quite

prepared for her objection, as may be judged from the following quotation from p. 135 of my *New Logic*: "Nothing is easier than to fake a syllogism that purports to show the process of an argument. We have only to garble the premiss so as to bring it into 'logical form,' then to pretend that the argument is an enthymeme, and to invent a premiss to suit the purpose, and we have the argument expressed in a syllogism." It would be unkind to complete the quotation, and out of deference to Miss Stebbing's sex I interrupt it at this point.

My first specimen argument was:

If The bed contains nothing but geraniums and violas,
then It contains no asters.

Miss Stebbing converts this into a syllogism by garb— I mean by altering my premiss into:

All the flowers in the bed are geraniums and violas.

Now far be it from me to doubt Miss Stebbing's word, but here she has taken upon herself the responsibility of making a categorical assertion, and I am not bound to accept such an assertion without proof. The burden of proof lies upon her. She says all the flowers in the bed are geraniums and violas. I regret the necessity of making the demand, and I make it with every possible courtesy, but really I must demand proof of this assertion. Is she sure of her facts? Is it her own observation, or did she obtain her information from some one else? If she examined the bed herself, what proof have I that her sight is sufficiently good, her knowledge of horticulture sufficiently extensive, her care sufficiently sedulous, to satisfy me that she has not overlooked a small aster concealed beneath the rank growth of geraniums and violas? If she gained her information from some one else, I shall need to know these particulars with respect to her informant, and more, I must be satisfied of his *bona fides* and truthfulness. I do not want to hurt Miss Stebbing's feelings, and I trust that she will not feel aggrieved at my questions, but I am obliged to put them, for they go to the very root of the matter. One of the main purposes of my *New Logic* is to drive into the heads of logicians the profound and radical difference between the *argumentum ex postulato* and the *argumentum in materiâ*. They belong to different realms of logic, and the mode of argument applicable to the one is totally

different from the mode of argument applicable to the other. They are based on different principles and governed by different methods; and Miss Stebbing calmly converts my argument from a supposition into a material argument; she does not seem conscious that the alteration makes the slightest difference; and then she accuses me of ignorance of Logic! I admit that I am only a novice, but I do know better than that.

But this is not the worst. Logicians are so in the habit of garb—altering statements to bring them into "logical form," that they are quite indifferent, and I imagine insensible also, to the serious alterations of meaning that they produce in the process. "Logical form" is such a passion with them that beside it meaning becomes insignificant, and of no account. I put the supposition, "If the bed contains nothing but geraniums and violas——" Miss Stebbing in effect tells me, "You don't know how to express your meaning. What you ought to say is, 'All the flowers in the bed are geraniums and violas.'" I beg your pardon, Miss Stebbing. I do not pretend to any greater knowledge of horticulture than of logic, but I have enough to know that what you say is botanically impossible. No flower can be both a geranium and a viola. No single one of them can answer your description; and if not one, then *à fortiori* not all. "Logical form" may be a worthy object of worship, but, not being a logician, I prefer accuracy of statement.

Miss Stebbing treats my other arguments with the same servility to logical form and the same breezy indifference to meaning. I commented adversely upon the uniform practice of logic for two thousand years of keeping the middle term out of the conclusion, and showed by an instance that it is possible, and may be very useful, to get the middle term into the conclusion. Miss Stebbing meets this by garb—altering the major premiss, altering the minor premiss, and so arriving at a conclusion from which the middle term is omitted. I said in my article that logicians are not better but worse reasoners than other people, and I think this is a case in point. Miss Stebbing is a person of exceptional mental power. In spite of her logical training, she always quotes me correctly. If her reasoning power had not been corroded and attenuated by the study of logic, she would, I am sure, have made some great achievement, but being a logician, how does she argue? I say that it is sometimes useful to do a certain thing, and I give an instance in

which the useful thing is done. Miss Stebbing refutes this by showing that in another instance it is quite possible not to do that useful thing. I know it is. The possibility of avoiding useful things by employing logical methods is illimitable. In my *New Logic* I have given hundreds of instances of the inability of the old logic to reach conclusions that are both useful and obvious. In this very case, I could have left the middle term out of the conclusion of my own argument if I had wanted to. But I didn't. I wanted to get it in. It is useful. It gives me information that I want, information that Miss Stebbing's argument does not give, and that no logical argument can give from the information in my possession. Miss Stebbing says, in effect, that I have no right to my information because the argument by which I arrived at it is not in logical form. But then you see, Miss Stebbing, I am not a logician, and I do not care a pin whether my argument is in logical form or not. All I care about is whether it is valid, and you do not venture to deny that it is valid, Miss Stebbing. The utmost you can say is that your own conclusion brings out the force of your argument. Perhaps it does, but it does not bring out the force of mine, nor does it give me the conclusion I want, and can get by my own method of reasoning.

Miss Stebbing does not deny the validity of any of my arguments, her only objection to them is that they are not in logical form, and she kindly tries to put them into logical form for me. But she does not succeed. For my part I do not think the attempt is worth making. The arguments are manifestly and undeniably valid as they stand, and nothing would be gained by perverting them into the cumbrous and clumsy forms of logic, even if it could be done; but it cannot be done, and Miss Stebbing does not do it.

As an example of an argument in which the middle term is undistributed I gave the following:

If Hannibal crossed the Alps,
and The part of the Alps that he crossed is impassable for elephants;
then He took no elephants across with him.

The middle term is the Alps, and it is not distributed because it is the predicate of an affirmative premiss, and consequently, although we do in fact refer to the whole class of the Alps, the

convention of logic requires us to suppose that we do not. In the second premiss part of the Alps only is referred to, and part of a class is an undistributed term.

To this Miss Stebbing replies that the middle term is distributed in *both* premisses. I welcome the admission, but it is in flat violation of the rules of logic. No doctrine of logic is more irrevocably settled than that the predicate of an affirmative proposition is undistributed; and when Miss Stebbing denies this, she makes a slip that would ensure her rejection at any examination in any university in the world. I heartily rejoice in her assertion that in the second premiss the part of the Alps refers to the whole of that part. I have strenuously urged in my *New Logic* that this is the proper interpretation, but it is opposed to the unanimous teaching of the books, and when I contended for it at the Aristotelian Society, I was almost torn to pieces by wild Aristotelians. They stopped their ears and ran upon me, crying, "This man blasphemeth: stone him to death!" With open arms I welcome Miss Stebbing as my first convert from the old logic to the new.

As to (5), I blush to have to call Miss Stebbing's attention to the Fallacy of Composition. Logic says that it is impossible to get a term distributed in the conclusion unless it is distributed in one of the premisses; and I give an argument in which a term is distributed in the conclusion although it is not distributed in either premiss. This is the argument:

If Some of the crew manned the jolly boat,
and Others of the crew manned the long boat;
then The whole of the crew were enough to man both these boats.

Here the whole of the crew is referred to in the conclusion, although part only of the crew is referred to in each premiss, and the rule of the syllogism is broken. Miss Stebbing says that the argument obeys the rule it professes to break, for "'the whole of the crew' is a summation of the two terms in the two premisses, viz. 'some of the crew' and 'others of the crew,' which together distribute the term used in the conclusion." This is quite a mistake. The whole of the crew is not a summation of these two terms. There were other boats on board the ship, a lifeboat, a dinghy, and a yawl; and after the jolly boat and the long boat were manned there were still

enough of the crew left to man all the other boats, so that the two terms in the premisses do not together distribute the term used in the conclusion. But if they did, the rule of logic would still be broken, for it says that a term must not be distributed in the conclusion unless it is distributed in *one* of the premisses, and the whole of the crew is not referred to in either of the premisses any more than it is in both taken together.

It would be easy to show that in the other instances Miss Stebbing's criticisms are equally invalid, but it would be tedious to do so, and I pass on to her general argument. She points out my two gravest mistakes: (1) I assume that the traditional syllogistic rules are intended to apply to *all* possible reasonings, and (2) I consider that the middle term is not essential for valid inductive reasoning.

With respect to (1), I trust I make no assumption without sufficient grounds. I have shown at the opening of this rejoinder the importance attached by logicians to the syllogism in comparison with other modes of mediate inference. If we may take the space allotted to them respectively as an indication of their relative importance, the syllogism ranks as 460, and all the other modes of reasoning put together as 6. Even Miss Stebbing herself, though she gives away the syllogism with one hand, by admitting that it is not the exclusive form of reasoning, immediately takes it back with the other by saying that it is coextensive with every form of argument; and she goes on to say that however complicated the reasoning may be it can always be broken up into a series of syllogisms; and this is the contention of every logician. I am quite content with these admissions. No one with any experience of logicians expects them to be consistent, and if they like to say that the syllogism is not the exclusive form of reasoning, but that it is the form in which all reasoning can be cast, and must be cast if it is to be tested, I know no way of preventing them.

My second mistake, so Miss Stebbing says, is much more serious; and then she proceeds to read me the usual lesson that has been read to me so many times by so many different logicians, upon the necessity of a middle term, and upon "the well-known general principle" that all reasoning consists in the application of general rules to particular cases. Every logician finds it incumbent on him or her to read me this lesson, and to do so in much the same manner as a senior wrangler might teach the

multiplication table, or a Paderewski the five-finger exercises, to a little child. They all explain it to me as if I had never heard it before, and are quite ignorant, or oblivious, or indifferent to the fact that I have long ago examined it, refuted it, rejected it, and denounced it. Bless you, Miss Stebbing, I know the logician and all his erroneous doctrines. I have read his works, and I do not agree with them. If I may trouble you to refer to my *New Logic*, which you are going to tear to pieces at some future date, you will find in it hundreds of arguments that contain no universal, and yet reach valid conclusions; and apart from my own discoveries there is the argument *à fortiori*, in which many a logician has searched for the universal, and no logician has found it. I will not go over the ground again, for I am as tired of the universal as any superannuated governess of the five-finger exercises. I could go on from now till this time next year giving you examples of reasoning without universals, reasoning that does not consist in the application of general rules to particular cases, but I will only trouble you with one, and I will give you from now till next year to find the universal in it—I say find the universal, I do not say invent a universal and pretend that you have found it in the argument, where it does not exist:

If There are more little pigs than there are teats,
then One little pig must go without.

"Continuous development in the light of criticism," says Miss Stebbing, "is the sign of a progressive science." How true! how noble! and how totally inapplicable to logic! But what, I would ask Miss Stebbing, is utter stagnation and wooden impenetrability to criticism a sign of? I fully agree with her; and how pleasant it is to be able to agree!—I fully agree with her in her appreciation of Mr. Alfred Sidgwick's criticisms of logic, but do any of the five books that I quoted at the beginning of this paper, or does any other orthodox book on logic, show one hair's breadth of modification in consequence of these criticisms? Not one. Miss Stebbing is of opinion that if I had confined myself to such criticism as Mr. Alfred Sidgwick's I should have done something to aid in the development of logic. I doubt it. Where Mr. Sidgwick himself has been unsuccessful it is very unlikely that I should have succeeded; and besides, I do not want to aid in the development of logic. I want to sweep the

whole nonsensical system away, root and branch; lock, stock and barrel; bag and baggage; and to supersede it by my own rational system. Miss Stebbing reproaches me with regarding logic merely as an inexhaustible field for the exercise of my own facetiousness. I assure her that is not so. It is true that I cannot help laughing at it: who could? Who could witness grown men, otherwise fit to be at large, writing reams of stuff on the thesis, "Whatever is, is," and seriously discussing whether the soul is not square, and virtue is not red? But my laughter is mingled with bitterness of tears to see so much labour, so much industry, so much ingenuity, so much subtlety, so much enthusiasm, wasted on such barren and worthless tasks. A logician always reminds me of a squirrel in a cage. He is for ever climbing and climbing, and he never gets a step higher. What progress has logic made since Aristotle's day? Nay, what department of logic has not deteriorated? "An excessive sense of humour," says Miss Stebbing, "is apt to destroy one's sense of proportion." I am afraid she means me; but if so, she could scarcely have gone farther astray from the fact. I don't know that my sense of humour is excessive, but if it is, it has most certainly not destroyed my sense of proportion. On the contrary, I have so keen a sense of Proportion, or Analogy, that I have restored it to its proper place, as the third cardinal process of reasoning, after it has been ignored, neglected, despised, and confused with other reasoning processes for two thousand years.

One little grievance I have against Miss Stebbing. She declares my assertion, that logicians exclude signs of quantity other than *all*, *some*, and *none*, is "entirely false." These are strong words, and have given me much distress. I trust they indicate rather Miss Stebbing's indignation with my attack upon her cherished logic than her considered opinion. She gives three exceptions, but what are they among so many? "Dr. Bosanquet," she says, "would certainly not adhere to the rule." Oho! then there is a rule? "De Morgan, too, works out a numerically definite syllogism." True; so he does. But has any other logician treated De Morgan's numerically definite syllogism as anything but a freak? "And Dr. Keynes admits other signs of quantity." Very liberal of Dr. Keynes; but does he admit all the other signs? Does he admit more than one other, and does he not admit this but grudgingly, as a half

admission? And has any one else followed him? Come, Miss Stebbing, now so many months have elapsed, and time heals all wounds, you know, won't you soften that hard, harsh expression, "entirely false"?

Let me say in conclusion how charmed I am to make Miss Stebbing's acquaintance, and with what enthusiasm I welcome her desertion of one doctrine at least of the old logic, and her acceptance of the new in its stead. It warms the very cockles of my heart to find that, after she had taken up her parable against me, like Balaam the son of Beor, of Pethor, she repented at the persuasion of a certain kind of animal, and adopted a contrary attitude. Miss Stebbing's name will go down to posterity as that of my first disciple.

CARBON: ITS MOLECULAR STRUCTURE AND MODE OF OXIDATION[1]

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As it often happens in the sphere of ideas, the conception ot the mode of oxidation of carbon has passed through several stages of development, gradually expanding with the accumulation of observations and experimental evidence.

During the last few decades the *reduction theory* of Lang (*Zeit. Phys. Chemie*, 1888, **2**, 62) has been replaced by that of *gradual oxidation* by Baker (*Phil. Trans.* 1888, **A 179**, 571) and Dixon (*J.C.S.* 1896, **69**, 774; 1899, **75**, 630), which in its turn is being substituted by the *theory of complexes* propounded by Rhead and Wheeler (*J.C.S.* 1910, **97**, 2181; 1911, **99**, 1140; 1913, **103**, 461).

After careful consideration we find that none of these theories is either absolutely wrong or a complete representation of the actual reaction in all its stages. Considering the reaction in its complete form we find that the three proposed theories rather supplement than replace one another in solving the problem of oxidation; each of them represents a more or less partial view of the phenomenon, the true explanation of which will require the correlation of these theories with one another and all the facts known up to now considered from a logical standpoint. In making this attempt we shall postulate the following three fundamental assumptions as a basis:

i. A carbon molecule is polyatomic.

(This is suggested by its high volatilisation-point and products of moist oxidation.)

ii. A carbon atom is potentially always tetravalent.

(Gomberg's work on triphenylmethyl and Nef's on poly-

[1] This paper (in a less developed form) has been read before the Manchester Literary and Philosophical Society, *Memoirs and Proceedings*, vol. 58, p. ii, 10, 1914.

methylene compounds do not necessarily imply the non-tetravalency of a carbon atom.)

iii. Carbon exists in three allotropic modifications.

(Several new modifications suggested by Brodie, Berthelot, Luzi, and others have been proved by Moissan and Le Chatelier to be either compounds or solutions and mixtures of carbon with some other element.)

Having to deal with the combination of carbon with oxygen, it is obvious that the knowledge of the mechanism of such a combination must be of essential importance to us. It seems there are two and only two possible hypotheses which can explain the process:

1. The carbon molecule disintegrates at the first instant into single atoms, which combine subsequently with oxygen.

2. Oxygen combines at the first instant with the atoms inside the carbon molecule, next follows a disruption of the complex and the formation of the known oxides.

Which of these two hypotheses is the correct one? Can we reasonably suppose that by the mere presence of oxygen, which has not yet reacted chemically, the carbon molecule falls to atoms, which would imply at least the volatility of carbon at a temperature below red heat?

I think the first hypothesis is, at least, improbable. Now, what can be said about the second?

(*a*) It is in complete agreement with the most recent experimental evidence.

(*b*) It is quite logical.

(*c*) It explains facts.

(*d*) It is the only one left.

Accepting the second hypothesis we are immediately confronted by a multitude of complexes, requiring in their turn a clear conception of a carbon molecule.

What is a carbon molecule?

Is it a formless mass of atoms in a chaotic state, or is it organised similar to the assumed structure of carbon compounds?

Our assumption will be—organisation.

Now, if carbon is always tetravalent, what is the constitutional, or as Butlerow calls it, structural, formula for a carbon molecule?

Not much work has been done up to the present in this direction.

Victor Meyer (*Berichte*, 1871, **4**, 810; *Liebig's Ann.* 1875, **180**, 195), from results obtained on moist oxidation of carbon, assumes a carbon molecule to be polyatomic.

Kekulé (*Zeit. für angew. Chemie*, 1899, 950) regards a carbon molecule as consisting of 12 atoms.

Barlow and Pope (*J.C.S.* 1906, **89**, 1742) suggested the possi-

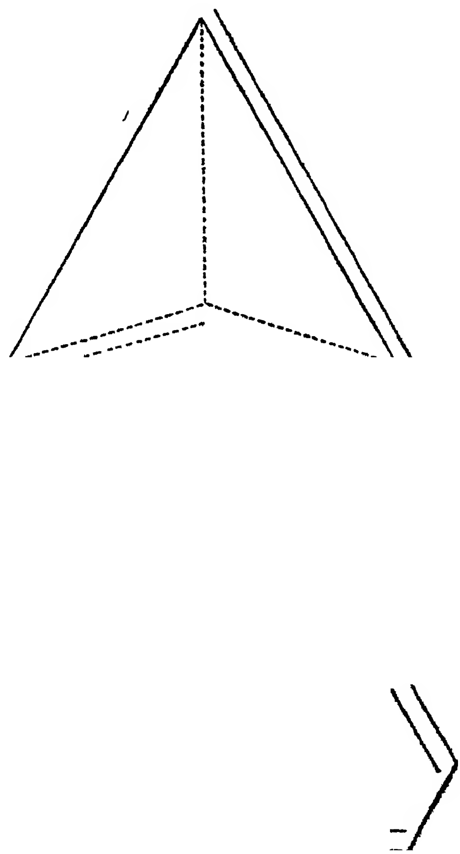

FIG. 1.

bilities of a tetrahedron and triphenylene configuration for a carbon molecule (see fig. 1).

Dewar (*Chem. News*, 1908, **97**, 16) proposed a concentric formula (see fig. 2), basing his view on the oxidation of amorphous carbon to mellitic acid—$C_6(COOH)_6$.

Redgrove and Thomlinson (*Chem. News*, 1908, **98**, 37) suggested certain modifications of Dewar's formula.

Aschan (*Chem. Zeit.* 1909, **33**, 561), criticising Dewar's suggestion, put forward the formula (see fig. 3).

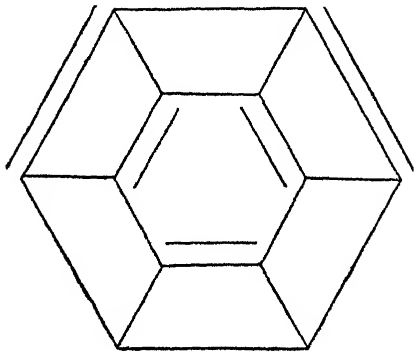

FIG. 2.

Dimroth and Kerkovius (*Liebig's Ann.* 1913, **399**, 120) thought a carbon molecule to consist of pentagons as well as hexagons.

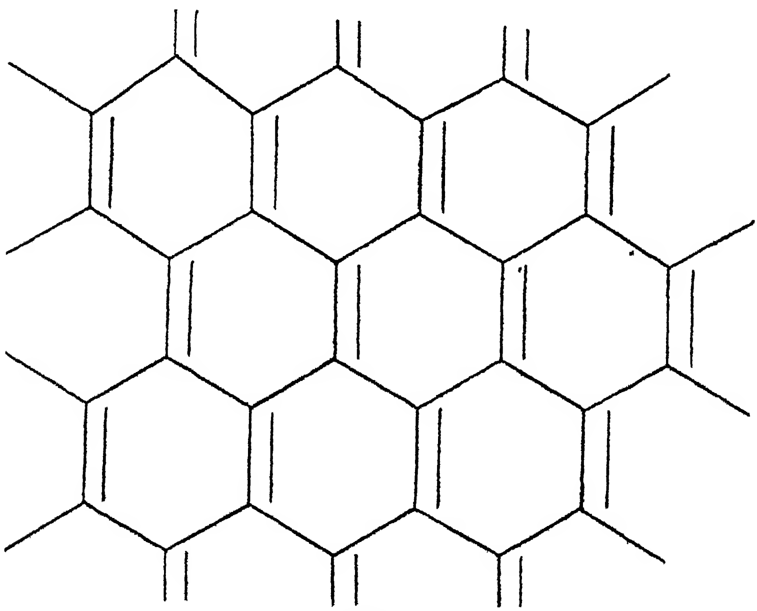

FIG. 3.

Bragg (*Proc. R. S.* 1913, **A 610**), studying crystalline structures by means of an X-ray spectrometer, advanced a three-dimensional configuration (see fig. 4) for a molecule of diamond.

Moseley (*Phil. Mag.* 1913 [vi], **26**, 1024; 1914 [vi], **27**, 703) applied the photographic method of X-ray spectra to the study of crystal structures.

Barlow (*Proc. R. Soc.* 1914, **A 623**) discussed Bragg's conclusions from the crystallographic point of view.

Ewald (*Phys. Zeits.* 1914, **15**, 399), criticising Bragg's reflection method, described a new method based on the study of Laue-graphs for the accurate determination of the crystal structures.

Hans Meyer (*Monatsh.* 1914, **35**, 163), discussing the carbon "molecule," puts stress on the difficulty, if not the futility, of trying to define the chemical entity of the three forms of carbon.

It is clear from the above that a method of investigation so

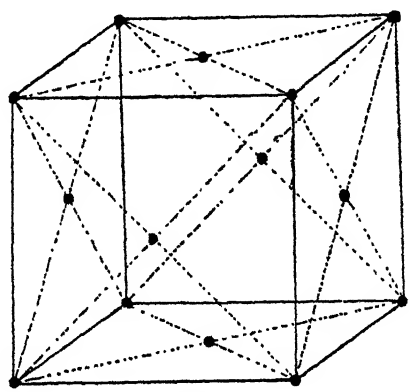

FIG. 4.

general as to include simultaneously all three forms of carbon has not yet been proposed. Whilst the method of moist oxidation can be applied more or less successfully to amorphous carbon and graphite, the X-ray spectrometric method is confined to the diamond alone.

Now, if we could consider the problem from the point of view of linkages, and consequently of molecular rigidity or resistance to chemical reactions, we should have a general method of attack based on and supported by the chemical and physical properties of the allotropic forms of carbon.

Keeping in mind the proposed fundamental assumptions, let us follow the possibilities for such a representation.

The first class is noted by the *power of free rotation* of the units (single atoms or groups) constituting the carbon molecule. (See fig. 5, Class I.)

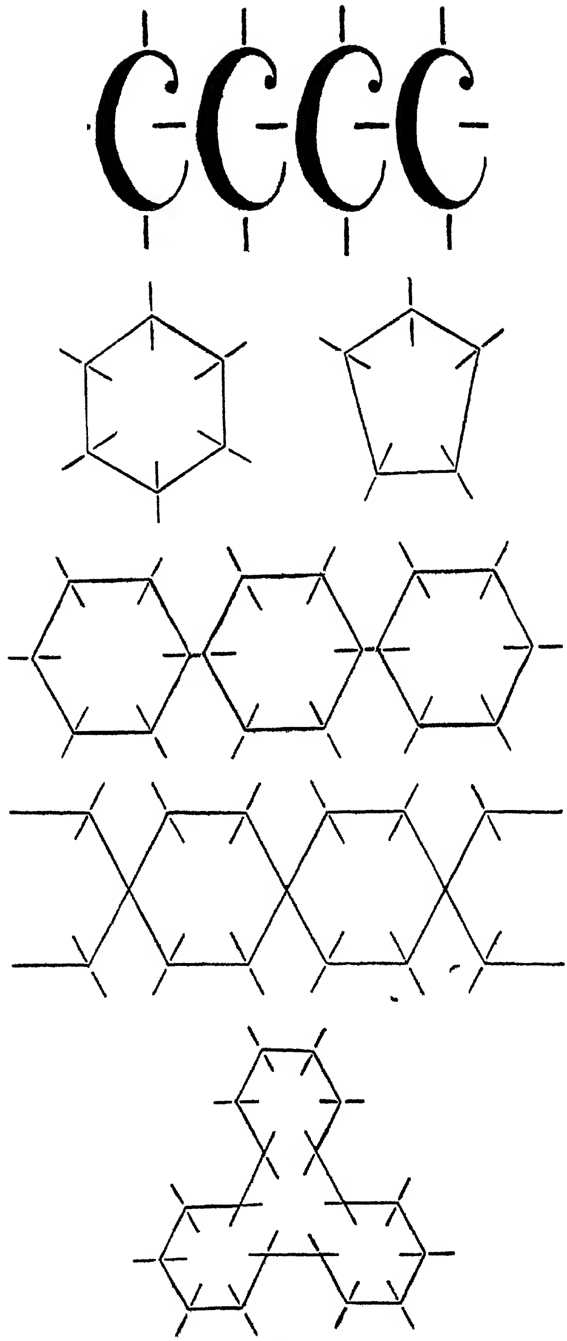

Fig. 5.

The second class is noted by the *partial rigidity* of the molecule, owing to the two single bonds linking some units. (See fig. 6, Class II.)

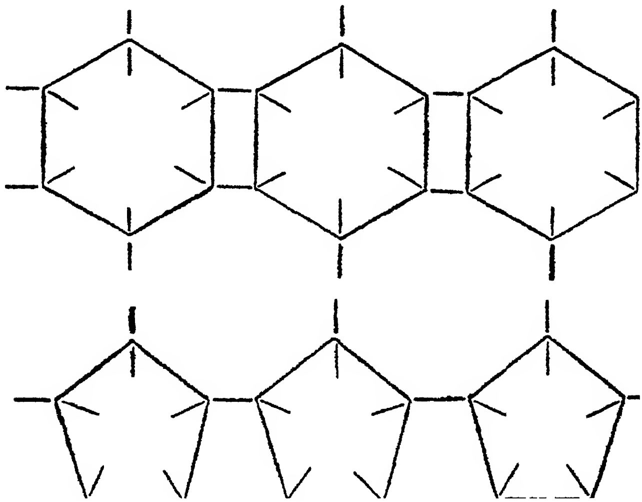

FIG. 6.

The third class is noted by the *complete rigidity* of the molecule. (See fig. 7, Class III.)

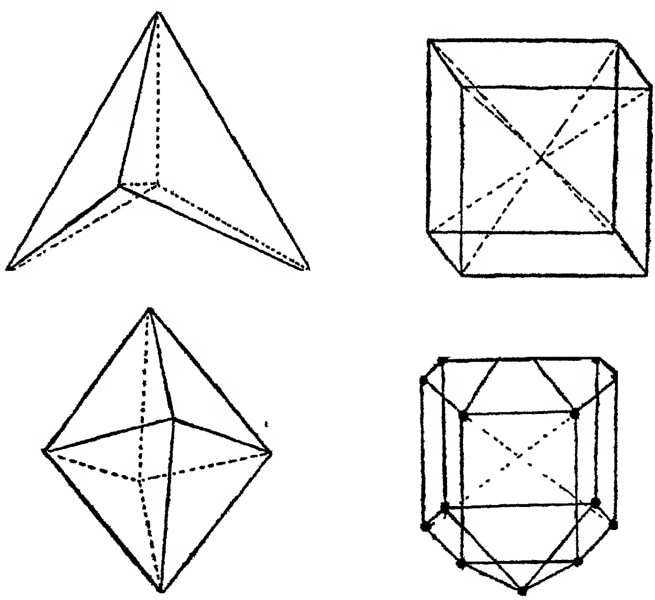

FIG. 7.

Here we have before us the striking fact that any possible formula for carbon will fall into one of these three classes. These three distinct classes are remarkable from the fact that they would account for the three modifications of carbon, and suggest the possible formulæ for amorphous carbon, graphite, and diamond.

The following considerations may serve as a pioneer attempt to assign to each modification its constitutional formula.

The calorimetric measurements of the heat of complete combustion of carbon per gram-atom give the following numbers (Berthelot and Petit, *A. Chim.* [6] **18**, 89–98, 1889).

Amorphous Carbon	97·650 Cals.
Graphite	94·810 „
Diamond	94·310 „

These calorimetric measurements indicate the sum-total of the energy liberated during the formation and degradation of the complexes, plus that of the oxidation of CO to CO_2.

Taking equal weights of amorphous carbon, graphite and diamond, and subjecting them to complete combustion, we find that the amount of heat evolved is different for each form of carbon, although the number of atoms taken and the number of CO_2 molecules formed is identical.

Looking for the cause of this dissimilarity, we are driven to attribute it to the varying stability of the molecules in the three cases, which must depend upon the mode of linkage of the units inside the molecule.

Returning to our table of classification, we expect that the least stability will be shown by molecules whose units have the power of free rotation; the maximum stability will be found in the molecule all the constituent units of which are in a state of rigidity, the intermediate case being a molecule having some units which are rigid and some free. Now, considering the fact that the greater the stability the smaller will be the evolution of heat on complete combustion (compare the case of phosphorus), and correlating this with the calorimetric measurements quoted above, we find that—

Amorphous carbon is represented by Class I., where *none* of the units are *rigid*;

Graphite, Class II., *some* units are *rigid*; and

Diamond, Class III., *all* units are *rigid*.

It must be noted that the calorimetric measurements of the heat of combustion of carbon differ considerably with every experimenter. (Favre and Silberman, *A. Chim.* [3] **35**, 357, 1852; Berthelot and Petit, *A. Chim.* [6] **18**, 88–98, 1889; Mixter, *Amer. J. of Science*, [4] **19**, 440, 1905; Roth and Wallasch, *Berichte*, **46**, 896, 1913.)

It would appear to be very probable, on the basis of the theory of the constitution of the three types of carbon advanced above, that the values of any selected physical property should show a regular gradation from amorphous carbon to the diamond, the value for graphite lying between the values for the two other forms. If we find that such a gradation appears when a number of physical properties are taken, we can say that this behaviour is consistent with the hypothesis on which the present discussion is based, and it may therefore in this sense be regarded as affording evidence confirming the hypothesis.

Below are collected the values for a number of physical constants which have been determined for all three forms of carbon. The numbers are taken from Landolt-Börnstein, *Tabellen*, 4th edition, 1912.

(1) *Density*

Diamond	3·51
Graphite	2·10—2·32
Amorphous carbon:	
Gas coke	1·885
Cocoanut-charcoal	1·860—1·67
Lampblack, sugar-charcoal, and wood-charcoal	1·70—1·80

(2) *Coefficient of cubical expansion*

Diamond	0·00000375
Graphite	0·0000104
Gas coke	0·0000162

(3) *Thermal conductivity*

Diamond (0° C.)	0·33 [1]
Graphite	0·0117
Charcoal	0·000405

[1] Eucken, *Physik. Zeitschr.* **12**, 1005, 1911.

(4) *Specific heats*

(All measured by H. F. Weber, *Pogg. Ann.* **154**, 367, 553, 1875)

Diamond	(10·7° C.)	0·1128
Ceylon graphite	(10·8° C.)	0·1604
Wood charcoal	(0°—24° C.)	0·1653

(5) *Electrical conductivity*

Diamond	(15° C.)	$0{\cdot}211 \cdot 10^{-14}$—$0{\cdot}309 \cdot 10^{-13}$
Graphite	(15° C.)	$0{\cdot}082 \cdot 10^{4}$
Charcoal	(12° C.)	0·25

An inspection of this table shows that, with the single exception of the electrical conductivity, all the physical properties are in the order we should expect. In the case of the electrical conductivity, graphite occupies an anomalous position, in that its conductivity is very much larger than that of either the diamond or amorphous carbon. In this connection two facts appear to be important. Firstly, the value of the electric conductivity of graphite is so very much larger than those of the other two forms that it would lead one to suspect that it is occasioned by something quite different from the mode of linkage of the carbon atoms in the different forms of carbon, since we could scarcely expect that the differences in atomic linkage would change a substance which is an insulator, like the diamond, into a substance which is a metallic conductor, like graphite. And, secondly, we can see that there is a real anomaly in the case of graphite, because the thermal conductivities are in the order expected, and thence, from the law of Wiedemann and Franz, which states that the ratio of the thermal and electrical conductivities is (at least in the case of metals[1]) approximately constant, we should also expect the electrical conductivities to be in the same order. The abnormal electrical conductivity of graphite is, in fact, a property which still awaits explanation. Clearing up the ground so far as we can as regards the structure of carbon molecules, we shall proceed with the consideration of the process of oxidation and the complexes of the general formula—C_xO_y—formed during that operation.

The whole process of combustion or oxidation can be represented in the following manner:

$$2C_x + YO_2 \xrightarrow{\text{I}} 2C_xO_y \xrightarrow{\text{II}} aCO + (2x - a)CO_2 \overset{\text{III}}{\gtrless} 2xCO_2$$

[1] Cf. L. Cellier, *Wied. Ann.* **61**, 511, 1897.

This scheme of the reaction in its complete form illustrates in a clear manner not only the various stages of the process of oxidation, but also the partial character of the theories advanced up to now with the object of clearing up this important problem.

The stages I and II of the reaction are represented by the theory of Rhead and Wheeler, stage III is the part of reaction elucidated by Baker and Dixon; stage III being reversible in character (above 600° C. under certain conditions) affords Lang's theory, too, the possibility of finding a place in the series of doctrines. Combining these apparently contradictory theories we attain a truer aspect upon the phenomenon of combustion.

In the process of gradual oxidation C_xO_y may be regarded as a physico-chemical variable, depending upon the allotropic form and extent of oxidation, x being always equal to the number of atoms in a carbon molecule, unless sub-complexes are also formed,[1] when the number of atoms in a carbon molecule will be a multiple of x.

Although the number of intermediate theoretically possible complexes is very large, still, judging by the actual ultimate products, the final products may be:

$$C_xO_y = \underset{\text{only CO formed;}}{\underset{\downarrow}{C_nO_n}}, \qquad \underset{\text{CO and } CO_2 \text{ formed;}}{\underset{\downarrow}{C_nO_{n+r}}} \qquad \text{or} \qquad \underset{\text{only } CO_2 \text{ formed}}{\underset{\downarrow}{C_nO_{2n}}}$$

where n is the number of atoms in a carbon molecule, and r the number of carbon atoms forming CO_2 molecules. Therefore the general formula for the final complex will be:

$$C_xO_y = C_nO_{n+a}$$

where a is a variable depending upon temperature, pressure or concentration of oxygen, as well as the particular form of carbon, and varying between zero and n.

[1] The lower oxides obtained by Brodie (C_4O_3), Berthelot (C_8O_2) (*Ann. Chim. Phys.* 1906, IX. 173), Diels and Wolf (C_3O_2) (*Ber.* 1906, **39**, 689), and several others have all been prepared in quite an indirect manner; whether they have a momentary existence as sub-complexes in the process of combustion or not is so far a matter of conjecture.

In case of the diamond, which has a rigid configuration, it is quite possible that it undergoes on heating firstly an allotropic transformation and only secondly the new modification forms the complex.

Generalisations

I. Polyatomic molecules combining with one another and giving finally simple molecules must either disintegrate before the reaction (under the influence of temperature, pressure, etc.), or form a complex or complexes, stable or unstable, as the case may be.

II. Polyatomic molecules of elements may be represented by constitutional formulæ in a manner similar to that adopted in case of molecules of compounds.

In conclusion, I wish to express my indebtedness to Mr. R. Partington, M.Sc., for the help given in connection with the collection of the physical data, and Mr. H. Stephen, M.Sc., for the diagrams.

A BIT OF ROCK

By GEORGE WILLIAM BETTANY

A bit of rock! I break it from the cliff
Where once the marl was quarried. Shall I fling
With careless hand th' unvalued fragment down
Into the pool below?—A piece of clay?
A sandstone? Or a limestone? Or a marl?—
I'll ask it with an acid what it is.
So for a space the sentient mind decrees
The fate of matter, till the time shall come
When senseless matter seals the fate of mind.
Ah! lauded Mind, how much art thou deceiv'd!
This rocky shard bore on its weather'd face
A gem of which I knew not; else had I
A better casket found wherein to lay
Its beauty softly. So I reach my home
Convinc'd I bring—a bit of rock: no more.
And now, behold, my hammer breaks it down
Almost to powder; and an acid now,
Of greater force to cleave the cloven dust
Than any force mechanic, eats its way
Where sight may never follow, rends apart
The long-lock'd atoms, slits the bond that ties
A spirit in the rock, whose egress now
The thinking mind with wonder may behold.
  At length the stone has yielded me its name
And all its secrets. Stay—not all. I fain
Would arm my sight with microscopic power
To view the structure of th' unalter'd rock
Wherein the spirit houses. So I search
Among the remnants which the hammer spared,
And take the largest piece, a slab whose side
Outmeasures not a barleycorn. With care
I set it on the stage and throw a light
Athwart its mass, and lower down the lens.

And now am I transported, and this stone
Is now a stone no longer, but a world
Of many millions whose extent the eye
Fails to embrace, e'en as the teeming hordes
Of living things that move upon the earth
Shortsightedly, like mites upon its crust,
See not its circling orb. That which had bounds
Is boundless now to microscopic sight:
That which the eye was powerless to discern
Is now a landscape, such as might belong
To fairyland itself. The fracturing blow
Hath hewn a chasm here, and there hath left
A precipice all sheer. I see below
Valleys and hills, ravines and lofty peaks,
And on the utmost verge a wilderness
Outreaches sight. That limit is a spur
Which pricks me to o'erleap it and neglect
The kingdom that I have for one whose bound
Must baffle e'en as this—an endless quest
T' o'er-reach the infinite. What lies beyond
For ever tempts our finite phantasy,
Lures it unceasing onward till at last
We struggle helpless 'gainst that awful bourn
Which hides a nameless Power. So now my mind
Pursues the hidden rather than enjoy
What is reveal'd, still conscious of a way
Into a region vast and unexplored
Which may be trod—still conscious of its power,
In kind the same as that wherewith proud man
Hath weigh'd the moon, made camphor out of dust
That ne'er knew life, and for his use hath tamed
The spirit of the lightning. "Use thy power,
O Man, with mercy. Take this bit of rock,
As thou wouldst have a greater power than thine
Take up the earth, with thee and all upon it,
To turn it o'er and view its nether pole.
In power thou art a god; in wit, a fool.
There is thy kingdom, *there* thy world; but thou,
Thou know'st not what thou rulest!" So might I
Justly have been rebuked; but, still untaught,
I take a needle, clumsily o'erturn

This all too delicate speck of earthen mould,
And scan its other face with that same power
Which makes me smaller. Now am I become
Once more a speck myself, and that which was
To me a speck again becomes a world
Wherein I wander worshipping and lost—
A region unsurvey'd, unmapp'd, unknown
Till I, the king on't, with a pigmy rule
Measure its hills and valleys, and essay
What leagues it stretches here against the North,
Or to the South as here. See yonder peak
That seems to burn with fire, its crystal walls
Inflamed with light, its top all tipp'd with gold,
And, half-way down, those caverns in its side
Gleaming with red. 'Tis sunrise on the hills,
While in the vales the darkness sleepeth still.
Nor can I cease to marvel though I know
The sun is but a lamp which I have placed
A foot away; nay, though I know its beam
Is under my control—to wax, to wane,
To rise, to set, to throw a shadow far
As in the Arctic, or with tropic aim
To burn o'erhead and bring the shade to nought.
See now, the shadows move. The morning grows
In yonder pass, revealing by their gleam
Huge slabs of mica in the sandstone rocks:
'Tis light now in the valleys: here 'tis noon,
And here I rest awhile to scan the work
Of ancient rivers: these have brought the lime
Down from the mountains in a bygone age
To fill their channels: veins of limestone now
In sinuous paths meander through the rocks,
And seem, and are, but rivers turn'd to stone:
Changeless as death they look; eternal calm
Seems in their features fix'd; and now, forsooth,
I see a world all moveless, silent, dead.
And yet this fear, this sense of time and space
Greater than I, is in my mind and me;
For do but make me larger, so that I,
In semblance like a god, may drown this world
Within a test-tube, pouring on these rocks

An acid deluge, then, in time far less
Than I have ta'en to contemplate with awe
These limestone rivers, lo, they are dissolv'd
And all their beds are empty. Such as these
It was within whose parts my acid work'd
And chased the spirit forth, and I am here
To see what form corporeal it took
Link'd with the lime as *calcium carbonate*;
Nay, more: to see what 'twas that did defy
The acid's power—these adamantine rocks
Where now the drowsy noon with static gleam
In crystal chambers sleeps; these banks of quartz
Like granite tombs unchanging, while the dead
That sleep within—these fleeting ribs of lime
That rose and fell with life—now moulder and decay.
So ends my search. The business of the world
In which I live bids me take leave of this.
I come. The shadows lengthen as I move
This radiant light, and still mine eye pursues
New hills and dales illumin'd. Pity 'tis
To leave so fair a world; and yet 'tis dead;
An utter solitude. At yonder ridge
Up which the light is creeping will I stay
An else unending quest. Already now
Yon peak has caught the last red gleam of day.
Farewell! But what is that? That streak of green
Beyond the dark? 'Tis lost now: now again
It cometh with the light. What can it be?
The distance will not answer. What! Shall I
The king here be denied? Come hither now
All ye my powers and ministers. Go, say
To yonder hill: "The king is on his throne,
And bids you come to him." And now, behold,
The vales and hills glide past me, and that ridge
That hath defied my power is brought along
A captive to my presence. On its face
I look as one who questions, but the ridge
Returns my gaze as one who answers not.
I scan its features vainly till I see,
Not that bright emerald which far away
Flash'd a green signal through the parting gleam

Of dying day; but this, yea, even this,
My impotence as king. The great grim rock
Keeps well its secret; or, if nothing hides
Under its dark and threat'ning brow of stone,
Then have I been deceiv'd, and by myself.
"Come down, then, from thy throne, O Feeble One;
If thou wouldst learn of Nature, take thy place
Humbly with those who serve her. Thou shalt learn
The patient ways of science, if thou'rt wise."
Forthwith am I dethroned, and now submit
Myself to judgment, summoning myself
Before that inquisition over which
The mind sits president. That court hath bid
This backward movement of the country-side,
Moorland and fell and all that lies between
The tors of Glenith and the rock-strewn gorge
Flanking the ridge of Seul—a spacious tract
Between whose bounds a thousand micra stretch.
'Tis done. The movement ceases. Here again
Below me lie the very veins of lime
I scann'd with wonder—Was it yesterday,
Or but a moment gone? And now once more
The shadows deepen, and I fix my gaze
Out in the West where yonder mountain spur
Runs darkly to the coomb of Meini Du.
Now at the foot the ridge begins to glow,
And all too swift the critic moment comes
When fact and fancy shall be put to proof.
The signal! There! Before the light hath crept
Far up the ridge! 'Tis gone. The summit burns
Resplendent all in gold. Yet further on
Flashes the green no longer as before.
Back with the light! Stay! Stay! What seest thou now?
—A silver bow, a shimmer in a pearl,
A crescent moon, a copper mine, a scarf
Dropp'd by Diana when she stoop'd to kiss
Endymion asleep, a phantom else
Within the mind, the body of a hope,
A fear, a dream, a memory revived
As by the touch of lips that bring again
The golden days of youth, a mystery

In which we cannot and we dare not rest.—
Festina lente. Note with care its place
And take its bearings. I will send a scout
With optic glass to view it from the side
Hence inaccessible. Since last I saw
And mark'd this twilight spectre of the hills,
'T hath overleapt a valley; or, perhaps,
This solid world that seemeth now to rest
Upon eternity hath had a shake,
Touch'd by a monster such as men believ'd
Lay under Etna when with wave on wave
Sicilia trembled. But here comes our scout.
He hath an eye so vast, this mountain spur,
Loos'd by a needle, floated on my breath,
Might all be lost under his smarting lids—
A piece of grit to draw a flood of tears
And tax a surgeon's skill. Distraught with pain,
Then would he know how huge a thing it was
That rack'd his eyeball. Nor can he be spared
Such bitter ways to truth, though he hath learn'd,
With pleasure and with wonder in his soul,
The way into the heart of little things
Is infinite as that which leadeth forth
Beyond the faintest star. His monstrous eye,
Arm'd with a lens, invisible to all
Who may be living in this fairy world,
At distant range hath swept the ridge of Seul
On every side accessible to learn
What yonder portent means. "Barren and cold,
A silent waste the Seulian ridge appears
Seen from Estelle," (so runneth his report)
"And from the further side, from Corin's heights
Which from the West command it, nought I saw
That can this mystery solve, unless, perchance,
A twisted spire uprising from the depths
Of Cwm y Llan means more than what appears.
I cross'd a gorge upon a shaft of light
Which carried me a millimetre o'er
Its dark abyss; and on the ledge beyond,
Creeping along a precipice until
That spiral monument lay sheer below

Under the jutting crags, I then advanc'd
Mine armour'd eye over the giddy edge
Of that unsounded gulf and peer'd below.
It was as though I saw a dream in stone
Built on the viewless air: not one, but three
Cloud-pointing spires bathed in a sea of light
Whose waves in dazzling whiteness backward roll'd
From off their sloping sides, while deep below
The darkness closed upon them and denied
Me further sight. Such harmony of form
Dwelt in those stony points that pierc'd the mists,
They seemed indeed the topmost pinnacles
Of some cathedral in whose cloister'd aisles
Men ponder on the infinite, and think
Of things beyond the clamour of an hour,
A day, a lifetime. This was all I saw.
Too big was I, too gross for such designs.
The task was one for delicate Ariel,
Secure upon a thread of gossamer,
To leap into the hollow and explore
What might be hid below."
It is enough,
Sir Scout: the place agrees. Those mystic cones
Are under our horizon, and they point
Not upward to our zenith, but between
Our *up* and *down*. The world hath many sides:
We see not all. And what are *up* and *down*
To him whose eye beholds the tiny earth
Suck'd in the whirl which draws the sun to join
The streaming stars, and flings both stars and us
To swirl in such a maelstrom? Oftentimes,
Like minnows in a creek, we judge the stream
When living in an eddy, quarrelling
Among ourselves which way the current leads,
Our reason hinder'd by the words we use
To give our thoughts a body, when we take
Symbols for things, and when, perchance, to things
Unknowable we rashly give a name.
But to our task: how much remains to do!
Reflection now hath held her high consult
And calls for Action—Action who is e'er

Impatient of her rule, and waiteth still
Her slow commands. She gives him measurements,
True to the thickness of the golden dust
Which studs the wings of summer butterflies,
That he may know with mathematic truth
Exactly where those tapering summits lie.
Then bids she him so tilt this bit of rock,
So lift it up and fix it, that her eye,
Centred upon this microscopic field,
May see those towering spirals and discern
What underlies them. All too eagerly,
Action obeys, and levers up the world.
I watch it through my glass. The mountains shake,
Uprear, o'erturn, till sight itself is sick,
And with an awful plunge they disappear
And leave me gazing on vacuity.
Now rack and pinion work, and I descend,
As in a mine, when the explosion's o'er,
To see the havoc done. Those fragile spires
Nor greet me all unharm'd, nor shatter'd lie
Among the rocks. Ill hath the work been done.
Pois'd on an edge to bring those spires erect,
The whole hath toppled o'er. The tiny world
Appears intractable. Yet Action, ever quick,
Dauntless, unteachable, hath seiz'd the stone
Between his horrid finger and his thumb,
E'er pale Reflection, paralysed, can speak.
In such a grasp Mount Everest would melt
And crumble into powder; Fate might spare
The marble beauty of the Taj Mahal,
But Art would weep that such a hand should fling
Even its shadow on it. But 'tis done.
And now, at last, the rock is duly set
At the right angle, on a base secure
Which that intrepid engineer hath built
Out of the ribs of pine-trees, such as lay
Shaped ready to his hand, common as clay,
Yet such as gods might envy, sulphur-tipp'd
By magic alchemy to carry fire,
The gift of science, for the needs of man.
The microscope invites, and yet I pause;

For Hope is slow to put her fears to proof
When truth might rob her of the sure delight
In which she lives. Enough! The fear, the hope
Is vanity itself. For what hath hope
To do with such as this, this bit of rock
Which yesterday was weathering in the cliff
Spurn'd by the climber's foot, and soon must make
Its grave upon a dust-heap? Yet my mind
Is curious: a peep will show me all.
'Tis not in focus: now it comes to view.
  O Living Gem! O Loveliness of Life!
Go, call the children; bid them come and see.
O Glory of the wild! O living proof
Of unperceivèd worlds! O rare delight!
Granny must come; bid granny come as well.
Nigh eighty years! yet her experienc'd eye
Hath seen no sight like this.
                                        Now one and all
Come, look, and question; one and all depart,
Amused or thoughtful, each according to
The riches of his mind; for 'tis the mind,
And not the eye, determines what we see.

  Alone! O charming solitude! Alone,
But with my love, sweet Nature, sweetest peace!
Here to thy bosom thou enfoldest me
Who am thy child. O Mother, thou art all
My refuge and my strength when the rough winds
Of calumny assail and human hearts
Are less than human. Pity me, that I
Among thy many children, one by one,
Lose all I love but thee. So I return,
Dear Mother, to thine arms, again a child.
Thy face is kind; thy heart is merciful
E'en when the lightning threatens, even when,
Germs thick'ning in the blood, I say good-bye
And the last darkness falls. Thou sparest not;
Thou sparest not thine own. Thy stern reproof
Is always just, because thou lovest all
And hatest none. The little and the great
Are one with thee. The star-clouds—they that float

In the great ocean of immensity,
Whose light, now reaching me across a space
Too vast for thought, commenc'd its viewless flight
Ere I had life, swift coursing through the years,
Through ancient time, through space abysmal, till
It cometh now where I (and what am I
In such a giddy dance of burning suns
And flaming systems?) stand upon the earth,
And see the Past while living in the Now—
The star-clouds! yea, and e'en this child of light,
That like a star adorns this bit of rock,
Are one with thee, great Mother of us all.
The dewdrop and the sun have giv'n it life
In shady hollows where the dewdrop lies
Shy of his secret visits. Yet, 'tis thine.
'Tis thine as I am thine. It shares thy life,
This little thing that folds its tiny leaves
Against the drought of summer, and outspreads
Its beauty to the light when the chill wind
Distils the rain, or studs its leaves with frost.
'Tis anchor'd on a crystal. It is held
By crimson cables. Sheathing leaves arise
And hide each other's beauty, and I see
Not all I would. The cells are hidden here;
But here, in checkered rows within a leaf,
With splendour shine a hundred tiny spheres
Dyed with the rainbow's green. 'Tis very young.
Its love-days are not yet. Its leaves are five;
Five only, as I count their threadlike tips
One o'er the other rising. He shall know,
Who named it a cathedral, I have seen
And stay'd to worship, where the infinite
Goes inward till I stagger at the thought
Of matter still divisible within
The atom's bound. This little plant is one;
One only; all alone; no fellow near.
Ten thousand millions mass'd upon a rock,
Close set as in a forest, might attract
The traveller's eye; and then might he remark
"The rocks are green," and with a giant's stride
Incurious pass on. What mortal eye can see

One only of those evanescent spheres
Which float in air and scarcely have a name
Till myriads make the mist? Who can discern
The colour of a raindrop, till the one
Is lost within the many, and the hue
Strikes the dull eye when rapture thrills the soul
Beside a mountain tarn, or when we gaze
Out on the ocean's blue? See, at a stroke,
What fills the painter's canvas when he takes
A flood of colour on his cunning brush
And paints a purple mountain. Can he see
Gems such as this which stud the distant rocks
And give them half their beauty? Can he count
The florets of the heather, as he sweeps
Their colour on his board? O little thing,
Thou tender moss-plant breathing on this stone,
There's majesty in thee. But thou art like
Those kings among a people, unperceiv'd,
Unreverenc'd, uncrown'd, save by a band
Of bleeding thorns, till after years have shown
The little may be great. So when I see
Thy loneliness, fair plantlet, I am touch'd
By that which doth belong to truest kings,
Whose majesty is in their solitude
And strength to stand alone. So when I see
Thy beauty unregarded, desolate,
Unsought, untreasur'd, like humility,
Which men pretend to hold in high esteem
Yet spurn because defenceless, I am touch'd
By voices from the infinite which ask
That ears have hearing, and that eyes may see.

THE RÔLE OF REDUCTASE IN TISSUE RESPIRATION

BY

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IT has been known for a long time that living tissues possess both reducing and oxidising powers. In 1883 Hoppe-Seyler drew attention to the strong reducing processes in living tissues; and Professor Theobald Smith has recently used liver juice as the best agent with which to close the open end of a tube where bacteria were to grow under anaërobic conditions. Ever since the time of Lavoisier it has been certainly known that the carbon dioxide and water eliminated from the animal body have been produced by the oxidation of carbon and hydrogen within it. It was originally held that this "carbonaceous" oxidation took place in the blood itself; but the undoubted production of carbon dioxide by a frog whose blood had been replaced by salt solution showed that at least that gas must have originated in the tissues and not in the blood. Oxidation and reduction evidently go on side by side within the living tissues; oxygen they must have, and they soon die if it is withheld. The source of this oxygen is of course the respiratory pigment, oxyhæmoglobin, whose loosely held oxygen is removed by the tissues, which are, therefore, said to have oxygen-avidity (Sauerstoff-Bedürfniss) or reducing power. This continual oxidation of materials within the tissues and the reduction of the oxyhæmoglobin in the circumambient blood is conveniently called tissue respiration. Within the last few years attempts have been made to gain a clearer insight into both the processes of tissue oxidation and tissue reduction, with the result that both are now thought of as carried out by intracellular ferments. Many workers on the Continent of Europe and in England have studied the action

of what have been called oxidases, ferments believed to be concerned in effecting oxidations of a large number of substances inside the living cells. These workers in fact have studied the expiratory phase of tissue respiration, the exact nature of the oxidative process leading to the final formation of carbon dioxide and water with the production of correlated intermediate substances.

Investigators of oxidases are continually meeting with evidences of substances in the living tissues which appear to be working in the direction opposite to that of oxidation. One particular oxidase has the power of oxidising α-naphthol and di-methyl-paraphenylene-diamine to indo-phenol-blue, and has been studied by Vernon, who calls it "indophenol-oxidase." Studying the quantitative estimation of this oxidase, Vernon encountered "the unavoidable presence of reducing substances some of which are possibly enzymes or reductases which act in direct antagonism to the oxidases and, under certain conditions, entirely overpower them. Hence the absence of an oxidising action cannot be held to indicate the absence of oxidase unless the conditions are so chosen to give the oxidase the best possible chance of exerting its activity." Now it is just these reducing agents which, on the other hand, we have been studying for some years past. In 1885 Paul Ehrlich published an elaborate research into the reducing power of living organs whereby they were able to reduce indo-phenol-blue to the leuco-compound and alizarine blue to alizarine white. The pigments were injected subcutaneously into living animals. Ehrlich found that almost all organs examined reduced one or other of these pigments, some organs with great energy, such as liver, fat, and the gastric mucous membrane. He recognised that even when he could not detect reduced pigment, that did not prove that there had been no reduction, but only that oxidation had been quantitatively greater. The title of Ehrlich's paper was "The Oxygen-avidity of the Organism," for he recognised that it was in virtue of the avidity for oxygen on the part of the tissues that they were also able to reduce certain pigments to the colourless or chromogenic condition. In other words, the oxygen-avidity is one expression of reducing power. Ehrlich made no suggestion that this power was due to a ferment.

In 1896 one of us (D.F.H.) noticed that when an animal, still alive though chloroformed, had been injected with the mixture

of gelatine and soluble Prussian blue (so much used by histologists for demonstrating microscopic blood-vessels), and had been opened up immediately, such an organ as the liver, instead of being blue, was colourless. On cutting up the liver and exposing the portions to the air, the blue colour was observed to be restored until one could see minute vessels which a moment before were quite invisible. The restoration of blue colour was very rapidly brought about by pouring hydrogen peroxide over the colourless surfaces. The bleaching of the soluble Prussian blue in the gelatine injection mass was attributed to *reduction* of the blue potassium ferri-ferrocyanide to the colourless dipotassium ferro-ferrocyanide; in terms of physical chemistry, to the removal of a positive ionic charge of electricity from the trivalent ferric-ion. Similar results were obtained on injecting the vascular system of the surviving kidney. It then occurred to us that if the kidney could reduce a pigment which was still in its vessels, the organ, if injected under sufficient pressure, might be constrained to excrete an artificial urine through the normal channel of the ureter. We found it possible to effect this. On injecting into the artery of a sheep's kidney the warm soluble Prussian blue and gelatine mixture, we obtained from the cannulated ureter a few drops of an absolutely colourless substance—an artificial urine—which on being treated with hydrogen peroxide at once became blue. The kidney had, then, excreted some gelatine and reduced soluble Prussian blue, proving that these substances had travelled from the blood capillaries to the ureter and in their passing had been reduced by the still living renal epithelium. But after a time we noticed that the outflow from the ureter had become blue, the kidney cells had become poisoned and so no longer able to carry on their vital reduction. It is not to be supposed that the living tissues can withstand for more than a certain time treatment with substances which cannot be other than ultimately toxic for them. In later experiments ferric chloride was used with both the liver and kidney in order to determine whether a substance devoid of oxygen could be reduced to the lower form—ferrous chloride—on being perfused through surviving organs. From the kidney we obtained an artificial urine which contained ferrous chloride, and some ferrous chloride was present in the liquid which emerged from the renal vein. Similar results were got with the liver; from its bile duct ferrous chloride was drawn

off (artificial bile), and in the fluid from the hepatic vein some ferrous chloride was present.

The activities of tissues may be studied in other ways than injecting chemical substances into their vessels; for instance, the organs may be crushed in a juice-press until thoroughly disintegrated, and the resulting juice mixed with some pigment or other substance, the reduction of which is expected. By this means the active reducing material is brought into a contact with the reducible material which is very much more intimate than when, for instance, masses of the organ are merely immersed in the reducible solutions. By this technique press-juices of liver and kidney of cat, sheep, rabbit, horse, and frog are able to reduce methylene blue to methylene white, sodium indigo-disulphonate to the colourless chromogen and sodium nitrate to sodium nitrite. It was found also that, *e.g.* liver juice could reduce the pigment methæmoglobin first to the stage of oxy-hæmoglobin and later to that of fully reduced hæmoglobin. A boiled control of these juices had no reducing power whatever.

Two French workers, Abelous and Gerard, as long ago as 1899 had suggested that these reducing powers of tissues might be due to the presence of a ferment to which they gave the rather barbarous name "reductase." We shall later give reasons for suggesting a more specific term for this tissue ferment.

The work of one of us (D.F.H.) carried out in 1909–10 was undertaken with a view to determine what was the evidence for the existence of a reducing enzyme in tissue press-juice. The results then obtained, taken in conjunction with others arrived at more recently, have gone far to convince us that there is a tissue ferment with reducing powers. We have no evidence that this ferment differs qualitatively whether it is derived from liver, kidney, or other gland.

Some of the evidence for this conclusion may be summarised as follows:

In the first place, in a control experiment where the juice is boiled, none of the reducible substances mentioned above is reduced thereby. The temperature of boiling water, as is well known, destroys the activity of all enzymes.

In the next place, the general behaviour of the juice according as the temperature is raised or lowered is in agreement with the behaviour of acknowledged enzymes. Thus at minus 10° C. there is no reduction of soluble Prussian blue by fresh liver juice,

and it is extremely slow at zero; inhibition may be induced indefinitely by keeping the mixture of pigment and juice surrounded by a freezing mixture; on the mixture being removed to air temperature, reduction goes on as rapidly as is normal for that temperature; the ferment, therefore, has been inhibited but not destroyed. As the temperature rises, the velocity of reduction increases correspondingly; the optimum temperature is somewhere between 40 and 45° C. Like recognised enzymes, reductase has a destruction temperature which is in the neighbourhood of 70° C.

While fresh juice reduces soluble Prussian blue within a minute or so at room temperature (17° C.), its activity rapidly falls off, so that after twenty-four hours it takes some minutes longer to bleach the pigment; yet juice which takes some minutes at room temperature has its time distinctly shortened at 40° C., the blood heat. There is a decay in the activity of tissue reductase the longer the juice is kept even when it has been covered with a layer of toluene to prevent putrefaction. In a particular series of recent observations extending over a week, the following fall off in activity of reduction of liver juice was estimated. At the end of twenty-four hours the activity had fallen to over 80 per cent. of its original value, at the end of the second day to 66 per cent., at the end of the fourth day to 30 per cent., and at the end of the eighth day to about 5 per cent. The survival of hepatic reductase to the eighth day is evidently not an isolated phenomenon, for quite recently it has been found that both hepatic xanthinase and uricase are active in liver juice as late as the fifth day. We shall later see that this decrease in activity is amenable to mathematical treatment.

Since several substances are known to be able to bleach soluble Prussian blue or cause it to fade, we had to eliminate the action of such as could possibly vitiate our results. Alkalies had first to be disposed of. It is of course true that alkalies can cause rapid fading of soluble Prussian blue and certain other pigments, but none of these is present in the living tissues. When all the various inorganic salts present in the blood or lymph had been examined, it was found that none of them caused any fading of the blue beyond what a similar dilution with water would have done. No more effective were mixtures of the salts; and Ringer's solution itself produced no fading.

Reductase certainly acts like a reducer in an alkaline medium. Acid, therefore, added to the soluble Prussian blue and gelatine mixture prevents that complete reduction in the capillaries of an injected organ which occurs in its absence. Histologists recommend acetic acid being added to this particular injection mixture in order to prevent "fading by the alkaline tissues." That the inorganic salts of the blood do not reduce soluble Prussian blue is shown by the fact that when the blue and the red (of the blood) meet in the large vessels they form purple in those cases where the blood is not washed out previous to injection; but if the blue were reduced to the colourless state, the blood would be red in the large vessels, whereas it is always purple when the one pigment does not predominate over the other. It is hardly necessary to say that reduction was not due to products of putrefaction, for not only were the juices kept under toluene, but the reducing power falls off with age while the products of putrefaction must necessarily accumulate as time goes on. The next factor which had to be eliminated was the supposed reducing power of proteins (colloids); this was taken up by one of us.[1] Briefly stated his conclusions were: colloids such as gelatine and egg-white reduce soluble Prussian blue with great rapidity at 100° C., in about half an hour at 60° C., while the reduction is barely perceptible at room temperature at the end of many hours. It was shown that the protein formed a colourless compound with the pigment, and that reduction was due to the removal of a positive ionic charge. Seeing, then, that fresh liver or kidney juice at room temperature can reduce soluble Prussian blue to the leuco condition within sixty seconds, the agent operative in the case of colloidal reduction is not that which we have been investigating in tissue juices.

We think it is possible that these colloidal phenomena worked out by Creighton are the reductions which Heffter has studied. Heffter[2] holds that the so-called reductase reductions are not vital (enzymic), but are all due to the interaction of colloids and pigments. He says that crystallised egg-albumen can effect reduction. The blood-proteins certainly cannot do so either at room temperature or at 40° C.

Now, however interesting and important the study of the

[1] Creighton, H. J. M., *Nova Scotian Inst. Sci.*, **13** (2), p. 61 (1911–12).

[2] Heffter, *Archiv f. Path. und Pharm. Festschr. f. o. Schmiedeberg*, p. 253 (1908).

action of reductase on various pigments and other salts capable of reduction may be, we have to remember that none of them is even approximately the natural medium of the tissues, and most of them are distinctly toxic for the living substance. Nothing other than oxyhæmoglobin is the natural substance yielding the oxygen dealt with by tissue reductase. We have recently, therefore, made a systematic investigation into the relationships of reductase and oxyhæmoglobin in solution, a research which has brought to light many fresh data. The method used was spectroscopic: that is the change from the two-banded spectrum of oxyhæmoglobin to the one-banded spectrum of fully reduced hæmoglobin was followed with a direct-vision spectroscope. There were two advantages in this method: the first that the reductase was acting in its own proper substrate, as it were, the respiratory pigment oxy-hæmoglobin; the second that the end-point was as accurate as could be obtained in a spectroscopic method. The personal factor was practically eliminated. As the mixture of juice and diluted blood was examined from moment to moment, the two bands were seen gradually to fade away and be replaced by the single fainter band of the reduced pigment. The change of colour seen by the naked eye was observed to correspond very closely with the spectroscopic appearances. The fresh mixture of tissue juice and oxyhæmoglobin was of course pink, but as reduction proceeded it became of a duller red until finally, when fully reduced, it was of the purple or livid colour so characteristic of venous blood. Towards the end of our work we were able to say even by inspection when a specimen was fully reduced; the spectroscope almost always corroborated us. We found that the fresh liver juice (cat) in presence of an aqueous solution of oxyhæmoglobin (cat's blood diluted one in twenty-five) would completely reduce the pigment at 40° C. within five to six minutes. At room temperature (17–20° C.) the time was approximately four times as long. The acceleration in the rate of the reduction of blood pigment with rise of temperature was particularly instructive with fresh juice; the times of reduction were 36 minutes at 10°, 22 at 20°, 10 at 30°, 5 at 40°, 2·5 at 50°, and 1·75 at 55° C.

Since reductase thus can work through a large range of temperatures, we might expect that it would be found both in cold-blooded and in warm-blooded animals. This we discovered

to be the case, for we had unmistakable evidences of its activity in tissue juices both from the frog and from the fish. A specimen of liver juice from the fish, which at room temperature reduced oxyhæmoglobin in seven minutes, reduced it at 40° C. in two minutes. We found reductase, in fact, in four out of the five great groups of the Vertebrata—mammals, birds, amphibia, and fishes. The reductase from fish's liver was amongst the most energetic of any we encountered. The reason seems clear: the fish has access to so little oxygen that its organs must be able to extract it very thoroughly.

The marked acceleration of reduction at temperatures above 40° C. is in accordance with what we know as to the intensification of respiratory tissue changes in fever. The late Christian Herter, of New York, has told us that in hog-cholera the reduction processes are exaggerated.

We had no evidence that reductase was qualitatively different in the various organs of the same animal nor in the different kinds of animal examined. There is no specificity of reductase from any one source in reference to hæmoglobin from any other. Thus the cat's liver juice can reduce the blood of any other mammal, or of a bird, a frog, or a fish. The reductase of a bird can reduce the blood of a mammal, a frog, or a fish, and so on. We have called these "crossed reductions"; they prove there is no mutual specificity of relationship between the enzyme and the pigment. Incidentally they corroborate the belief that the resemblances between the hæmoglobins from various animals are more numerous than the differences. The physiological significance of the absence of any specificity between reductase and hæmoglobin is that foreign blood introduced into any animal can still be reduced by that animal, so that, for instance, human tissues can obtain oxygen from the hæmoglobin of any lower animal. The danger to the human being as regards blood-transfusion is not that the foreign blood will not be reduced, but that it may act hæmolytically toward the red corpuscles of the receiver.

The object of the tissue reduction of oxyhæmoglobin is virtually to cause the oxygen to dissociate from the pigment; so that all work on the subject of the dissociation of oxyhæmoglobin has a bearing on the present problem. Some workers have laid stress on rise of temperature as a factor in this dissociation. Where this factor is operative, it is a vastly

slower action than that of reductase. After many hours a tube of oxyhæmoglobin kept at 40° C. is still unreduced, whereas, as we have seen, certain juices at room temperature will reduce twice or more of their volume of diluted blood in two to three minutes. Temperatures above body temperature (40° C.) do not enter into the problem in healthy animals. Since oxyhæmoglobin can be slowly reduced at 10° C., and even at 0° C., we hold that reductase is the factor operative at low temperatures in the cold-blooded animals. The almost complete cessation of reduction at 0° C. and below is an interesting demonstration *in vitro* of the artificial counterpart of the cessation of tissue respiration which constitutes the condition known as "latent life."

In the next place, the presence of carbon dioxide in the blood has been proved by Barcroft to be a factor in facilitating the dissociation of oxyhæmoglobin *in vitro.* This is regarded as a most important factor in the case of cold-blooded animals. Important as this has been shown to be in laboratory experiments, we are fully convinced that it is not the chief factor in the reduction of oxyhæmoglobin even in the poikilothermic animals. The factor responsible for the reduction of oxyhæmoglobin is highly insoluble; but carbon dioxide is very soluble. Similarly, traces of acid have in laboratory experiments been demonstrated to facilitate the separation of oxygen from oxyhæmoglobin. We do not think that this either is a factor of high importance in tissue reduction. Since it is true that juice kept aseptic develops acidity in autolysis, the older the juice the more vigorously it ought to reduce if acid were an important factor, but we have shown that the exact opposite is the case. Traces of acid tend to form methæmoglobin, a pigment we have never noticed in any mixture of active tissue juice and oxyhæmoglobin. Again, the acids in question—for instance, lactic—are soluble; the reducing agent in press-juice is comparatively insoluble. It might be noted that, in dealing with liver juice and oxyhæmoglobin, we have eliminated both bile and dextrose as factors in the reduction of the pigment. It may be remarked that the so-called reducing power of colloids is exerted only against certain pigmentary substances, and not at all against oxyhæmoglobin. In other words, the "Creighton effects" have no analogues in connection with the reduction of oxyhæmoglobin; for, for one thing, it is impossible to heat blood to 100° C. without its being decomposed.

If the substance responsible for reduction in tissue juices is an enzyme, it ought to be injuriously affected by contact with poisons, substances known to destroy or retard the action of catalysts in general. A considerable number of such substances were examined by allowing fresh liver juice (cat) to remain in contact with solutions of the poison for ten minutes, and then comparing the time required by the poisoned juice to reduce oxyhæmoglobin with that required by the same quantity of unpoisoned juice. Two strengths of poison were employed, 0·1 molar and 0·01 molar, a strong and a weak respectively. All the following were investigated—formaldehyde, mercuric chloride, potassium cyanide, gold chloride, osmic acid, manganous chloride, ammonium bromide, arsenious acid, ammonium chloride, and sodium arsenite. Unfortunately, certain toxic substances could not be used at all on account of the way in which they caused the blood solution to fade when added to it; among such were acids, copper sulphate, etc. In a particular series of experiments, ten minutes was the time found to be necessary for the complete reduction of oxyhæmoglobin by unpoisoned juice, whereas the times for poisoned juice were, with the weaker solutions, as follow: arsenious acid 33′, potassium cyanide 30′, mercuric chloride and sodium arsenite 17′, gold chloride 15′, osmic acid 13′, and formaldehyde 10′. When the stronger solutions were employed, the times were lengthened; for instance, for formaldehyde 48′, potassium cyanide 34′, manganous chloride 25′, and osmic acid 19′. Ammonium chloride alone of all the substances tried had no poisonous effect at either concentration; this is in accordance with what we know of it therapeutically.

One substance highly poisonous to animals, carbon monoxide, is of particular interest spectroscopically. It is a poison because it unites so firmly with hæmoglobin that it prevents the formation in the lungs of the much less firm combination, oxygen and hæmoglobin. The pigment, therefore, carries carbon monoxide instead of oxygen to the tissues, which are, in consequence, starved of oxygen or asphyxiated. The affinity of carbon monoxide for hæmoglobin is stronger than that of carbon monoxide for the tissues. Translated into terms of our conception, the tissues cannot split off the carbon monoxide from the hæmoglobin because reductase, being an oxygen carrier and oxygen activator, has no affinity for carbon monoxide. It was,

therefore, very interesting to determine whether the carbon-monoxide-hæmoglobin, a pigment with a well-known spectrum, would remain unaltered in the presence of reductase, or whether it would be in any way changed. It remained unaltered for many hours at 40° C., showing that reductase in its state of comparative freedom in tissue juice was as powerless to break up the carbon-monoxide-hæmoglobin union as it is in intact cells.

Recently we have studied the enzymic nature of the active agent of tissue juice from the kinetic standpoint. Measurements were made to determine the value of the temperature coefficient of the activity of reductase, and also to determine the nature of the law governing the decay in the activity of the enzyme. As regards the former, we obtained the necessary data from experiments on the reduction of oxyhæmoglobin by cat's liver juice at different temperatures. To determine the temperature coefficient, the time required to reduce oxyhæmoglobin at any one temperature was divided by the time required to reduce it at a temperature 10° higher. Between 10 and 40° C. the velocity of reduction is approximately doubled for each 10° rise in temperature, so that the temperature coefficient is about 2. This discovery as to the behaviour of tissue juice with rise of temperature confirms our general contention that we are dealing with an enzyme. Above 40° it has been found that the increase in the velocity of reduction with rise in temperature rapidly falls off. Between 50 and 60° C. the temperature coefficient has been found to be 1·43. Although usually the temperature coefficients of reactions decrease slightly with increase of temperature, the decrease in the values obtained for the reduction of oxyhæmoglobin by reductase at temperatures above 40° C. is much greater than would be the case in ordinary chemical reactions. Since it is exceedingly probable that the optimum temperature of reductase lies between 40 and 46° C., the acceleration of the velocity of reduction due to increase in temperature is evidently to a certain extent counteracted by a partial inhibition or destruction of the enzyme, the result being a decrease in the value of the temperature coefficient.

Mathematical treatment of our data shows that if the decrease in activity of the enzyme be regarded as proportional to the decrease in its reducing power, then the expression

$$\frac{1}{0{\cdot}4343t} \cdot \log_{10}\left(\frac{a}{a-x}\right) = k$$

is found to hold. Considering the sources of error in our experiments, the values obtained for k are sufficiently constant to warrant our assuming that the decay in the activity of reductase follows the monomolecular or logarithmic law. In the foregoing equation, a represents the initial activity (100 per cent.) of the enzyme, and x the percentage decrease in activity at the end of time t. Determinations of the reducing power of cat's liver juice of different ages, carried out at 55, 50, and 40° C., gave the following respective mean values for k, 0·0132, 0·0134, and 0·0121.

We are now perhaps in a position to summarise the evidence, which has been accumulating to indicate that in living tissues there is a ferment for internal respiration capable of effecting chemical reduction.

1. The criterion of solubility naturally occurs to one first of all. Reductase is certainly not soluble if by "soluble" we mean capable of entering into pure water. We have, however, found that with difficulty some of it can pass from liver juice into 0·75 per cent. NaCl, and from disintegrated muscle into glycerine and saline solution; this mixture seems better than either menstruum alone. Reductase is not soluble in the sense that pepsin is soluble; it leaves its association with the cell proteins with great difficulty, nor will it dialyse away from them. The glycerine and saline extract (muscle), or "solution," did, however, reduce oxyhæmoglobin in two or three minutes, while the boiled control had no effect whatever. Neither reagent by itself has any reducing effect. This glycerine and saline muscle extract also reduced soluble Prussian blue.

All subsequent attempts to isolate the ferment have failed. Indeed, it was through finding the injurious influence of alcohol and ammonium sulphate on the ferment, used with the view of precipitating it, that we were led to study the poisonous effects of other materials. The comparative insolubility of reductase is perfectly intelligible. The rôle of the ferment is to obtain activated oxygen at the boundary of the cell; it would not serve the interests of internal respiration if reductase were able to leave the cell and circulate in the blood. It is not a secretion like the exo-enzymes pepsin, ptyalin, etc.; it is an endo-enzyme as is glycogenase but, unlike glycogenase, it is insoluble, and as such by no means alone in that class.

2. The next criterion that may be applied is the manner in

which the activity of an enzyme varies with fluctuations of the temperature. The behaviour of reductase is wholly in accordance with that of undoubted enzymes; there is inhibition but not destruction below and at 0° C. As the temperature rises there is greater and greater velocity of action, until the destructive effects of heat begin to make themselves felt. That is to say, there is an optimum and there is a destruction temperature.

3. The temperature coefficient of, approximately, 2 between 10 and 40° C. is in line with known enzymic action.

4. This is also true of the logarithmic nature of the decay in the activity of the enzyme.

5. The deterioration with age of reductase in a moist medium also conforms to the behaviour of other enzymes. It withstands complete desiccation badly.

6. The fact that poisons for catalysts similarly affect the actively reducing substance in press-juice is in favour of that substance also being an enzyme.

7. The criterion of reversibility is one which is difficult to apply to reductase. Stated baldly, the ferment does not induce any reversed action in the direction of oxidation. Press-juices reduce materials once for all, and no oxidation in virtue of the presence of reducing agents is possible. Oxyhæmoglobin once reduced is not reoxidised. But since oxidases are always present acting simultaneously with reductases, the chemical complex oxidase-reductase is functionally equivalent to a reversible ferment. In this connection one should remember that the Cannizzaro reaction—simultaneous oxidation and reduction—has actually been obtained when certain aldehydes were digested with liver tissue, thus—

$$2R.CHO + H_2O = R.CH_2.OH + R.CO.OH$$

where one molecule of the aldehyde is oxidised to the acid and the other reduced to the alcohol. Parnas has in fact suggested the term "aldehydemutase" for the hypothetical enzyme concerned.

The object of tissue respiration as distinct from tissue nutrition may be said to be twofold: to produce heat and to prepare katabolites for excretion. Thus carbohydrates are oxidised to yield carbon dioxide and water as end-products, although exactly how is not even yet thoroughly understood. As aldehydes

(aldoses) the carbohydrate chains are capable of disintegrative oxidation, and already aldehydases have been assumed.

The fatty acids are certainly oxidised in the tissues after being desaturated in the liver. The long carbon chains are broken up at the β-carbon atom at each oxidation until finally β-hydroxybutyric acid is obtained, which is ultimately oxidised in the presence of sufficient carbohydrate to carbon dioxide and water. If insufficient carbohydrate be present, then the acetone group of bodies is excreted which is the chemical abnormality in diabetes.

Amino-acids are certainly oxidised after deamination, which is itself regarded as enzymic. Amino-acids thus deaminated are ordinary fatty acids which can then undergo the progressive oxidative break-down to which these acids are liable. Lastly, bodies of the purin group undergo oxidations within the tissues, and specific enzymes are described which accomplish these.

These various oxidations in the tissues which are now regarded as enzymic are thermogenetic and are entitled to be considered as making up internal respiration on its disassimilatory side. We may, in fact, speak of tissue expiration, since the various processes have for their object the excretion of waste-products. The consensus of chemical opinion, then, is that tissue expiration is dominated by intracellular ferments, and all that we claim at present is that tissue inspiration should be so regarded. We have evidence of a very powerful intracellular ferment which brings active oxygen within the sphere of the oxidases. This is the ferment already called reductase in 1899. We think that the time has come to give it its specific name of *hæmoglobinase*, for hæmoglobin is the source of the oxygen it deals with, hæmoglobin is the substance reduced by it, hæmoglobin is the natural substrate or substance on which it normally acts.

A comparatively recent observation by Dakin[1] is of interest in this connection. "The oxidation of β-hydroxybutyric acid to aceto-acetic acid was shown by Dakin and Wakeman to be due to an enzyme which could be roughly separated from liver tissue. The action of the enzyme was not very vigorous, but was markedly increased by the presence of oxyhæmoglobin. Oxyhæmoglobin alone was entirely without action." It seems ex-

[1] Dakin, *Oxidations and Reductions in the Animal Body*. Dakin, Longmans, 1912, p. 23.

ceedingly probable that in separating, admittedly roughly, the oxidising enzyme, a certain quantity of reductase was present along with the oxidase and added hæmoglobin. The reductase would reduce the oxyhæmoglobin activating the oxygen which, under the influence of the oxidase, oxidised the acid as reported. This explanation at least accounts for the phenomenon observed and especially for the fact that the hæmoglobin *alone* had no influence.

In his recent text-book of Physiology, Professor Starling[1] writes: "There is no doubt that reducing substances are found under normal circumstances in the tissues . . . and it is possible that such reducing substances may aid in activating oxygen and in the *induction* of certain oxidative processes." The italics are ours, for this precisely expresses our belief that the chief reducing substance in fresh normal tissues is an enzyme which does originate or induce the oxidative tissue activities. This was indeed Hoppe-Seyler's original suggestion; all our work is in the direction of confirming it.

The source of active oxygen, as is well known, has been referred to intracellular peroxides, of which an inorganic example is hydrogen peroxide. Now it is remarkable that most tissue juices contain the enzyme catalase whose duty would seem to be to decompose hydrogen peroxide into water and molecular oxygen thus—

$$2H_2O_2 = 2(H_2O) + O_2.$$

But that catalase is not responsible for the reductions we have been studying we have detailed evidence. A peroxidase has been postulated as responsible for the decomposition of hydrogen peroxide which yields the active oxygen thus—

$$H_2O_2 = H_2O + O_1.$$

Whatever, then, the peroxidase is, it is virtually a reducing agent. We are not prepared to explain the relationship of reductase to peroxidase or to the hypothetical peroxides, but we prefer to conceive of the inspiratory phase of tissue respiration as the removal of oxygen from cell lymph and, therefore, ultimately from oxyhæmoglobin by the activity of a reducing endo-enzyme. We find in tissues an enzymic reducer; it reduces

[1] P. 1236.

hæmoglobin to the completely reduced condition, and since it carries oxygen from it in this way it is, in one sense, an "oxygenase." It is quite possible that the "organic peroxide plus ferment" of certain writers is none other than our hæmoglobinase, which is certainly of colloidal nature. There seems no need of retaining the term oxygenase when hæmoglobinase is a more specific term for a ferment which can do all that oxygenase is supposed to do and considerably more.

We have seen that reductase is very insoluble; the true meaning of this may be that what we call reductase is in ultimate cellular analysis the totality of certain side-chains of the living protoplasmic molecule (biogen) which possess affinities for oxygen. These, in the nature of things, cannot be disrupted from the biogen without compromising its functional integrity. This sort of thing in ultimate analysis an *endo*-enzyme proves itself to be; and Vernon has indeed remarked on the insolubility of certain oxidases.

It is true that on this view the distinction between "vital protoplasmic activities" and enzymes is obliterated, but it is quite possible that that distinction has been made too absolute. When an exo-enzyme or enzymic secretion, such as ptyalin or pepsin, can perform its function equally well in the cavity of a viscus or *in vitro*, we may be justified in maintaining the distinction between vitality and enzymic action. The secretion-enzyme was, however, part of the protoplasmic molecule before its separation. It is not the act of disrupting the side-chain that constitutes an enzyme; doubtless disrupted side-chains are our separable ferments, and, because disrupted, are more or less soluble. But non-separated side-chains can still be called ferments (endo-enzymes), which because undisrupted are "insoluble." The former—the secretion-enzymes—are destined to leave the parent protoplasm; the latter are not intended to be separated from the biogens and are, therefore, called endo-enzymes. It is not whether they act outside or inside the protoplasm that constitutes them ferments, it is their functional powers that confer the title on them. Since animal heat is genetically an intracellular affair, the ferments hæmoglobinase and the oxidases, which are concerned in its evolution, are therefore also intracellular. The former is non-specific, the latter highly specific; the former is for obtaining oxygen wherever available, the latter are specialised each for the oxidation of only one kind of substance.

But reductase is not merely a deoxidiser. Although in Nature it is concerned only with the reduction of oxyhæmoglobin, yet it is a true reducer: it can reduce substances as stable as soluble Prussian blue, alizarine blue, methylene blue, and indigo blue; it can reduce nitrates to nitrites and ferric chloride to ferrous chloride. Let us, therefore, in the light of recent work not hesitate to bring tissue respiration under the category of fermentation. Its oxidative side is now generally admitted to be endo-enzymic; let us complete the conception and regard its reducing or inspiratory aspect as also endo-enzymic.

THE HISTORY OF ADRENALIN

By S. C. BRADFORD, B.Sc.

The Science Museum, South Kensington

THE story of the discovery of the function of the suprarenal capsules, followed by the isolation of the active principle of their secretion, the determination of its structure, and its subsequent synthesis, forms one of the most fascinating chapters in the history of bio-chemistry.

The suprarenal capsules are two epithelial flattened bodies situated above the kidneys, each weighing about 4 grammes. Although known from early times, no suspicion was entertained of the important rôle which they play in the life of the individual, until experimental investigation of these glandular organs was initiated by the work of Addison published in 1855. He described a form of disease which is nearly always fatal, and is characterised by progressive anæmia, weakness of heart-beat, irritability of stomach, and general want of tone of the nervous and muscular system, together with abnormal brown patches on the skin, from which latter symptom he gave it the name "bronzed skin." As the result of extensive research Addison discovered profound alterations of various kinds in the suprarenal capsules of almost every one who had died of this disease. Following Addison's lead Brown-Séquard made a series of experiments on animals in 1856, and came to the conclusion that the suprarenal capsules were organs indispensable to life, and that their destruction was invariably followed by death. This view, however, aroused opposition, and the subject remained a matter of controversy until recently, when Brown-Séquard's results have been fully confirmed and extended.

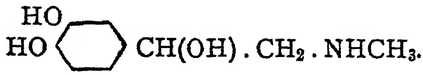
In 1891–2 Abelous and Langlois inferred that death from removal of the suprarenals is due to the accumulation in the blood of some toxic substance. It had been shown in 1890–1 by Marino-Zuco and Dutto that the suprarenal capsules normally contain a considerable amount of neurine, and that persons

suffering from Addison's disease eliminate appreciable quantities of this substance. In the following year Albanese found that a dose of 1 milligram of neurine is always fatal to a decapsulated frog, while it takes four times that amount to kill one in the normal condition. From these results Marino-Zuco and Albanese concluded that Addison's disease and the effects of artificial destruction of the suprarenals are due to neurine intoxication. The work of numerous other experimentalists leaves no doubt as to the protective antitoxic action of the suprarenal capsules, though it is uncertain whether this is effected by removal of specific toxic substances, or by the secretion and output into the blood and lymph of active substances which are directly or indirectly antitoxic.

It was proposed by Cybulski in 1895 that the effect of the compounds secreted by the capsules is to keep up the tone of the vasomotor, cardiac, and respiratory centres which are so acutely depressed by the destruction of these glands. He showed that intravenous injection of aqueous suprarenal extract raises arterial pressure, slows the pulse, and quickens respiration. These results were confirmed by Salvioli and Pezzolini in 1902. More recent work leads to similar conclusions, and as stated by Hoskins in 1915, "The sum total of available evidence seems to indicate that the essential feature of adrenal deficiency is an interference with fundamental metabolism, possibly oxidation, in which the more active tissues of the body suffer first."

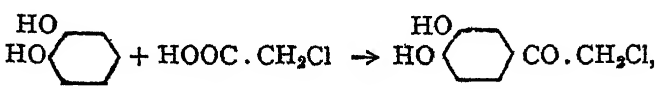
The discovery of the physiological action of the substance, or substances, secreted by the suprarenal capsules led to attempts to effect their isolation. In 1896 Fränkel obtained an impure substance which he termed "sphygmogenine," and one year later Abel and Crawford prepared "epinephrine." In 1900 v. Fürth obtained a similar body under the name of "suprarenin." By isolating "adrenalin" in the crystalline form in 1901 Takamine proved the former substances to be mixtures. Shortly afterwards Aldrich independently prepared the pure substance. Takamine proposed the formula $C_{10}H_{15}O_3N$, but Aldrich preferred $C_9H_{13}O_3N$, which was confirmed by v. Fürth and by Pauly in 1903. V. Fürth proved the presence of a benzene ring, a methyl-imido group, and three hydroxyl groups, two of which were attached to the nucleus. The next year Jowett found that, by methylation and subsequent oxidation, trimethylamine and veratric acid, $(CH_3O)_2[3, 4] . C_6H_3 . CO_2H$, were produced, proving the presence

of the complexes $NH(CH_3)$ and $C_6H_3(OH)_2C$, and establishing the structural formula:



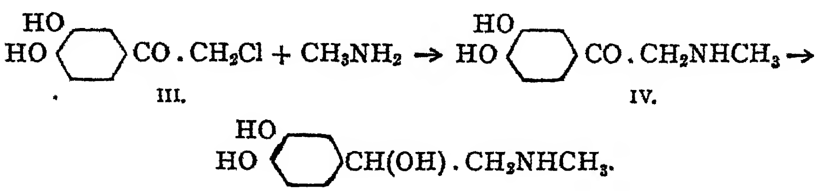
Adrenalin is soluble with difficulty in cold water, more easily in hot, and dissolves in weak acids with the formation of salts. It acts as a weak alkali to litmus. Alcohol dissolves it slightly. On account of its phenolic nature it is soluble in caustic alkalies. It is readily oxidisable, its aqueous solution rapidly becoming pink and finally brown, and is a strong reducing agent. The quantity of this substance occurring in the adrenal glands of mammals is only a few parts in a thousand. It is one of the most potent drugs. Injection in minute amounts causes an enormous increase in the blood pressure, and acts exactly as extract of the suprarenal glands. Applied externally in aqueous solution to a mucous membrane it causes contraction of the arterioles, and is consequently much used in minor surgery.

As soon as the constitution of adrenalin had been determined, efforts were made to effect its synthesis. And in 1904 Stolz showed how this might be accomplished. In his method catechol, I., is condensed with chloracetic acid, II.:



I. II. III.

the ω-chloro-*mp*-dihydroxyacetophenone, III., so produced is acted upon by methylamine, and the resulting methylamino-ketone, IV., converted by reduction with aluminium amalgam into the secondary alcohol V., which is racemic adrenalin:

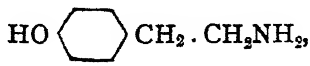


III. IV. V.

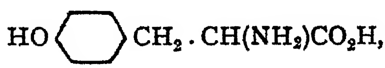
Cushny found that the substance so obtained is physiologically less active than the natural product, which is the lævorotatory form, and has twelve times the activity of its

dextro isomer. The racemic substance was resolved by Flächer by means of its acid tartarate in 1908, and *l*-adrenalin is now manufactured by this method under the name of "suprarenin" by the Farbwerke vorm. Meister, Lucius u. Brünning.

Adrenalin is related to the simpler substance *β*-*p*-hydroxyphenylethylamine,

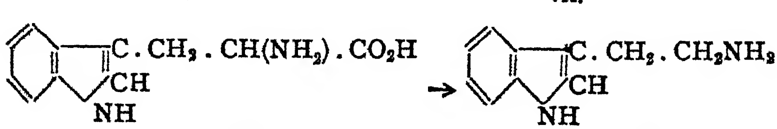


which Barger and Walpole identified in 1909 as the chief pressor principle occurring in extract of putrid meat and ergot. It is produced by fermentation of tyrosine,



one of the amino acids, which, like small magnets having positive and negative poles, form the links of which the highly complex proteins are composed. It seems probable that *β*-*p*-hydroxyphenylethylamine, formed by putrefaction in the human intestine, is the cause of the high blood pressure observed in certain persons, to which Metschnikov has suggested that arteriosclerosis in old age is due. The physiological effect of this substance is about one-twentieth that of adrenalin. The production of *β*-*p*-hydroxyphenylethylamine from tyrosine, by loss of carbon dioxide on fermentation or heating, is typical of a general method for the formation of amines from the corresponding amino acids. Thus valine, VI., gives iso-butylamine, VII.; and tryptophane, VIII., another of the decomposition products of the proteins, yields 3-*β*-aminoethylindole, IX.:

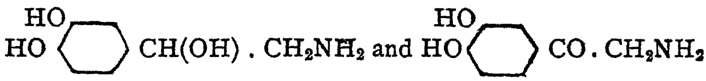
$$\underset{\text{VI.}}{(CH_3)_2CH.CH_2.CH(NH_2)CO_2H} \rightarrow \underset{\text{VII.}}{(CH_3)_2CH.CH_2CHNH_2};$$



VIII. IX.

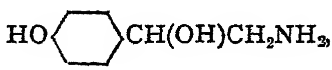
All the fatty amines from butylamine to trydekylamine have an appreciable pressor action. The introduction of a heterocyclic or aromatic nucleus causes considerable increase in physiological activity, which becomes further enhanced in those

compounds which approach closely the constitution of adrenalin. Bodies such as

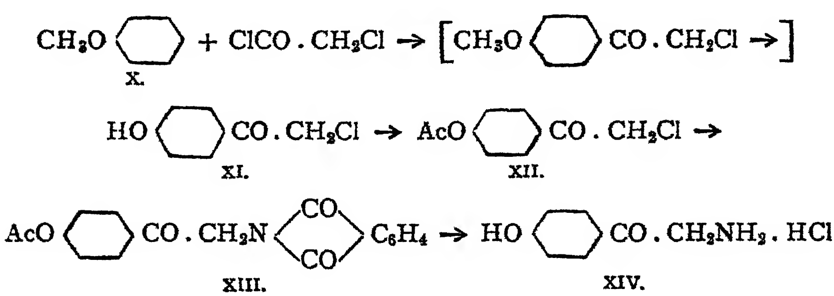


have an action comparable with that of *β*-*p*-hydroxyphenylethylamine. It is remarkable, however, that the diamines have an opposite physiological action to that of the monamines. Thus putrescine, $NH_2CH_2 . CH_2 . CH_2 . CH_2NH_2$, and cadaverine, its next higher homologue, which were the first ptomaines to be identified, cause a fall of blood pressure.

The researches in the adrenalin series undertaken by Tutin, Caton, and Hann in 1909–10 are typical of much work that has recently been carried out in this connection. Finding the methods previously employed to be inapplicable, *β*-*p*-dihydroxy-*β*-phenylethylamine



was prepared from anisole, X., which, with chloroacetylchloride in the presence of excess of aluminium chloride, yielded *ω*-chloro-*p*-hydroxyacetophenone, XI., the acetyl derivative of which, XII., was heated with potassium phthalimide, and the resulting *p*-acetoxy-*ω*-phthaliminoacetophenone, XIII., converted into the hydrochloride of *ω*-amino-*p*-hydroxyacetophenone, XIV., by hydrolysis with concentrated hydrochloric and acetic acids:



The physiological action of the free base was found by Dale to be of the same order as that of the keto-base corresponding to adrenalin, or about one-tenth that of *β*-*p*-hydroxyphenylethylamine. The secondary alcohol, above, co-ordinate with

adrenalin, was formed by reducing XIV with sodium and boiling alcohol, but could not be crystallised. Its physiological activity was found to be only about twice that of the keto form, reduction in this case not being accompanied with that enormous increase in pressor action which occurs in the case of adrenalin.

ω-Amino-*o*-hydroxyacetophenone and ω-amino-*op*-dihydroxyacetophenone were also produced. The former was found to have very slight hæmostatic action, and the latter an effect equal to that of the corresponding *p*-hydroxy compound. From which it appears that the hydroxyl group in the ortho position exerts no influence on the physiological activity of the substance.

Although it is often felt that the advance of organic chemistry is no longer rapid, such investigations as these indicate the field in which brilliant achievements may still be expected.

SOME EUGENIC ASPECTS OF WAR

By A G. THACKER, A.R.C.Sc.
Public Museum, Gloucester

WE all know only too well that the catastrophe which came upon Europe last August will be far-reaching in its effects. A stupendous sum of money utterly wasted, hundreds of thousands of men killed and several millions maimed, hundreds of thousands of women desolate and several millions of children fatherless, a serious check to social reform and to scientific research, and, worse still, a disgraceful and blighting legacy of international hatreds: assuredly the evils of the war are in no need of exaggeration. And yet there are some who hold that the conflict will bring another misfortune of a still more fundamental character. It is contended that the very racial qualities of Europe's population are being injured. It is pointed out that the healthy, energetic, strong, and courageous part of European manhood is being passed through the furnace and decimated, whilst in each country the weakly section is being carefully preserved. And it is inferred from this that the average standard of physical and mental fitness in the men of the present generation is being lowered, and that consequently the quality of the next and later generations will be inferior. This view of the matter is taken by Dr. C. W. Saleeby,[1] who has done so much, perhaps in some directions a little too much, to popularise eugenics; and similar opinions have been expressed by the American eugenist, Dr. Starr Jordan.

The purpose of the present article is to consider certain aspects of this subject which have not always been remembered. It appears to me that when the problem is subjected to a closer scrutiny, the pessimistic conclusion above mentioned is seen to be extremely dubious. The question of the biological effects of war is one of extreme complexity, and it is certainly true that the effects may be very dissimilar in different sorts of war. No doubt if those who think with Dr. Starr Jordan are right, they have found a new and stout stick wherewith to beat the

[1] See the *Contemporary Review*, March 1915.

chauvinists. But it is notorious that there are few things more mischievous than a weak argument in favour of a good cause. And as I am not advocating this particular argument against war, I may perhaps say in passing that if any pacifist should seek in this essay for the cloven hoof of militarism, he will not find it. There may be reasonable difference of opinion about the part which war has played in the past. It may perhaps be legitimately contended that warfare was once a necessary condition of human progress, although I think it would be a mistake to subscribe offhand to that fatalistic doctrine. But certainly nobody holds more strongly than the present writer that war in modern Europe is a detestable enormity. The biological effects of war are not the dominant consideration,[1] and even if war were definitely proved to be beneficial in the eugenic sense, it need hardly be said that it would not be thereby proved desirable. The case against those who apologise for war as such (which is of course quite a different thesis from supporting some particular war as a necessary evil), against the Bernhardi school of writers, is so crushing that there is no call to go in search of questionable pleas for arbitration.

There are two methods of attacking the problem: (1) we may go to history and see whether it affords us any proofs of a connection between war and national degeneracy, and (2) we may apply the laws of heredity to the known phenomena of war and draw inferences regarding the effects which ought theoretically to follow from those phenomena. Both methods have been adopted by the pessimists. I propose, however, to deal chiefly with the theoretical side of the problem, since so far as the direct historical evidence is concerned it appears impossible to add much to the remarks already made by Sir Ronald Ross.[2] It is worth mentioning, however, that in Europe, and over the greater part of the world, wars have been in the past so perpetual that if national degeneracy has occurred at all, it must almost necessarily have followed these wars. We, whose first impressions of international politics were gained in the peaceful Victorian Era, are liable to forget how exceptional a prolonged

[1] It is right to say that Dr. Saleeby also emphasises the subordinate importance of the whole discussion. Thus, although he thinks that the quality of the British population will tend to deteriorate owing to the war, the cause of the Triple Entente has no more enthusiastic supporter than he.

[2] SCIENCE PROGRESS, January 1914.

peace is in history. Consider how the Crusades affected most of Europe, and how the Hundred Years' War drained England of large numbers of its most adventurous men. These instances are instructive, because the belligerent countries were not overrun by conquerors who extirpated the weaker and less daring men, the non-combatant men; nor were all, or nearly all, the males forced into the wars. A residue composed of the less adventurous men was preserved in relative safety, a condition which does not obtain among bellicose savages, whose wars are therefore not comparable to the present European conflict.

The pessimistic writers cite the case of Rome. Rome waged great wars and Rome decayed; so likewise did Spain. But England and Prussia have also waged great wars, and it will not be contended that either England or Prussia is decadent. Prussia's wars during the last three centuries have been much more terrible than ours—the land has been ravaged again and again, by fellow-Germans, by the Russians, and by the French—but on the other hand, the case of England is especially pertinent by reason of the phenomenon already mentioned, for the life of the homeland could be carried on almost normally during nearly all our recent wars, which merely extracted some of the vigorous men. It should be said, however, that there appears to be nothing in history which warrants the contrary conclusion that peace leads to degeneracy. It is difficult to find any considerable population of European race which has been free, or almost free, from war for any lengthy period, but there are a few not very satisfactory instances. The Newfoundlanders have enjoyed a relatively long peace, and they are certainly not degenerate; again, the Icelandic immigrants into America are said to make energetic colonists. History does not help much with the problem that we have before us.

When we pass on to the theoretical side of the question, to the application of the laws of heredity to the problem, we find that although it is possible to show that the pessimistic argument is very ill founded, yet this second method of attack also fails to lead us to any very definite conclusions. And this for the best of all possible reasons: there are no generally accepted laws of heredity to apply. It is remarkable that any scientist writing for the general public on a question of this kind should not make this fundamentally important consideration clear. And yet the point is often ignored. Even

Dr. Saleeby is not quite as clear as he might be; one infers from his article in the *Contemporary Review* that he approaches the problem from the Neo-Darwinian standpoint, but he does not mention that his entire thesis is raised upon a foundation which is itself a veritable quagmire. Of course those who are convinced by the Neo-Darwinian theory of heredity do not themselves consider it a quagmire, but one would expect that they would at least make it clear that the whole principle is in dispute. In all scientific discussions, and especially in those which are carried on for the enlightenment of the general public, it is essential to maintain a sharp distinction between the peculiar tenets of any mere school of thought and the unanimous declarations of science. It may be as well, therefore, to repeat that although a considerable number of isolated facts are known with certainty, there are no known general laws of heredity; and it is this which makes many discussions on eugenics quite unprofitable.

Consider one of the most obvious instances. Either acquired characters are heritable or they are not. Most biologists now hold that they are not heritable, but some authorities—scholars thoroughly versed in the facts—still believe in the reality of use-inheritance. The matter is therefore in doubt. Now how is it possible to discuss profitably such a question as the racial effects of compulsory military service unless we previously know whether acquired characteristics are inherited or not? Some of the popular advocates of conscription (in peace-time) have urged that their scheme would improve the race, without apparently realising that they were assuming the truth of a theory of heredity which has the great weight of professional opinion against it, and even in the *Eugenics Review*[1] there is a loose reference to the subject by a writer who does not trouble to explain whether he is a Neo-Darwinian or a Neo-Lamarckian. It must be admitted, I think, that military training is beneficial to the individual, certainly physically, and in most respects mentally as well. If, therefore, acquired characters be transmissible, even in a small degree, the benefits of conscription would affect the race, would be cumulative, and the improvement in a few generations would probably be marked. But if, on the other hand, use-inheritance be a figment, then conscription would benefit each generation, but would have no direct racial

[1] T. G. Chambers, *Eugenics Review*, January 1915.

effect, and the benefits would not be cumulative. We must not hastily assume, however, that there would be no indirect racial effects, and this illustrates well the intricate nature of all these problems. Conscription is not universal service, for there is no such thing; the weaker males are left out of the scheme, and thereby gain two or three years [1] start in the struggle for social and economic success. This gives an advantage to the unfit. On the other hand, the benefits of service in the Army may be great enough ultimately to compensate, and more than compensate, for the loss of two or three years' work, so that the fit will come out of the process with a greater advantage over the unfit in the struggle for success than they originally possessed. But that does not end the matter. There is a tendency, at least in England, for social and economic success to be correlated with a birthrate so greatly diminished that it more than counteracts the low deathrate associated with a good environment, and hence conscription might well have a slightly harmful racial effect on account of the very fact that it is advantageous to the individual. And there are other complications. I mention all these opposing and uncertain factors in order to show how hopeless it is at present to attempt to reach any reliable conclusion on the subject.

It has already been stated that the racial effects of war must be theoretically inferred to be very different according as we suppose the different theories of heredity to be true. But not only will the effects under the Neo-Lamarckian and Mendelo-Mutationist hypotheses be very different from the presumed effects under the Neo-Darwinian theory, which is the theory upon which the pessimists have usually based their argument, but the applications of the Neo-Darwinian theory itself have not been thought out with the ingenuity that one would have expected. I should perhaps say that the Neo-Darwinian theory is that which implies the possibility of producing a stable change in the character of a population by the summation of minute variations through the selection of individuals showing those variations, but which excludes the transmission of acquired characters.[2] Now a certain number, say about 75 to 80 per cent.,

[1] Those who advocate national service for Great Britain usually propose only six months' continuous training.

[2] Lest any reader should be unaware of the fact, I may mention that this is not exactly Darwin's own view, for he himself believed in the transmission of acquired characters as a subordinate factor in evolution.

of the males of military age are taken off to war under conscription, whilst a smaller section, and that the weakly section, remains behind, and this process is said to be "reversed selection," *i.e.* it is the unfit instead of the fit who are predominantly selected for fatherhood. It may be reversed selection, but also it may not be, because fortunately not all who go to the war are killed, and until we know both the quantity and quality of those who return we cannot decide whether the whole process is reversed selection or not. The problem may be best expressed algebraically. Let A be the males of military age who remain civilians. This group consists of a small number of seriously defective persons (with regard to this small minority the pessimists have a strong case), of a considerable number who are naturally weakly in various respects, and of another section of men who possess weaknesses which are merely acquired and are therefore, *ex hypothesi*, not heritable. Moreover, in the exceptional case of Great Britain, voluntary service swells the size of A by a large addition of those who are fit for service, but who do not enlist—the men with private responsibilities that they feel unjustified in leaving, the indifferent, political dissentients, Quakers, and so forth. Let B represent the entire body of soldiers. B then passes through the dangers of war, and we will represent the survivors, fortunately a large majority in very recent wars, by the letter M. Now I have not seen it anywhere definitely stated, but there appears to underlie the argument of the pessimists a tacit assumption that the average level of inborn capacities will be the same in M as in B. If that were true, then of course the average quality of $A + M$ would be lower than that of $A + B$. I may say in parenthesis that the problem is not really one of simple averages, because even Darwinians admit that the offspring of A, even if A married only women as inferior as themselves, would tend somewhat to revert towards the racial mean, but for the moment we will ignore that incalculable complication. Now in so far as some of the members of A possess definite heritable defects which are wholly absent from B, it is no doubt true that the quality of M is identical with that of B, though even here there may be a variable liability to develop some of the defects. But with other characteristics, in which there is a gradation of inborn variation—muscular strength, the endurance of fatigue, height, strength of nerve, etc.—the case is quite otherwise. It is a legitimate assumption that war exercises

on B the right kind of selection. Even in the present war there has been much hand-to-hand fighting, and deaths are due to disease as well as to the enemy's projectiles. Hence M is superior in quality to B, and may be so much superior that the average of $A + M$ is actually above that of $A + B$. There are of course no anthropometric data, but since the pessimists have appealed to mere hypothetical possibilities, we will take a hypothetical instance. Consider marching powers. Let $A =$ 25 per cent., $B = 75$ per cent., and $M = 50$ per cent. of the males of military age. And, let the marching powers of A be represented by 100, of B by 120,[1] and of M by 124. Then the average marching powers of $A + B$ equal:

$$(100 + 120 \times 3) \div 4 = 115.$$

Similarly the average marching powers of $A + M$ will equal:

$$(100 + 124 \times 2) \div 3 = 116.$$

In such a case as this, which is perfectly possible, war would actually raise the standard of the whole manhood taken collectively. This result, however, could not of course be produced if M were a very small quantity compared to A and B, and Dr. Saleeby gives some figures which go to show the extremely high rate of casualties in the Roman armies. The point to be observed, however, is that the final effect varies with the balance between the quality and quantity of M. Some of the sires in a warring generation are weakly civilians, but we must not forget that others are returned warriors.

So much for the applications of Neo-Darwinism. If the Lamarckian factor (use-inheritance) be operative, it would presumably upset the doleful conclusions; because just as the individual becomes adapted to the hard life of campaigning, so also would the race tend to be hardened. Of course the inheritance of injuries need not be considered.

The Mendelo-Mutationist theory introduces all manner of complications. According to Mendelism variations are of two distinct kinds, unstable "fluctuations" and stable "mutations," and no permanent or stable change in the character of a population can be caused save by the selection of definite mutations. There are said to be many mutations in our species. Man, says

[1] These proportions are guesses, of course, but will be recognised as approximations to the truth by all who have taken an interest in athletics.

Prof. Bateson, "is the very type of a polymorphic species."[1] So polymorphic, a Darwinian might say, that Mendelians can only explain him by an exercise of ingenuity which is not always very convincing. For our purpose, therefore, it is necessary to disentangle the evidence as to how far the military authorities select soldiers on the basis of mutations, and how far on account of fluctuations. This is a most difficult task. Certain diseases of the eye are heritable in the Mendelian sense, and these and similar instances further strengthen the case of the pessimists in relation to that minority who possess definite defects. But mutations do not blend with their opposites,[2] and therefore even where a number of individuals with a desirable mutation are lost in war, this will only reduce the proportion of individuals with that mutation in the next generation, and will in no way lower the standard of those who do possess the quality. Moreover, variations in stature, muscular strength, powers of endurance, and so on, are fluctuations, and the extremes of fluctuations tend to revert rapidly to the racial mean. Human height is a classic instance of a fluctuation, and this is interesting in view of the legend that Napoleon "lopped a cubit off the stature of the Frenchman." No doubt Napoleon succeeded in reducing the height of the men of his own time, but we should require convincing evidence before we could believe that the Frenchman of 1913 was shorter than he of 1788.

The foregoing considerations are sufficient to show the complexity of the whole subject. For my part, I am inclined to think that the most potent factor is probably the tendency to revert to the racial mean, and this may well explain how nations have passed with little change through peace and war.

If the peoples of our continent were really to become weaker, more sluggish, more stupid, more enslaved to preconceived ideas, then in truth we might despair of any ultimate good arising out of the present war. But I see no real cause to fear degeneracy. On the contrary, we may hope for an advance, not perhaps in racial qualities, but in the comprehension of our environment. After terrible carnage, the iniquity of the wars of religious persecution was at last realised. So may we hope that this conflict will lead ultimately to some further extension of the realm of justice in international affairs.

[1] Presidential Address, British Association, 1914.

[2] In certain puzzling cases, however, mutations do undergo fractionation.

THE SPINNING PROPERTIES OF COTTON

By W. LAWRENCE BALLS, M.A.
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In a recent number of SCIENCE PROGRESS the writer indicated some of the ways by which purely scientific investigations were likely to yield results of economic value to the cotton trade. The present note purports to show how some unexpected light has since been thrown upon the causes on which the strength of yarn depends, thereby indicating the possibility of a substantial advance in the technique of spinning.

Since the days of Crompton and Arkwright, over a century ago, there has been only one fundamental change in the spinner's craft. The ingenuity of the machinery which can take a hard mass of compressed cotton and turn it first into a fragile rope of parallel hairs, and then into a delicate twisted yarn, seems to have reached the limit of invention. The one advance mentioned above was the introduction of the comber, which—at an intermediate stage in the preparatory processes—extracts every hair below the required length. All the many improvements and refinements which otherwise differentiate the modern spinning mill from its predecessors have been in details, however important, and not in essentials.

As the previous article pointed out, the cotton trade has been severely practical in its methods, for good reasons. Nevertheless, some technologists have been impressed by the imperfections of even the best products, and combining such criticism with the actual technical difficulties experienced in the mills, the whole problem reduces itself to one essential matter—the strength of the yarn. For a given "count" or fineness of yarn, and for a given number of twists per inch, the best yarn is that which is strongest.

It is the common experience of research students that some side issue of an investigation is often more informative than the particular inquiry first projected, and that when a subject is being studied from several different aspects, these side issues

have a trick of converging to some unlooked-for discovery. The conclusion here sketched is a good instance of such convergence, none of the contributory investigations being such as were likely to lead to results having any bearing on the technique of a spinning mill; nor, indeed, was any such conclusion desired. It may be summarised as follows.

The familiar hairs of cotton lint are each a single dead cell, having a delicate but resistant cuticle and a primary wall, with a secondary thickening of cellulose. For most purposes these cells may be regarded simply as cylinders, the final living dimensions being from about 10 mm. to 50 mm. in length, and from 0·022 to 0·013 mm. in diameter. In the secondary thickness of the wall there are numerous oblique simple pits; consequently, when the internal osmotic pressure falls, just before the opening of the capsule-fruit and the death of the lint-hair cell, the pits close up, and the cylinder undergoes torsion. Further reduction of pressure through desiccation allows the cylinder wall to collapse, and this collapse follows a spiral line, forming convoluted ribbons with thickened edges, which are the characteristic cotton lint-hairs of commerce.

The breaking-strain of the normal lint hair-cells depends almost entirely on the thickness of the secondary deposits of cellulose, which is itself determined by inheritance and by environment, just as in the case of the length of the same cells. Incidentally this thickness will obviously influence the width of the ribbon, and also the amount and pitch of the convolutions. The breaking-strain, or lint hair-strength, has long been known to have no appreciable effect on the yarn-strength, since only about 20 per cent. of the total available tensile strength in any cross-section of yarn is actually obtained in that yarn, showing that the strength of yarn depends principally upon the grip of adjacent fibres on one another. Yarn breaks primarily by slip, and not by rupture of individual hairs.

The results obtained by introducing pure-strain lint to the spinner have been most instructive. In order to control the seed supply of Egyptian cotton, four existing pure strains which had been bred out or isolated from the commercial varieties were propagated into bulk. The lint obtained from such pure-strain populations is necessarily more regular in length and breaking-strain than that from impure varieties, but nothing was known about the spinning properties of the lint of these strains when

they were chosen for propagation (except that it was of the length required in each case), for the very good reason that no such information can possibly be obtained until enough of the seed is available to raise a crop under field conditions. When this had been done it was found in the spinning-tests that all four were equal to high grades of the equivalent commercial varieties, although these are the sole survivors of a stringent process of economic selection. That four random choices should have been equal to, and even markedly better than, the standard commercial product, is a clear indication that the mere elimination of gametic impurity, leaving only the irregularities due to zygotic fluctuation, is sufficient to improve the strength of the yarn. Confirmation of this view is found in the fact that most of these pure-strain samples which spun so well had previously been condemned by expert graders on the grounds of their appearance and handling, owing to indifferent cultivation; the results of the spinning-tests were invariably better than the handling of the sample had indicated, sometimes to a degree which would have been ludicrous, had the consequences not been serious.

The question therefore arises as to the cause of this improvement. If yarn-strength can be improved by introducing the cultivation of pure strains, could not machinery be made to "purify" the lint of impure varieties, and so avoid the expense and trouble of organising pure-strain seed-supply systems?

On examining the available data from the preparatory processes of spinning it would seem that the amount of waste removed in carding and combing both pure and impure lint is much the same; on the other hand, since the comber waste consists of short hairs, and since the frequency distribution of lint-hair length has a lower coefficient of variation in the pure strain, one might reasonably expect the waste to be less. This discrepancy seems to indicate that much of the waste consists of hairs which have been damaged in the preparatory processes (from ginning onwards) and not only of hairs which were initially short. The evidence regarding length thus throws no light on the reasons for the superior spinning properties of pure strains.

The solution seems to be found in the breaking-strain characteristic. In the past it has been rather too easily assumed that differences in "fineness" of lint were equivalent to differ-

ences in the "diameter" of lint-hairs, measured as the actual width of the convoluted ribbons. This equivalence seems, however, to hold good to a much less limited extent. Two of the writer's pure strains had practically the same diameter, but one was a typical "fine" cotton and the other decidedly robust. The difference lay almost solely in the thickness of the cell-wall, the fine cotton producing hairs which thickened their wall to a smaller extent than those of the robust cotton. The former has been spun satisfactorily to 180's count,[1] the latter only to 90's. Consequent on these differences in wall-thickness, the breaking-strain of the average hair in the fine cotton was much less than in the relatively coarse one; the difference was of the order of 5·7 grams in the latter to 3·6 grams in the former.

Since the yarn from the fine cotton would be stronger than that from the coarse one, if both were spun to the same count, it follows from the above figures that the weakest hairs make the strongest yarn, even when the diameter is constant. This apparent paradox is actually quite reasonable, when we remember that yarn-strength depends mainly on the grip of hair to hair, and is therefore—convolutions of the hairs being equal—a function of the total Hair Surface per unit weight of cellulose, which ratio is obviously greatest when the wall is thinnest.

A further inconsistency has still to be eliminated from the argument, for the expert grader of cotton knows that weak lint is inferior lint. The difficulty here lies in the meaning of the terms "weakness" and "strength" as used by the grader to describe the manner in which a tuft of cotton has broken under the strain applied by his hands. The terms cover a complex of phenomena. A sample should break evenly, with a snap, and not raggedly, if it is to be described as "strong," so that the term embodies uniformity of breaking-strain. There is, further, the question of friction: a tuft of lint may be quite unbreakable in the hands, the tightest grip of thumb and finger being unable to retain it up to the breaking-point, but if both ends of the tuft are touched with sealing-wax it will break with ease, because the hairs cannot then slip through the grip. Without entering into further details, we can see clearly that the term "strength" as used by the student of single cells, by the grader, and by the spinner, has three entirely different meanings, so that no objection to the writer's view can be raised by the grader on a

[1] Count = number of hanks of yarn, each 840 yards long, from 1 lb. of cotton.

matter of words alone. In point of fact, some of the pure-strain samples were condemned by graders as being weak, and yet made most excellent yarn.

Admitting then the possibility that the strongest yarn may be spun from the weakest hairs, provided only that these hairs have developed sufficient secondary thickening to form their normal convolutions, it remains to see how this bears upon the undoubted superiority of pure-strain lint.

The explanation seems to lie in the frequency distribution of single-hair breaking-strain. The pure strain of fine cotton mentioned above had such a low breaking-strain under even the optimum conditions of cultivation, that admixed rogue plants and natural hybrids (formed when a portion of the strain was exposed to crossing) could be detected by the greater strength of their lint-hairs. It follows that any sample of fine cotton derived from other than a perfectly pure strain must contain a certain proportion of strong hairs. Now there is nothing in the existing machinery which would tend to eliminate these strong hairs, they being quite normal in all other respects; they are indeed less likely to be removed, and the percentage of them in the sample probably increases as the successive preparatory stages of spinning are survived.

The presence of such strong, thick-walled hairs at any given point in the yarn will weaken the yarn at that point, much as the inclusion of an inch or so of horse-hair would weaken it, simply because of the consequent reduction in hair-surface. Such hairs being absent from pure-strain lint, so far as gametic causes are concerned, the yarn produced from it largely escapes this defect. The absence of such hairs is not easily perceptible to the grader, who therefore inevitably under-estimates the value of pure-strain lint. The whole matter opens up a number of interesting problems in technology, in the physics of colloids, and in agriculture, but it will suffice to consider the immediate application.

The invention of the comber has already been mentioned. The function of this machine is to remove all short hairs. With our present recognition of the similar *undesirability of strong hairs*, it may well be asked whether it is not possible to devise a machine which will eliminate them also. The advantages to be obtained from the use of pure-strain lint seem to be very appreciable, but an intelligent control and somewhat elaborate

organisation are needed to supply it, especially as pure strains are not so catholic in their tastes for different localities as are impure varieties in which natural selection can act. Moreover, the cotton trade is not yet organised with the solidarity needed for co-operation of this kind between the spinner and the grower. At the same time, it is not easy to see how any such mechanical device could be constructed; the removal of weak hairs would be simple, and is indeed already done, but the removal of strong hairs—except by a process of elimination which broke all the rest—would seem to be impossible; the other points of difference, in weight, volume, and thickness of the hair, while relatively great, are so small in absolute measure that they seem to be beyond the reach of commercial machinery.

To the writer's mind, however, there seems to be no doubt that the invention of such a machine, or of any other method achieving the same result, would constitute a real advance in the technique of spinning. This conviction is strengthened by the accidental origin of the discovery, as the result of the convergence of several independent lines of research, and the purpose of the present article is to present the possibility for the co-operation of other investigators.

ESSAY-REVIEWS

HOW THE PROBLEM OF THE X-RAYS HAS BEEN SOLVED, by PROF. FREDERICK SODDY, F.R.S.: on **X-Rays and Crystal Structure,** by W. H. BRAGG, M.A., D.Sc., F.R.S., and W. L. BRAGG, B.A. [Pp. vi + 228, with 4 Plates and 75 Illustrations.] (London: G. Bell & Sons, Ltd., 1915. Price 7*s.* 6*d.* net.)

THE title of this book connects two branches of science that have not been previously connected, but its subject-matter is an essential link in the connection between, not two branches, but almost the whole range of modern physical science. There never was any doubt about the nature of the electric waves discovered by Hertz, now, in wireless telegraphy, so powerfully applied to the arts of war and peace. A simple magnification of the scale, a multiplication of wave-length from ten thousandths of a millimetre to metres, and the mind passes easily from the old to the new, from light to electric waves. A magnification of the scale is easy to conceive and easy to put to the test, but the opposite process, though easy enough to imagine, is by no means so easy experimentally to put into evidence. Fraunhofer's original diffraction grating consisted of fine silver wires wound regularly upon a frame, and with this rough instrument he diffracted and measured for the first time the wave-length of sodium light. A grating capable of diffracting electric waves, on the other hand, would have to be an enormous structure in which the width of the space between the wires was comparable with the wave-length of the waves.

An extension of the scale of known wave-lengths, similar to that in passing from light to the waves of wireless telegraphy but in the opposite direction, occurred at about the same time as wireless messages first began to be transmitted through the ether. But many years had to elapse before this new excursion of science into the region of the infinitesimal was clearly understood or before the new rays deserved any other than their original name, signifying, in algebra, an unknown quantity.

The modern grating of Rowland used for the diffraction of light-waves with its five thousand or more lines to the centimetre, ruled by a diamond on a glass plate with perfect regularity, is rightly regarded as a triumph of mechanical art, beyond which it is impossible for art to go. Yet something ten thousand times finer had to be found before the X-rays were to belie their name and take their right place in the gamut of electromagnetic radiations, as far removed from visible light-waves in the direction of smallness as these in turn are in the opposite direction from those of the waves of wireless telegraphy.

For seventeen years after they were discovered by Röntgen, the real nature of the X-rays was discussed and left undecided until Dr. Laue, of the University of Zurich, in 1912 conceived the idea of employing a crystal, with its marshalled ranks of atoms, packed in regular order, many million to the linear centimetre, to do for X-rays what the diffracting grating does for visible light. The success of the attempt opened a new era of activity, not for one but for many sciences, and the first chapter of this new era in the science of crystallography is set forth in these pages.

It is fitting that in this work the pioneers in this country should have been Prof. Bragg and his son. The γ-rays of radioactive bodies, and what are in effect artificially generated γ-rays—the X-rays of Röntgen—are old acquaintances of Prof. Bragg, who turned to them, after his distinguished elucidation of the problem of α-ray transmission, for fresh worlds to conquer. For many years he laboured to establish a view of their nature which is now mainly of historical interest, but it was the discovery of Laue and his colleagues which finally directed the inquiry into its present channels.

The problems involved have a general resemblance to those of the diffraction of light by a ruled grating, but are considerably more complex, for, in the crystal, instead of lines ruled at equal spaces in a plane, we have to deal with points or atoms orientated in definite but unknown manner in three dimensions of space. At the outset both the wave-lengths of the X-ray and the distances between the diffracting points of the crystals were quite unknown, but very soon both unknowns were determined in absolute measure. A great simplification of the problem was effected by the younger author at the outset. He showed

that, since each atom of the crystal emits spherical diffraction pulses, the latter must resolve themselves into a *reflected* wave. This reflection is quite independent of any polished surface, but is an interior effect depending on the existence in the crystal of parallel and definitely spaced planes of atoms. A crystal may in imagination be bisected by any number of planes in every conceivable direction, but certain of these, namely those, in general, parallel with the natural faces of the crystal, will be crowded with atoms to a much greater extent than planes taken at random. Fig. 1 shows a beam of X-rays of wave-length λ falling at angle θ on a natural face of a crystal, which consists of parallel planes, p, p', etc., spaced apart a definite distance d. The reflected waves from the lower planes add themselves on to and augment those from the upper plane

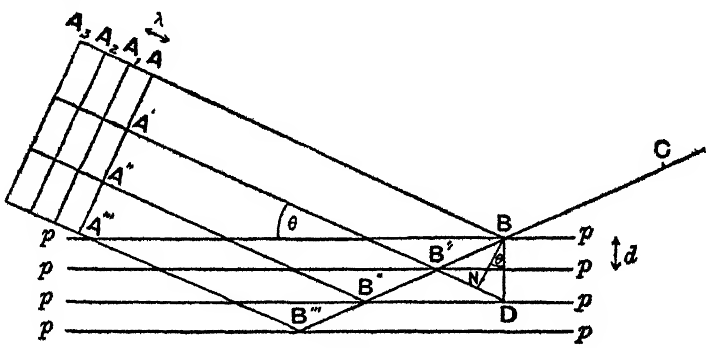


Fig. 1.

only when the length of their path, from the line A, A', A'', A''', say, to C, is some whole number of wave-lengths longer than that reflected from the surface. Otherwise they are in different phases, interfere and cut each other out. In short, the path of the ray from each plane must be the length of the line ND, which is $2d \sin\theta$ longer than that from the one above it, and therefore $2d \sin\theta$ must be some integral multiple of λ, the wave-length. According as this multiple is 1, 2, 3, and so on, we have reflections of the first, second, and third order, much as in the diffraction grating.

A crystal imagined to be slowly rotating from an initial position in which a beam of X-rays, of definite wave-length, just grazes its face, will reflect X-rays intermittently as its face makes the angles θ_1, θ_2, θ_3, etc., with the beam. The sines of these angles, or the angles themselves approximately if, as is

usually the case, they are small—are in the simple ratio 1, 2, 3, etc. With the same X-rays another face of the crystal with a different spacing d' between the planes will reflect at another set of angles $\theta'_1, \theta'_2, \theta'_3$, etc. Nothing else is required to determine directly the ratio d/d' of the spacing in different planes. This is generally known from crystallographic considerations, and the direct verification of the latter is easy to carry out, and furnished one of the first proofs of the theory.

On the other hand, if different X-rays but the same face of the crystal be used, the ratio of the wave-lengths of the two X-rays at once follows. But the absolute determination of both λ and d is possible when a complete knowledge of the structure of any one simple crystal is obtained.

For isomorphous crystals and for similar planes the spacing d should vary from crystal to crystal proportionately to the cube root of the molecular volume, and this was found to be the case for a whole range of cubic crystals. From this molecular volume and the mass of the hydrogen atom, these distances d can be computed, and consequently λ also. Thus, in rock salt, the distance between the successive planes parallel to the (100) face was found to be $2 \cdot 8 \times 10^{-8}$ cm., and the wave-lengths of the two strong characteristic X-rays of rhodium, for example, $0 \cdot 607$ and $0 \cdot 533 \times 10^{-8}$ cm., *i.e.* 0·607 and 0·533 Ångstrom units. Compare this with the wave-lengths of the visible spectrum, from 7,000 to 3,500 Ångstrom units, and this again with the waves used in wireless telegraphy, half a kilometre or more in length, and ponder for a moment on the incalculable service this extension of the scale of radiations has incidentally rendered to humanity. What unexplored stretches in this vast gamut yet remain blank! What secrets may still lie hidden within so vast a range!

It has been too often the reproach of the otherwise well-trained scientific man that crystallography remains to him a sealed book, a science with the meaning even of its nomenclature he is unfamiliar, a science of one too many dimensions to be easily pictured in what, owing to the fatal facility of scribbling upon paper, is in danger of becoming a two-dimensional mind. The authors have met this difficulty frankly and well, and it would be difficult to find anything of the kind more excellent than Chapter V., dealing with the rudiments of crystallographic principles. The labour of assimilation is

lightened by numerous well-executed drawings of solid models, some of which, by the courtesy of the publishers, are here reproduced. On the other hand, the complementary task of giving the crystallographer the necessary introduction to the science of the X-rays is less successful. For example, we read in Chapter IV.—" The next table is well known." Yes! but to whom? One unfamiliar with the subject is plunged into very intricate phenomena at this point, with the minimum of help and guidance.

The first application of the new method made to the problem of crystal structure at once showed its power. A set of isomorphous cubic crystals, sodium chloride or rock-salt, NaCl, potassium chloride or sylvine, KCl, potassium bromide, KBr, and iodide, KI, gave, by reflection from the (100) face, a set of values for the various crystals of *d*, the spacing between the planes, proportional to the cube-root of the molecular volumes of the crystals. But potassium chloride alone gave for the spacings for the (100), (110), and (111) faces the ratio $1 : \sqrt{2} : \sqrt{3}$, required of a simple cubic space-lattice. An examination of the other crystals showed that potassium chloride was apparently simpler than the other salts because its constituent atoms, potassium and chlorine, being nearly identical in mass, act indistinguishably towards the X-rays. For the other salts the behaviour was completely explained on the view that each of the two sets of atoms, of alkali-metal and halogen respectively, lie on two different face-centred cubic space-lattices intersecting in such a way that when the two sets of atoms are alike the form reduces to the simple cubic space-lattice. This showed at once that it is the individual atoms, and not the molecules, which act as the diffracting points in the crystal.

This set of crystals is depicted in fig. 2. The black and white dots represent the two kinds of atoms. The (100) planes are those parallel with *ADHE*, the (110) planes are parallel to that containing the points *CDEF*, and the (111) planes are parallel with that containing the points *BDE*. It will be seen that in the (100) and the (110) planes the atoms consist of equal numbers of both kinds, but the (111) planes consist alternately of all halogen and all alkali-metal atoms, the one kind of plane being midway between two of the other kind. This is shown in the figure in the diagrams below the crystal-model.

Another cubic crystal, zincblende, ZnS, was found also to

have its zinc and sulphur atoms respectively in two separate intersecting face-centred cubic space-lattices, but the mode of intersection is different from the former case (fig. 3). The one set of atoms occupy the centres of four out of eight of the eight small cubes into which the large cube is divided. Or, what may not be so immediately obvious, each atom of either kind occupies the centre of a regular tetrahedron, with four atoms of the other

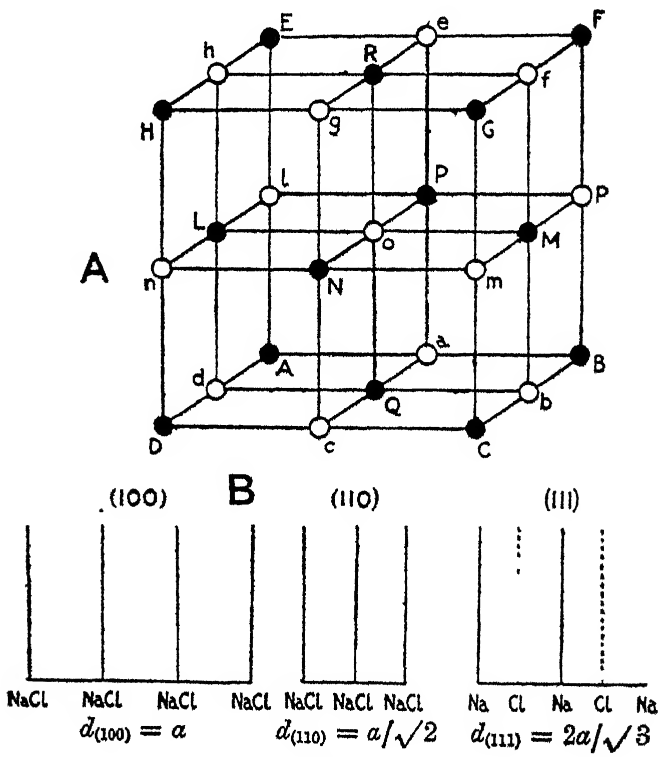
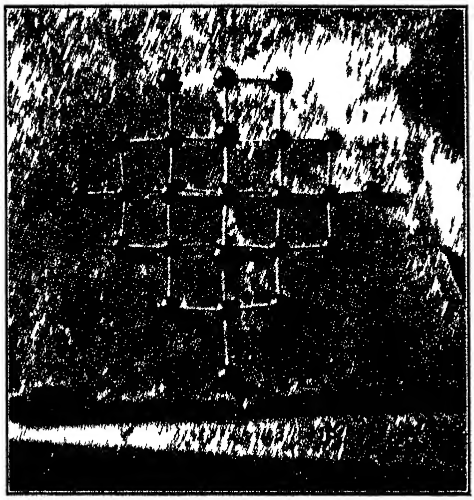


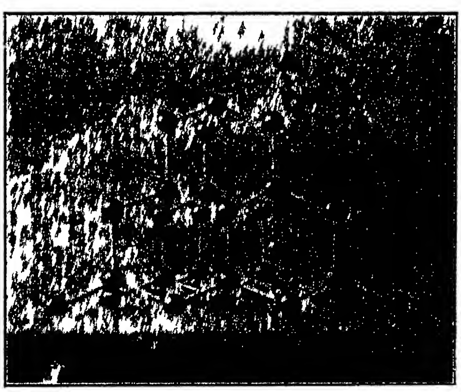
FIG. 2.

kind at the four corners of the tetrahedron. This is best seen for the tetrahedron *DLNQ* of the figure.

The next crystal, the diamond or crystal carbon, is one of the most instructive. Its structure is identical with that shown for zinc sulphide, with this difference, that both sets of atoms are now alike. Hence crystal carbon is built up out of atoms of carbon so orientated that each atom occupies the centre of a regular tetrahedron the points of which are occupied by four other carbon atoms. One can hardly appreciate too highly the



Horizontal and vertical planes perpendicular to the paper are (110) planes



Horizontal planes perpendicular to the paper are (111) planes

FIG 4

genius of Lebel and van't Hoff, who, without any of these direct and powerful weapons of modern physical investigation, arrived forty years ago at this conception of the spatial relations of the carbon atom from the constitution of optically active organic compounds. Nor, indeed, must the patient work of the mathematical and experimental crystallographers be forgotten. Working solely from the external forms of crystals and the

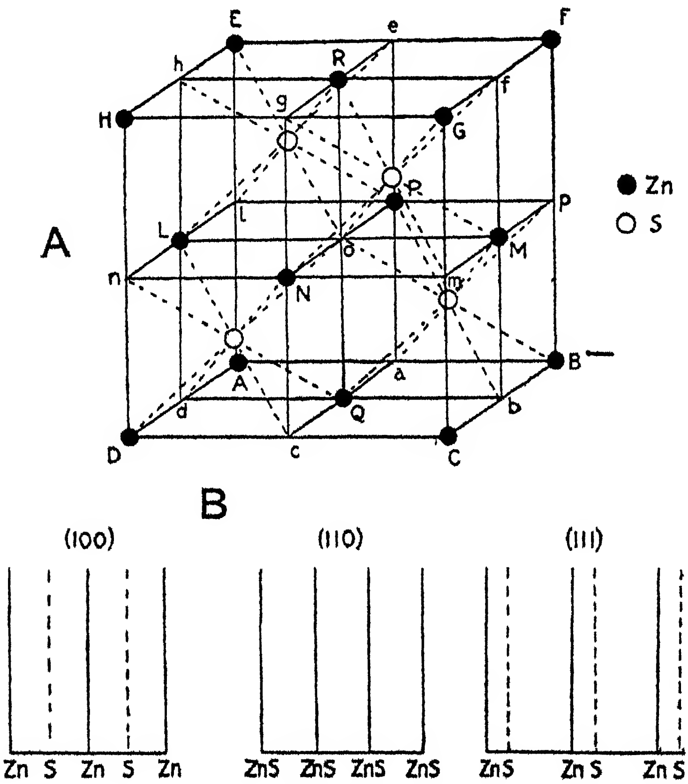


FIG. 3.

general principles of solid geometry, they prepared the way for these striking and rapid advances.

Other representations of the structure of the diamond crystal, built up out of the ordinary model carbon atoms of the organic chemical laboratory, are shown in fig. 4. The peculiarity of this structure from the standpoint of the X-rays is well seen in the lower figure. The (111) planes of the crystal are arranged in pairs with a gap between twice that between the components of

a pair. This results in the entire suppression of the second order spectrum in the reflection from this plane, and of the first order spectrum in the reflection from the (100) plane, through interference.

Of other interesting cubic crystals, calcium fluoride or fluorspar, CaF_2, gives X-ray spectra analogous to those given by the diamond. Here the weight of the two fluorine atoms is very nearly the same as the weight of the single calcium atom. Its structure is that of zinc sulphide, with the difference that there is a fluorine atom in each of the eight small cubes of the figure. In iron sulphide or pyrites, FeS_2, another cubic crystal, the structure is similar to that of fluorspar, except that the sulphur atoms no longer occupy the centre of the small cubes, but a position on the diagonal joining one of the iron atoms with the

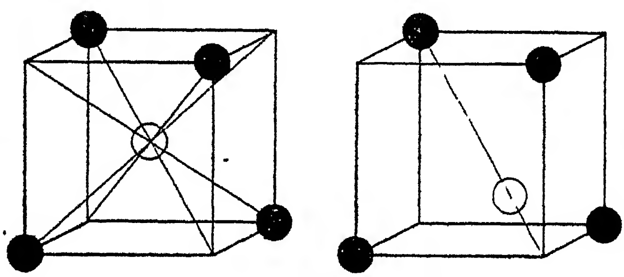
(a)  (b)

Fig. 5.

unoccupied corner of the small cube, approximately four times as far from the iron atom as from the unoccupied corner. In fig. 5, (*a*) represents the arrangement for fluorspar and (*b*) for pyrites.

The authors then go on to the consideration of more complex crystals, of which the examination has already been begun, but these examples must suffice. To do full justice to such a subject would require a mastery of many branches of science not yet interlinked. "We have refrained from the discussion of a number of interesting points of contact with other sciences"—it is stated in the preface—"and with older work, such as for example the remarkable investigations of Pope and Barlow." Tutton's work on isomorphous crystals, and his clear distinction between eutropic series and isomorphous series, is recalled by the following passage: "It is interesting to notice that ammonium chloride is in no sense isomorphous with the other alkali haloids,

although it crystallises in the cubic system. Each unit cube of the structure contains in the one case half a molecule of KCl and in the other case a whole molecule of NH_4Cl." There are other cases also in which workers in allied fields must supply for themselves the due co-ordination of the old and the new, a monopoly of attention being, naturally, lavished upon the latter. But this, perhaps, is inevitable in a subject with so many points of contact with others. It is probable that the work both on its systematic and theoretical side is only in its beginnings, and the book will undoubtedly do much to stimulate the most profitable of all scientific work, the correlation of the various branches of knowledge.

THE ORGANISM AS A THERMODYNAMIC MECHANISM, by HUGH ELLIOT: on

1. **Is the Organism a Thermodynamic Mechanism?** Article by Dr. JAMES JOHNSTONE in SCIENCE PROGRESS, April 1915.
2. **The Mechanistic Principle and the non-Mechanical,** by PAUL CARUS. [Pp. 125.] (Chicago: The Open Court Publishing Company, 1913, price 4*s.* net.)
3. **The Principle of Relativity,** by PAUL CARUS. [Pp. 105.] (Chicago: The Open Court Publishing Company, 1913, price 4*s.* net.)
4. **The Analysis of Sensations,** by Dr. ERNST MACH. Translated from the First German Edition by C. M. WILLIAMS. [Pp. xiv + 380.] (Chicago and London: The Open Court Publishing Company, 1914, price 6*s.* 6*d.* net.)
5. **The Mirror of Perception,** by LEONARD HALL, M.A. [Pp. 129.] (London: Love & Malcomson, 1914.)

AN interesting feature, which constantly recurs in the history of science, is the gradual progress of unpopular doctrines against a solid body of hostile superstition and ignorance. Science is unpopular, partly because it demands severe mental concentration, and partly because its conclusions and principles are often at variance with those which the public desire to believe, and which, therefore, they intend to go on believing, notwithstanding any evidence that may be presented to them in a contrary sense. Among the doctrines which are thus exposed to public animosity is the theory that an organism—animal or plant—is a machine, all the processes of which are controlled and determined by physical and chemical forces

identical with those found in the inorganic world. The public prefer to believe that an animal, or at all events a human being, is inspired by a spiritual element which is entirely beyond the control of any gross material laws; they prefer to believe it, and they therefore do believe it. It is true that they have not inquired into the evidence; it is even true that they resent being offered any evidence which might disturb their existing predilections—though in truth the danger of so untoward a result would scarcely be very imminent.

There are a certain number of people, however, who take upon themselves the task of defending unpopular doctrines which have been established by science. They do not embark upon this course in order to convert the public, for such a project would be comparable to trying to sink a Dreadnought by pricking it with a needle. There are at all times, moreover, a certain number of writers who *do* examine the evidence, who *do* genuinely desire to find the truth, and who yet come to results which happen to be in harmony with those demanded by the public. Such are some of the writers whose works I am about to criticise. They are all careful thinkers and able writers; and, although a critic may differ profoundly from the views they express, he cannot but recognise the labour and honesty of purpose which inspire them.

I propose to deal first with Dr. James Johnstone, whose work on the *Philosophy of Biology* constitutes one of the best defences of the popular view with which I am acquainted. Not that his treatment itself is in any way popular. He is filled with the philosophies of Bergson and Driesch, two writers who have obtained much vogue with the public, no doubt because the public feels that, if it could understand their doctrines, it would certainly approve of them. Dr. Johnstone does not intrude Bergsonism overtly upon us; but he writes under Bergsonian influence, which makes him quick to see and collect any biological events which harmonise with that philosophy, while slow to observe or record other events which do not harmonise with it. I wish in particular to comment on the special point raised by Dr. Johnstone in the last number of this review. The writer, it may be remembered, starts with a paradox of physics; and then sets forth a vitalistic theory of life which he regards as the only possible mode of escape from that paradox. The paradox is as follows: Under the second

law of thermodynamics, there is a universal and unceasing tendency towards the degradation of energy in the form of dissipated heat. The universe, as we see it, is passing from a state in which energy is concentrated at certain places at high potential, to a state in which all the energy will have become dissipated equally everywhere; and the universe will then be a dead, inert, motionless existence, possessing a uniform temperature, representing the degraded sum-total of all the various forms of energy previously concentrated in particular regions. But the universe has existed throughout infinite time: therefore the state of final equilibrium must already have been reached, unless there were somewhere a source of "restoration of available energy"—that is to say, a phenomenon which *reverses* the degradation of energy, and sets up contrary processes leading once more to concentration at high potential. And we are then introduced to the suggestion (already made by Dr. Johnstone in his *Philosophy of Biology*) that Life is the source of this reversal of physical processes.

Now I propose, in the first place, to deny that we are called upon to believe in any such reversal of the second law of thermodynamics; and in the second place to deny that, if there were any such reversal, it could have any possible connection with Life. Had I more space available, I should have begun by criticising Dr. Johnstone's statement of the law itself, which appears to me imperfect in certain particulars. But in order to limit the discussion to the truly essential points, I am prepared to accept his statement of the law as true and accurate.

I may begin then by pointing out that the whole argument is purely deductive in character. Energy is continually being degraded: the past duration of the universe is infinite: therefore, unless there be some source of restoration, the degradation must long ago have been complete; but it is far from complete: hence there must be some source of restoration. Now we may gravely doubt the value of any chain of deduction which leads you by two or three simple steps from the infinity of time to a theory of vitalism. The infinity of time is too vague, too metaphysical, and too incomprehensible a doctrine to be suitable for use as a major premise for the deduction of physical and biological facts. It may, indeed, be questioned whether *any* deduction should suffice to convince us of the reversibility of a law of thermodynamics, which is based (like the law of con-

servation of energy) on the uniform and unbroken experience of mankind. But if any deduction of this kind could be valid, it surely is not one which is based upon the infinity of time—a theory which we cannot genuinely bring before consciousness and which staggers every attempt to realise it. Let me pass, however, from Dr. Johnstone's logic to his physics.

In order that the second law of thermodynamics may be reversed, he tells us, we must assume that at a given moment in some portion of gas every molecule should happen to collide with another molecule moving at the same velocity and in the same straight line. In that case the direction of motion of every molecule would be instantaneously reversed, while its velocity would remain unaltered; there would then occur a reversal of sign in the second law of thermodynamics: entropy would be diminished, instead of being increased. The chance against such an event occurring is, as Dr. Johnstone rightly observes, immeasurably great; but, as he continues, time and space are also immeasurably great, and we may therefore suppose that the thing can actually happen.[1] Yet he does not adequately realise the infinite improbability of the occurrence. He says: "At any instant many of the molecules in a decilitre of gas must be approaching each other in the same straight line and with the same velocity." On the contrary, the odds against two molecules approaching each other in the same mathematical straight line is infinity to 1. The odds against their moving with a mathematically equal velocity is also infinity to 1; and the odds against a combination of these two events is the square of infinity to 1. Further, the chance that all the molecules in a decilitre of gas should fulfil these conditions at the same moment is represented by a fraction of which the numerator is 1, and the denominator is infinity raised to a power equal to twice the number of molecules in the gas; and that we may call for practical purposes infinity to the power of infinity.

Seeing that it is Dr. Johnstone's object to throw doubt upon the universal validity of the second law of thermodynamics, it is not apparent why he should have selected a mode of argument which represents the odds against its being "violated" as infinity to the power of infinity. No one has ever claimed

[1] Dr. Johnstone has an inveterate habit of cancelling out infinities on the two sides of an equation. We know that twice infinity = 3 times infinity; but we cannot cancel infinities and say that 2 = 3.

for it on inductive grounds the overwhelming certainty which Dr. Johnstone confers upon it by means of deduction. To most physicists the validity of the law is based upon the unbroken experience of mankind; and that experience, though it may bestow a very high degree of probability, does not rise to the attenuated regions of $\infty^{\infty}:1$. It would have been simpler and much more convincing to say that, as the second law of thermodynamics is based only on finite experience in a very limited time, it may possibly not hold good everywhere, and may not have held good in past times.

Let us pass, however, to the conclusion of Dr. Johnstone's physical deduction. Let us suppose that energy is and has always been degraded throughout infinite time. Yet we see concentrated forms of energy still in existence: are we then bound to infer that "there must be a restoration of available energy"? Certainly not. Dr. Johnstone is fond of dealing in infinities; let us add one more. Let us suppose that the energy in the universe is, like time and space, also infinite—an entirely reasonable hypothesis. Then even the lapse of infinite time would not involve the extinction of all differences of energy-potential. Dr. Johnstone's argument is only sound (even in appearance) on the assumption that the universe is limited; but that surely far exceeds our possible knowledge. We know our own world and solar system; we behold in the inconceivable depths of the universe other worlds and solar systems much like our own. May not these worlds extend right away to the uttermost limits of conceivability? What right has Dr. Johnstone to assume that space is infinite and that the universe is finite? Does he suppose that "the universe"—the great stellar systems which are disclosed by powerful telescopes—are a dot in the midst of infinite and empty space? They may be, but how does he know? How can he venture to found a theory of life upon such an assumption? We know that light travels with a velocity of about 186,000 miles a second. We know that there are stars so remote that the light which they emitted when Christ was alive is only now reaching us. The space which separates us from those stars dwindles to a mathematical point when looked at from the standpoint of infinite space. If we represent that vast distance on a star-map by a space of one inch, there may, for all we know, be other stars at a distance which we should have to represent on the same scale

on the map by a yard, a mile, a million miles, a million light-years! We cannot say that the universe is limited, because such a statement is overwhelmingly beyond our present or possible knowledge; and if we cannot say that the universe is limited, we cannot say that the energy of the universe is limited, nor can we share Dr. Johnstone's surprise that the available energy has not already all been dissipated. Indeed, one cannot but feel that Dr. Johnstone does not appreciate the magnitude of the factors with which he tries to operate. He affirms with confidence that restoration of energy is the "only way out of this deadlock." He forgets the other possible way out named above: which, indeed, seems far more probable than his. But even had I been unable to indicate another way out, it does not follow that because you can only think of one solution, therefore that solution must be true. This is not sound reasoning in any department of science: it is less sound than ever when you are dealing with such incomprehensible doctrines as the infinity of time and space, and similar intangible conceptions, which we are bound by the limitations of our intellect to believe, and yet are for ever incapable of understanding.

Reverting to the above suggested star-map in which the distance of the most remote of visible stars is represented by the length of one inch, we may imagine that on the same scale at a distance of a million miles there may be other bodies; and others again at a distance of a further million and so on *ad infinitum*. Even were these bodies actually no larger than a pea, and even were their temperatures not more than 1° above the average of *our* stellar universe, yet the total amount of energy contained in them would be infinite, and they never in finite time would lose any fraction of their excess of potential over that of our universe. Energy, moreover, may be infinite not only in *extension*, but in *intension*. The vast sources of energy lately discovered within the atom may be paralleled by other infinite stores within the electron. In short, Dr. Johnstone's attempt to argue that the second law of thermodynamics *cannot* à priori always hold good, fails absolutely: he cannot prove any such doctrine by deduction, more especially by deductions with such shadowy and metaphysical premises: he can prove it, if at all, only by induction—by citing a case in which the law is found to be in abeyance; and he does not even hint at any such case.

Except indeed in the case of living organisms! And I now turn to this, the second part of Dr. Johnstone's argument. He has established that there must be somewhere a *restoration of available energy*: he has established it, that is, to his own satisfaction, but not (I hope) to the satisfaction of readers of SCIENCE PROGRESS. He goes on to affirm that living organisms effect that restoration. Plants absorb radiant energy for the purposes of chemical synthesis. They exhibit therefore endothermic reactions: that is to say, their chemical processes are accompanied not by the evolution but by the absorption of heat. This apparently he regards as opposed to the second law of thermodynamics. He admits, of course, that endothermic reactions do occur in inorganic substances; but he says "they do not occur of themselves." What does he mean by that? Presumably that they only occur in human experiments. But an experiment is simply a method of bringing together conditions not very frequently or conveniently realised in nature. The reaction occurs "by itself"; and the peculiar set of conditions under which it occurs may quite possibly and in course of time must certainly happen to come together in nature, as we bring them together in the laboratory. The entire activities of human beings consist in altering the relative positions of objects, *i.e.* in moving things. The whole work of man and power of man is limited to moving objects from one place to another: and the rest happens "by itself." An experiment is nothing more than a rearrangement or transference of various objects; and then the same thing happens as would happen if the same collocation of objects occurred in nature. If sulphur vapour passes over red-hot carbon, carbon disulphide is formed. This is an endothermic reaction, and it occurs "by itself." Presumably Dr. Johnstone will answer that, except in a laboratory, you do not get sulphur vapour in contact with red-hot carbon. You have to heat the carbon, and then pass sulphur vapour over it. But surely, although this particular reaction may not occur in the districts frequented by Dr. Johnstone, he would find it occurring if he were to make a descent into a volcano: and it certainly occurred at large in early periods of the earth's history, and does now occur in all parts of the Sun. Similarly acetylene is formed when carbon and hydrogen come in contact at high temperatures. The reaction again, like that of the plant, is *endothermic*: and it occurs "of itself"; not perhaps in Liverpool, but at all events

in the Sun. Dr. Johnstone is discussing the restoration of energy, not on the Earth, but in the Universe; and we can no more decide the attributes of the Universe by the capacities of Liverpool than we can decide the attributes of God by the capacities of the Lord Mayor.

Hydriodic gas is another endothermic compound which is formed by itself *at ordinary temperatures* when sulphuretted hydrogen passes through iodine suspended in water: there are many other endothermic substances, as, for instance, hydrazoic acid; and of all these it can be said that they occur by themselves. In physics, as well as in chemistry, absorption of dissipated heat happens too constantly to require mention. It happens when a gas expands against external pressure; when water evaporates; when crystals are dissolved in water; when snow is mixed with salt, etc. In some cases the dissipated energy of heat is concentrated into a new form of much higher potential than any previously present. A slight rise of temperature over the ocean causes many tons of water to rise into the sky, where they become condensed as clouds. These are precipitated as rain which gives rise to a kinetic energy that may easily be reconverted into heat of a far higher potential than existed at the commencement of the operations.

Dr. Johnstone appears in short to have misapplied the second law of thermodynamics. When a gas expands against external pressure, it becomes cooled below the temperature of surrounding objects; but that is no breach of the law, for if the gas were restored to its original condition, the cycle of operations would display some dissipation of heat. The heat evolved in the second part of the process would be greater than the heat absorbed in the first part. Similarly in the case of plants the synthesis of compounds of high energy value involves some absorption of heat; but this again does not contradict the law, for if the compounds were broken up again into their former constituents, the certain result of the entire operation would involve a certain dissipation of heat. Dr. Johnstone fails absolutely therefore to show the slightest deviation on the part of organisms from the second law of thermodynamics; he fails to name a single condition in the synthesis of organic compounds which is not realised in "free" nature, far from the dwellings of any human experimentalist. Certain secondary considerations are raised by Dr. Johnstone in the course of his article; but I

venture to hope they are disposed of by the destruction of his primary argument.

I may perhaps be allowed briefly to allude to one. In the case of warm-blooded animals, there occurs an obvious wastage of heat by radiation into the environment; this would appear at first glance as an illustration of the second law of thermodynamics; but Dr. Johnstone is compelled by his theory to deny this apparently obvious application. He affirms accordingly that the production of heat is not a mere chance by-product of organic metabolism, but is a "purposeful" activity of the organism. With this view we may quite well agree; but we cannot agree that because the evolution of heat has a teleological value, it is, therefore, any the less a manifestation of thermodynamic principles. Nearly all the bodily functions are "purposive"; but for all that they are produced by "blind" physico-chemical means. In steam-engines, there occurs a wastage of heat from the boiler, by the loss of hot furnace-gases up the chimney and in other ways. This wastage is that contemplated in the second law of thermodynamics. But it may be, and constantly is, greatly minimised by fitting in the flue an "economiser," or nest of tubes exposed to the hot gases and containing the water to be fed to the boiler. By increasing the size of the economiser, it is possible to extract as much heat as is desired from the wastage and add it to the boiler water. In this case we may affirm that the heat in the waste gases has a purposeful signification, for it markedly raises the efficiency of the engine. The case is analogous to that of heat-production in animals. The exhaust-steam turbine provides another analogy. This turbine, especially used for the winding engines of collieries, is driven by steam (taken at atmospheric pressure or a few pounds above it) that is exhausted from ordinary non-condensing engines. The heat dissipated under the second law of thermodynamics from one engine is thus used to drive another. But the fact that it is put to a definite purpose does not in any way invalidate the fact of its original production as a frictional by-product.

It is interesting historically to note that, whereas the older vitalists attempted to show that organisms were outside the control of the first law of thermodynamics, the newer now apparently have shifted their attention to the second law. It has at length been proved abundantly that organisms fall within

the first law: even Dr. Johnstone admits that. The proof was a severe blow to the vitalist party, which Dr. Johnstone now tries to retrieve with the help of the second law. Not in this case, any more than in the other, is there any sort of basis for the allegation; and we can only contemplate with calm the desperate expedients to which vitalists are now driven. They search high and low for some instance of a law of physics, broken through by organic activity; and no such instance can they find. If their theory demands, as it seems to do, that some fundamental law of physics shall be broken, surely they would be well advised to drop the subject, and pass quietly over to the orthodox biological school.

The work of Dr. Paul Carus, *The Mechanistic Principle and the Non-Mechanical*, is in great part an attempt to deny that evil consequences in practical life flow from a belief in mechanism. There are many writers—and Dr. Carus quotes Mark Twain as an illustration—who are inclined to believe in the mechanistic principle, but deplore the supposed slight which it involves upon the dignity and "divinity" of man. To use such an argument against the truth of the theory is of course nothing else than setting up our ignorant and uneducated desires as the standard of truth. Just the same argument was used during the evolution controversy: some people felt that man lost something of his "divinity" if he was descended from an ape-like ancestor; they denied in consequence that he was descended from such an ancestor. It is scarcely necessary in SCIENCE PROGRESS to point out the hopeless confusion of thought involved. The truth of a theory does not depend upon whether or not we like it: to say so would be to prostitute truth to sentiment and desire. Nevertheless it is very difficult to see why anyone should be depressed by the establishment of one or other theory of human functions. As Dr. Carus remarks, "A man's a man for a' that." No theory, either of his origin or of his physiological processes, alters in the slightest degree his actual nature. He remains just the same, whatever view we take of his constitution; and if we regarded him as suffused with divinity before, we must still regard him as suffused with divinity after we have corrected our theories about him. The whole matter is, however, too elementary and too obvious for further discussion. Dr. Carus examines the mechanistic theories of four different writers, somewhat strangely assorted, Mark Twain, La Mettrie, Prof.

W. B. Smith, and Dr. Bixby, a theist. Dr. Carus deserves our thanks for his defence of the views and character of La Mettrie. That most remarkable philosopher, born in 1709, was the first to apply to human beings those principles of physiological mechanism which Descartes had asserted in the case of the lower animals. La Mettrie had an intellect of surpassing power and originality: he was able to think right off the lines of the conventional beliefs of his day. He drew a sharp and much-needed line of distinction between science and ethics; and his philosophy even at the present day still retains high interest. But it was his misfortune that nearly all the theories which he espoused were unpopular. He was an atheist and a materialist: he regarded man as an animal. His character was uncontrollably gay and lively: and in certain respects he disregarded the code of morals enjoined by the Church. He even wrote and published in his *Œuvres philosophiques* one or two charming essays, which however would scarcely obtain the approval of a Wesleyan Methodist, nor perhaps be accepted by the editor of a Parish Magazine. He has therefore been attacked on the grounds of morality; and Dr. Paul Carus well points out that he was certainly no "worse" than his times. He might have added that the writings in question cannot, as regards morals, be compared for an instant with certain productions of Voltaire, Diderot and other great contemporaries. The opponents of La Mettrie, finding that they disliked his views and were unable to answer them, flew to the resource of blackening his moral character; and so ready was the world to believe his theories false, that his works lapsed almost into oblivion for nearly a century. Lange in Germany first did justice to his memory; and now happily an American translator has published his *L'homme machine* in Chicago.

The Analysis of Sensations, by Dr. Ernst Mach, is a book already tolerably well known in England. Originally published in Germany in 1886, it was translated into English in 1897. A second translation has now been made from the fifth German edition; and the book is in many ways more complete than that with which English readers were previously acquainted. Among several new chapters, one of the most interesting is that dealing with "Physics and Biology: Causality and Teleology." Dr. Mach deals with the special difficulty, which many people find, of bringing the conception of purpose into a mechanistic scheme

of the universe. This difficulty is not to be met by denying the reality of purpose. It is obvious that the structure of animals is purposive; and that many of the actions of mankind are directed to a conscious end. All that we assert is that blind physical forces are the true agency in the production of the phenomena called purposive, as well as of all other phenomena. Their purposive character is no more than a subjective attribute. To other kinds of organic beings, other kinds of physical events would be considered as purposive, while those which we now consider purposive would be regarded as obviously mechanical. Taking a physiological view, Mach observes that "Purposiveness only comes in when the organic functions are resolved into one another, when they are seen as interconnected, as not limited to the immediate, as proceeding by way of dêtours." "By an imperceptible modification in our thought, we can formulate every teleological question in such a way as completely to exclude the conception of purpose." He points out further that in ancient times purpose filled a much larger sphere than it does at present. "Aristotle, for example, conceives heavy bodies as seeking out their position; Hero thinks that, from motives of economy, nature conducts light by the shortest paths and in the shortest times." To which we may add, as a more recent example, that Bernardin de Saint-Pierre believed that melons have ribs for the purpose of facilitating their consumption in family circles. Dr. Mach epigrammatically describes animal life as "nothing but combustion in complicated circumstances," for it is a conflagration which "keeps itself going, produces its own combustion-temperature, brings neighbouring bodies up to that temperature and thereby drags them into the process, assimilates and grows, expands and propagates itself."

The Mirror of Perception, by Mr. Leonard Hall, is a work of originality and ability, though we are wholly unable to agree with its conclusions. He endeavours to show that the physical world is not real, while the psychical world is; the main object of the book is to show that "the material world does not really exist." It appears, in fact, that he has fallen into a very common misunderstanding of the idealist position; for the representation of matter in terms of consciousness does not, properly understood, involve any falling off in the "reality" of material existence. It would be as reasonable to say that because colour can be represented in terms of ethereal undulation, therefore it

ceases to be real. Mr. Hall endeavours to prove, further, that the mind of a man is "the result of the social evolution of a community of protozoan-minds"; and that the minds of Protozoa again are compounded from the aggregate minds of their constituent molecules—a belief which at present can be regarded as nothing more than vague speculation. He desires to supplement Natural Selection as a factor in Evolution by "purpose"; that is, the purpose, not only of the "principal mind" of the animal, but of "all the minds and parts of the minds of which the animal consists." The author here gets into very thin air; and it is scarcely worth while to follow him.

SCIENCE AND THE SUBLIME VISION, or the SEVEN AGAINST ONE: on Science and Religion, by Seven Men of Science: Sir OLIVER LODGE, F.R.S., Prof. J. A. FLEMING, F.R.S., Prof. W. B. BOTTOMLEY, Prof. E. HULL, F.R.S., J. A. HARKER, F.R.S., Prof. G. S. WOODHEAD, Prof. S. P. THOMPSON, F.R.S. (Speakers in Browning Hall during Science Week, 1914). With Portraits, and a Suggestion from JOHN EDWARD STEAD, F.R.S. (Pp. 138.) (London: W. A. Hammond, price *1s.* net.)

NOTHING has been more delightful to mankind during its history than what may be called the Sublime Vision. When we wish to see this Vision we have only to shut our eyes and think. The world becomes a delightful garden made by the Personal Omnipotent for that part of His creation which He loves best, namely Man. Here in this Paradise we wander among flowers and streams for our beatific life, perhaps labouring a little, perhaps weeping sometimes as our friends suddenly disappear from among us, and often gazing heavenward for communion with the Power that has so wonderfully made us. But this is only the beginning of the story. When we disappear so mysteriously from this garden we do not cease to exist. On the contrary, we are instantly translated to another Elysium a thousand times more beautiful, where we shall live for ever. There we meet those whom we knew before in the lower terraces of earth—let us say our old parents, our missing brothers and sisters and children and friends. In this second abode we shall have no troubles, nor care; and the greatest of all sorrows, namely the ending of happiness, cannot threaten us.

In this little book these seven gentle and enthusiastic Knights

of science and religion set out to convince us of the truth of the Sublime Vision. They all wear armour and are seated on white war-steeds. We learn from the Introduction that they conducted the tournament in a place called Browning Hall at Walworth, London, in November, 1914, and were quite victorious. "Materialism had a very bad time of it at their hands. Haeckel was dismissed as 'hopelessly out-of-date and antiquated.'" In other words, those who questioned the truth of the Sublime Vision were routed. As each Knight advanced in turn, dressed in full armour, with his plumes streaming on the wind, he invariably upset his Paynim adversary. We can understand the roars of applause which arose from the crowd; for what is not the Sublime Vision worth to each of us—more than comfort to the bereaved, more than old age pensions to the poor, more than health to the sick, more than strength to the feeble, victory to the weak, assurance to the coward, and finally life everlasting to the dying. Is not all this the greatest possible boon to humanity; and what is there in health, wealth, prosperity or long life that can equal such a gift? And yet we can all acquire it at a moment merely by thinking with the eyes closed. Naturally then the victors in this combat were acclaimed, and will be acclaimed by thousands.

We would not for worlds disturb this complacency if we could; but still, really, some of the deeds performed appear to the old soldier scarcely quite up to form. In fact there is somewhat too much of the great *per saltum* feat, by which the knight in the tournament takes all the opposing difficulties at a single bound. The redoubtable leader of the band, Sir Oliver himself, seems never to tire of exhibiting this somewhat questionable performance. We first force our adversary to admit that something can or may just possibly exist, and then we immediately assume, because he cannot prove the contrary, that it really does exist. In fact the ruse succeeds by causing a clever confusion in the minds of the dull audience between a "may be" and an "is." Now unfortunately there is a very great interval between these things. Using Cartesian co-ordinates, we should say that an absolute positive certainty consists of an infinity of positive reason-units. Gradually, as negative reason-units are introduced, the certainty shrinks first to a mere probability, then to a zero, and the judgment then begins to emerge upon the negative side of the origin, until, when the negative units

vastly outnumber the positive ones, we reach the negative infinity of absolute impossibility. Now, when a thing is just possible, we really mean that it stands almost, but not quite, at negative infinity, that is, that there are a few positive reasons in favour of it in opposition to a large number of negative ones; but in order to turn a possibility into an absolute certainty we have to reverse the process just figured and add so many positive reason-units that the negative ones are entirely swamped, as in $x - 1$, when x is positively infinite.

But our triumphant logicians do not trouble to make this laborious accumulation of positive units. They take their stand to begin with on the mere possibility, and then with a single jump clear the whole range of negative greatness, zero, and positive finity, and land themselves triumphantly in a moment in absolute certainty! This feat is a very agreeable one at public tournaments. It creates vociferous applause, and the endless clapping of hands and waving of handkerchiefs among the fair sex especially. It is all done in a trice; and the Paynim adversary who is standing painfully at the barrier of zero is left looking like a fool while the successful competitor leaps right over his head. For example, suppose that A maintains that there are elephants in the moon. He asks his adversary B whether the latter can prove with certainty that there are no elephants in the moon. When B admits that he cannot, A triumphantly explains that, as B cannot deny it, there must be elephants in the moon. We have noted that this form of argument is constantly being used in numerous controversies by the philosophers of the day. For instance Sir Oliver is inquiring about inspiration and concludes that "it is a possibility." In the very next sentence however he adds, "it is in my belief a fact." Here he has performed this feat—though he does not tell us what has converted with such speed the possibility into the certainty. Again, he argues very rightly that we must not believe man on the earth to be necessarily the highest existence possible. Of course we must not, for there *may be* very many higher intelligences, let us say in Venus, or Mars, or outside the Solar System. Quite rightly, he maintains that if we assert that there are no higher intelligences we are dogmatising in a negative direction. So we should be; but immediately he takes a flying leap over the whole of the intervening difficulties and declares, because we cannot make

this negative asseveration, that therefore these higher intelligences do actually exist. In other words he first refuses to allow us a negative dogmatism and then immediately indulges in a positive one.

Again, he says quite rightly that it is a pitiful business to suppose that nobody knows anything more about the universe than we do; but immediately goes on to assume that there is someone who knows more about it than we do. A third instance is his treatment of the question regarding the brain. Some people think because a blow on the head immediately interrupts consciousness that therefore the brain is the centre of thought. No, he replies, the brain "is the *organ* of mind. If you take a hatchet and smash that organ, the organist will not be able to play. You have not smashed the organist." He, and most of those who take this dualistic view, seem to think that they have proved their case by such an argument as this. It is indeed quite possible. The brain may be a mere mechanism played upon by some superior outside being. So may the liver or the digestion be mere mechanisms played upon by some outside (probably diabolical) spirits of bile formation or of digestion. We cannot assert the negative certainty that they are not. But Sir Oliver and his friends, after securing our assent to this view, immediately go on to assume without further proof that these outside spirits, at least in the case of the brain, actually do exist. Thus in an instant from arguing that mind may possibly be the sonata performed on the organ of the brain by some mysterious outside spirit he at once leaps to the conclusion that the *may be* is an *is* and that this outside spirit exists and that the mind is nothing else but its performance on the said instrument. Of course we may use the same image of the organ and the brain in many other ways which are not so agreeable to Sir Oliver's contention. Thus we know that mind is influenced not only by the smashing of the organ by a blow, but any physician will tell us that it is influenced in a thousand little ways by a thousand little things—sickness, alcohol, opium, internal secretions, indigestion, want of exercise. It will be admitted that all of these affect the organ, but according to Sir Oliver's thesis they certainly should not affect the organist. If the latter is an outside spirit, how can he be driven by such despicably small agencies to play the vile discords which as a matter of fact so often interrupt the

harmony which we hope should be the normal effluence of the mechanism worked by that lofty angel?

But we admit that Sir Oliver claims to have found some of the steps by which he argues the existence of superior intelligences. For instance he says, "It is unreasonable that the soul should jump out of existence when the body is destroyed. We ourselves are not limited to the few years that we live on this earth. We shall go on without it. We shall certainly continue to exist, we shall certainly survive. Why do I say that? I say it on definite scientific grounds. I know that certain friends of mine still exist because I have talked with them. . . . And I have conversed with them as I could converse through a telephone with anyone in this audience now." We wish that he would give the details; but indeed honestly admit that even if he did we should remain sceptical. A little while ago we saw a stately lady, with the remains of considerable beauty, living in a poor apartment far removed from Greece. And yet she told us that she was the Queen of that country. Now we do not imply that our seven Knights are as she is; but our experience in this old world does not allow us to accept every statement that is made—certainly not if it is unaccompanied by the most careful details as to time and place and person, or if we are not privileged to see the experiments. Sir Oliver says, "You must have evidence, of course. The evidence, so far, is recorded in volumes of the Society for Psychical Research." Alas, not all the writers in these volumes are so sanguine as Sir Oliver Lodge. For example, Count Perovsky-Petrovo-Solovovo reviews in the *Proceedings* of that Society for January 1914, the book by Mr. Hereward Carrington on *Personal Experiences in Spiritualism*, etc. He quotes, on page 184, a sentence by M. Aksakoff: "In the decline of life I ask myself sometimes, 'Have I in truth done well to have devoted so much toil and time and money to the study and the publication of facts in this domain? Have I not struck into a blind road?—followed an elusive hope? Have I not wasted my experience with no result to justify all my pains?'" M. Aksakoff, nevertheless, does not regret this devotion of his whole life, but the reviewer adds, "Mr. Carrington seems to agree entirely with this optimistic attitude. May I add that I do so but partially? Certainly, the more I advance in age, the more I am inclined to think that the road is blind indeed and the hope elusive. I wish I had spent the energy

I have devoted to investigation in the domain of spiritism on some altogether different aim. I think the results might have been somewhat more fruitful."

It does seem such a pity that the Personal Omnipotence, when He constructed the terrestrial and the heavenly gardens, had not also made it perfectly clear that it was He who did it. Really, why should poor men with their partially perfected brains be forced to seek in this manner for what they might think ought to be perfectly obvious from the beginning? Then again, the old question arises, are there in this terrestrial garden at least no poisonous fruits, and no adders under the flowers; and if so, how is it that benevolent Omnipotence has allowed them? And a thousand more questions of the same kind might be asked. But our gentle Knights leap over all of them.

There is much with which we agree in the various essays and we would like, had we space, to examine each in turn. One of the best is that by Professor Sylvanus Thompson. But we must really protest against his definition of religion. "What, then," he asks, "is this common element, this *Religion which is at the back of all religions*? I take it they all presuppose this: in the first place, the existence of Higher Powers than man; and in the second place, though not quite universally, that there is a life beyond, an immortality of the soul; and thirdly, that there is an obligation of right conduct, of justice, of mercy, of obedience to duty." Now really the true definition of religion lies only within the third of these clauses. Religion has nothing whatever to do with any form of faith or belief or philosophical deduction. It is the property of the human mind instilled into us by intertribal evolution, because it has been absolutely necessary for our development. In fact, it is as easily explained by the theory of evolution as are our hands, feet, and teeth. Without it mankind, and probably most gregarious animals, would have perished ages ago. Religion is really what the word means, namely obligation and duty. It is what we must do for the nation or race apart from ourselves. The nation or tribe which is not religious must inevitably perish. Surely this is plain enough to the meanest understanding, and there is no necessity to attach to this great word the much inferior cognitions which some seek to attach to it in order to support their theses. To-day we see the most magnificent exhibition of religion; but it is not found so much in the thousands of pastors

who thump their desks every Sunday, or among the philosophers who perform at intellectual tournaments, as among the hundreds of thousands of young men going to their death, and often almost to a certain one, for their country—driven not by any sense of recompense, but by the profound instinct, instilled into them by millions of years of evolution, which is true religion.

The fact is, on reading this little book we feel that our worthy Knights have indeed destroyed their foe but that it is one made merely of wood. The combat is not a real one; it requires no self-sacrifice, and no other quality of true religion. Have they ever asked whether their thesis is on the whole as beneficial to humanity as it is agreeable? There is such a thing as wine which, when red and strong, buoys us for a while, but which, as some of our Knights will certainly confess, is detrimental in the end. It does not follow in this world that because a thing is pleasant it is good. Have they ever thought that the very spirit which they commend may quite possibly be the principal cause, even more potent than wine, of many of the miseries of men—let us say, for example, of the hideous war upon whose Moloch-altar our sons are now being flung, pierced, mangled and bleeding in thousands? What if their creed is not of Ormuzd but of Ahriman? Perhaps, after all, there is a higher God and a higher religion than we read of in this book.

RECENT ADVANCES IN SCIENCE[1]

MATHEMATICS. By Philip E. B. Jourdain, M.A., Cambridge

The war has had a disastrous effect on mathematical publications, at least abroad. No foreign periodicals, which contain the great mass of mathematical literature usually put out, seem to have been published of late, with the exception of some from America, France, and Italy. The *Comptes Rendus* of the French Academy seems to be the only scientific publication in France which has appeared regularly. However—and this is of great importance for all who are interested in the principles of mathematics—the *Revue de Métaphysique et de Morale*, which, in spite of its title, is by no means chiefly devoted to questions of metaphysics or morals, is shortly to resume regular publication. To mathematical logic in particular the limitation of discussion to a small group of nations is especially harmful. The history of it shows that this is so in perhaps a better way than that of any other scientific subject. The first really adequate idea of a science of mathematical logic was due to a German, Leibniz, but his idea of a "universal characteristic" remained in part unpublished and almost entirely unnoticed until Frege, another German, two hundred years afterwards, developed the same idea in a narrower domain, but with even greater subtlety. But since then no German has made any important contributions to the subject, and only one Austrian. With unexampled obtuseness, German mathematicians have treated Frege's glorious logical and mathematical work with that amused contempt that arises from ignorance. They even call it "philosophy." And the modern developments—including proper appreciation of Leibniz and Frege—are due almost wholly to Frenchmen, Italians, Britons, and Americans. The best popular work on the subject is due to a Frenchman, Louis Couturat, who was killed by a motor car last August, but was not a victim of the war. The work of Couturat has done very much to convince those mathematicians who care to think that

[1] To be continued every quarter.

this modern research into the logical foundations of mathematics is simply a natural continuation of the work of three men who must be ranked among the greatest pure mathematicians of the nineteenth century—Weierstrass, Dedekind, and Cantor. It is interesting to remember that Weierstrass once said that the final object of mathematical research was a knowledge of the principles of science.

Some of Couturat's last—but by no means his best—work on exposition of mathematical logic is an article in Windelband and Ruge's *Encyclopædia of the Philosophical Sciences* (English edition, vol. i., Macmillan & Co., Ltd., 1913, 7*s.* 6*d.* net).

If we glance at the works of modern German writers on the principles of logic and science in general, such as Driesch and Natorp, we cannot fail to be struck by the absence of intelligent criticism of modern work on principles. In this they are not very greatly surpassed by Brunschvicg's recent French work on mathematical philosophy, in which the author seems to have tried unsuccessfully to imitate the scholarly learnedness of some of Couturat's works. The best recent works on questions concerning the foundation of mathematics and mathematical physics have all been produced in Great Britain. Of course, the first place must be given to Dr. Whitehead and B. Russell's *Principia Mathematica*, of which the first volume appeared in 1910, the second in 1912, and the third in 1913 (Cambridge University Press). A fourth volume is to be concerned with the principles of geometry. Other questions closely connected with the principles of mathematics, and which used to be considered as "philosophical," are treated in C. D. Broad's *Perception, Physics, and Reality: an Enquiry into the Information that Physical Science can Supply about the Real* (Cambridge University Press, 1914, 10*s.* net), A. A. Robb's *Theory of Time and Space* (Cambridge University Press, 1914, 10*s.* 6*d.* net), and Russell's "Lowell Lectures" of 1914 on *Our Knowledge of the External World as a Field for Scientific Method in Philosophy* (Chicago and London, Open Court Publishing Co., 7*s.* 6*d.* net). It is only to be expected that, in the work just mentioned, we should have topics discussed which are so interesting to the logician and mathematician as infinity, continuity, and Zeno's puzzles about motion. In November 1914, Russell's "Herbert Spencer Lecture" on *Scientific Method in Philosophy* (Oxford, Clarendon Press, 1914, 1*s.* 6*d.* net) was

given at Oxford; and its position, like that of this book, was the showing the possibility and importance of applying to philosophical problems certain broad principles of method which have been found successful in the study of scientific questions. Other subjects closely connected with Russell's logical doctrines are discussed in many articles of his in the *Monist* for 1914 and 1915; that in the number for January 1914 is of particular interest to mathematicians.

Vol. xvii. of the international journal of scientific synthesis *Scientia*, published in Italy, contains an article by Eugenio Rignano in which mathematical symbolism is studied from a psychological point of view. In the third and last part of this article, mathematics and mathematical logic are compared from this point of view, and, as we should expect if we remember that the aims of symbolism in the two cases are almost exactly opposite and that psychologists hardly ever show any sympathy with or knowledge of the aims of logic, the comparison is not favourable to mathematical logic.

On the subject of Zeno's exceedingly subtle puzzles about motion, which concern the most vital conceptions of pure mathematics, the *American Mathematical Monthly* for the present year contains a very thorough study of the history of them through all times. This valuable piece of work is by Prof. Florian Cajori, and is the first detailed paper on the subject that has appeared.

We will now notice shortly some other work dealing chiefly with the history of mathematics and mathematical physics. The numbers of the *Mathematical Gazette* which have appeared during 1915 contain the final part of a translation of Prof. Gino Loria's address on "The Achievements of Great Britain in the Realm of Mathematics"; accounts by W. W. Rouse Ball of the work of (1) Pythagoras, (2) De Morgan; and the Presidential Address to the Mathematical Association by Sir George Greenhill on "Mathematics in Artillery Science." There are numerous short mathematical notes in this periodical, and the reviews of books are a special source of the *Gazette's* pride: they are nearly always good. Prof. E. W. Hobson published a series of short lectures on the history of investigations on π in *Squaring the Circle* (Cambridge University Press, 1913, 3s. net). An interesting *History of Japanese Mathematics* by D. E. Smith and Y. Mikami (Chicago and London, Open Court Publishing

Company, 12*s.* net) was published in 1914. It is particularly interesting to read of the approaches made by Japanese mathematics to the infinitesimal calculus, of which an embryonic form was known as "yenri"; and it may also be noticed that determinants were invented in Japan before Leibniz in Europe had any idea of them. In the *Monist* for 1914 and 1915 there have been several articles in which the development of the principles of mechanics with Newton and his contemporaries is treated in great detail, and the papers in question will be shortly collected and republished. The most interesting feature is an extract, in the number for October 1914, from a manuscript of Newton's which has been hitherto unpublished, showing that the important conception of "mass" developed much earlier with Newton than is generally supposed. The whole question of the principles of mechanics with Newton is extremely closely connected with Newton's mathematical work; and Newton's mathematical work is also considered in some detail in the annotated *Essays on the Life and Work of Newton* by Augustus De Morgan (Chicago and London, the Open Court Publishing Company, 5*s.* net), which has been already reviewed in this magazine (April 1915, pp. 687–9). A very valuable monograph on *Aristarchus of Samos: the Ancient Copernicus*, by Sir Thomas Heath, was published in 1913 by the Clarendon Press.

Vol. v. (1913) of the *Journal of the Indian Mathematical Society* contains a good address by Prof. G. A. Miller, given to the Illinois Society, and entitled "Some Thoughts on Modern Mathematical Research"; vol. vi. (1914) contains the first part of an historical and critical study of the various theories of irrational number by Philip E. B. Jourdain. In the volumes of this *Journal* are a great number of short notes, and there are not very many longer articles. In the current volume (vii.) there is a long paper on "So-called Cases of Failure in the Solution of Linear Differential Equations" by Eric H. Neville.

We will now consider more particularly some work on algebra and the theory of numbers. One of the mathematical papers in Section A of vol. xxxii. (1914) of the *Proceedings of the Royal Irish Academy* is by M. W. J. Fry, and bearing the title "Real and Complex Numbers Considered as Adjectives or Operators," carries our thoughts back to the first half of last century. The object of this paper is to define the symbols $+$, $-$, and i in such a way "that the rules to be followed in

using them may be obvious, and that what are called negative and imaginary solutions of problems may have as real and precise a meaning as those called positive." In vol. xvii. (1913) of the *Proceedings of the Cambridge Philosophical Society*, H. C. Pocklington discusses some Diophantine impossibilities, and Prof. A. C. Dixon gives a short proof that a determinant cannot exceed its leading term. In 1914 Prof. L. E. Dickson published an elementary introduction to the general theory of *Linear Algebras* (Cambridge University Press, 3*s.* net), including non-associative algebras. Among papers on algebra, we may notice Dr. J. Brownlee's (*Proc. Roy. Soc. Edinb.*, vol. xxxii. 1913) analysis of observations on inheritance of hair and eye colour. He finds that Mendel's laws are satisfied to a remarkable extent.

In analysis and the theory of functions the most important books recently published are Prof. A. R. Forsyth's *Lectures Introductory to the Theory of Functions of Two Complex Variables* (Cambridge University Press, 1914, 10*s.* net), delivered at Calcutta University in 1913; the second edition of G. H. Hardy's *Course of Pure Mathematics* (Cambridge University Press, 1914, 12*s.* net), which has several interesting additions to the first edition published in 1908; an English translation by S. E. Rasor of Dr. H. Burkhardt's admirable *Theory of Functions of a Complex Variable* (London: D. C. Heath & Co., 1914, 12*s.* 6*d.*); an able tract by G. N. Watson on *Complex Integration and Cauchy's Theorem* (Cambridge University Press, 1914, 3*s.* net); and a fourth edition of Forsyth's well-known *Treatise on Differential Equations* (Macmillan, 1914, 14*s.* net).

Every linear differential equation of the second order leads to a continued fraction, so that theorems involving the transformation of one such differential equation into another would be expected to lead to transformations of continued fractions hitherto unknown. A. Lindsay Ince (*Proc. Edinb. Math. Soc.*, vol. xxxii. 1914) uses a method depending on these facts to get certain theorems on continued fractions equivalent to Riemann's and other transformations of the *P*-function. A fairly large proportion of the papers in this volume is devoted to the subject of harmonic analysis. Of these, the most noteworthy seem to be papers by Prof. E. T. Whittaker on the calculation and investigation of the functions of Hermite and some other functions by continued fractions, and the solution of Mathieu's equation by introducing a new parameter and avoiding the use,

due to Hill, of infinite determinants. In the same volume Pierre Humbert shows how a well-known transformation of Laplace, which is of great help in finding the solutions of linear differential equations, gives also interesting results in the theory of integral equations.

In vol. xliv. (1914–15) of the *Messenger of Mathematics* there are, as usual, a number of short papers, most of which are decidedly interesting. Dr. J. W. L. Glaisher gives some new relations between sums of the reciprocals of powers of integers; S. Ramanujan and G. H. Hardy supply calculations of some definite integrals and work on other questions in the integral calculus; and there are various notes on pure and analytic geometry, the theory of potential, elimination, the theory of differential equations, and so on. Dr. Thomas Muir gives a collection of properties of a pair of orthogonants; and F. Jackson gives a simple method for proving that Laplace's integrals for the functions called $P_n(x)$ and $Q_n(x)$ satisfy Legendre's equation. In the *Quarterly Journal of Mathematics* (vol. xlvi. 1914), there are two papers on substitution groups by G. A. Miller, two papers connected with the generalised potential by C. E. Weatherburn, a paper by W. Woolsey Johnson on the history, computation, and tabulation of Cotesian numbers, a paper by S. A. Joffe on sums of like powers of natural numbers, which is closely connected with Dr. Glaisher's paper of 1899, a paper by Dr. J. R. Wilton on a point in the theory of partial differential equations, and a paper by Tomlinson Fort on periodic solutions of linear difference equations of the second order. Dr. J. R. Wilton has a paper on Darboux's method of solution of partial differential equations of the second order in vol. xiv. (1914–15) of the *Proceedings of the London Mathematical Society*; and the same volume also contains two important papers by Prof. E. W. Hobson on (1) the representation of the symmetrical nucleus of a linear integral equation, and (2) theorems relating to functions defined implicitly, with applications to the calculus of variations. The paper by Prof. W. H. and Mrs. Young is of great interest in connection with the "Heine-Borel" and analogous theorems. Prof. A. E. H. Love, in his Presidential Address printed here, considers certain qualities by which valuable mathematical research is characterised. The papers in these *Proceedings* are often so technical as not to admit of abstraction; they are practically all of the greatest value—the

system of referring a paper to expert referees ensures this in most cases.

Dr. D. M. Y. Sommerville (*Proc. Edinb. Math. Soc.*, vol. xxxii. 1914) discusses an error in one of Gauss's three proofs of the reciprocity of parallelism. This seems to indicate matters of historical interest as regards the order in which Gauss devised these proofs; and it is of some interest that neither Bonola nor his German translator noticed Gauss's fallacy, but Carslaw, the English translator of Bonola, did. In 1914 Dr. Sommerville published an excellent text-book on *The Elements of Non-Euclidean Geometry* (London: G. Bell & Sons, Ltd., 5*s*.). Also Sommerville (*Proc. Roy. Soc. Edinb.*, vol. xxxiv. 1914) gives a description of a projection-model of the figure bounded by 600 congruent regular tetrahedra in space of four dimensions. The volumes for 1914 and 1915 also contain various papers on determinants by Thomas Muir and by W. H. Metzler, and in the volume for 1915 Prof. E. T. Whittaker shows that the functions of Lamé are the solutions of a certain homogeneous integral-equation.

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Parallax Determinations.—Amongst the most remarkable of the recent advances in astronomical knowledge may be placed the rapid increase in the number of stars whose *parallax* (to use a technical word) is accurately known. The parallax of a star is the small angle by which it appears to be displaced from its mean position, due to the motion of the Earth in its orbit; and if this angle can be accurately measured the distance of a star can be calculated, since the radius of the Earth's orbit is known. A knowledge of the parallax of a star is of great value, because it enables the proper motion, or apparent angular motion of the star in the sky, to be converted into an actual velocity of so many kilometres per second, and also its apparent brightness to be converted into an intrinsic luminosity relative to the Sun. The difficulty of the determination of stellar parallaxes lies in the fact that the stars are at such immense distances, that the angle to be measured is extremely small—of the order of a few hundredths or thousandths of a second of arc, a second of arc being very nearly equal to the angle subtended by a halfpenny at a distance of three miles. Very careful pre-

cautions must therefore be taken to obviate all possible sources of error. The problem of the "Measurement of the Distances of the Stars" was chosen by Sir F. W. Dyson, Astronomer Royal, as the subject for the Halley Lecture delivered by him in the University of Oxford on May 20 last (reprinted in the *Observatory*, vol. xxxviii. No. 488); this forms a valuable account of the difficulties attendant upon this problem, and of the manner in which they have gradually been overcome. It was not until 1838 that the parallax of any star was determined with any certainty, but in that year the parallaxes of three stars were obtained by three different astronomers: that of α Centauri by Henderson, from meridian observations made at the Cape; that of 61 Cygni by Bessel, using a heliometer; and of α Lyræ by Struve, using a filar micrometer. The introduction of the heliometer by Bessel was a considerable advance, and until the application of photography this remained the best method for the determination of parallaxes. Its disadvantage was that the observations were laborious, and very great skill was necessary in the manipulation of the instrument. However, in the hands of Gill and Elkin at the Cape, of Elkin, Chase and Smith at Yale, and of Peter at Leipzig, a number of valuable results were obtained by means of it.

The recent rapid advances have been due to the introduction and perfecting of the photographic method, by which, with a few simple precautions, results can be obtained of an accuracy equal or superior to that reached in the best heliometer observations, and with much less trouble. In 1910, Schlesinger published the parallaxes of twenty-five stars from photographs taken with the 40-inch refractor at Yerkes, and he concluded that "the number of stellar parallaxes that can be determined per annum will, in the long run, be about equal to the number of clear nights available for the work." This statement shows the possibilities of the photographic method. During the past year, the parallaxes of over fifty stars, also determined at the Yerkes observatory, have been published by Slocum and Mitchell (*Astr. Nachr.*, Nos. 4709 and 4760), which are of the same high order of accuracy. The latest contribution to our knowledge comprises the parallaxes of about forty stars determined at the Royal Observatory, Greenwich (*Monthly Notices, R.A.S.*, 1915, June), and equal in accuracy to the Yerkes results, the mean probable error being $\pm 0''\cdot009$. These latter results

have been obtained in the manner proposed by Kapteryn, the photographic plate being exposed at the epoch when the parallax factor is a maximum, then put away without being developed, and re-exposed after an interval of six months when its parallax factor is a minimum. This method has some advantages over the "single plate" method used at Yerkes, in which separate plates are exposed at the two epochs and developed immediately. The change in the position of the parallax star relatively to the much fainter neighbouring stars is measured in each case. The rapid advance in our knowledge was summed up by Sir F. W. Dyson thus: "We may expect that the number of stars whose distances are fairly well known will soon amount to thousands, as compared with 3 in 1838, about 20 in 1880, about 60 in 1900, and now perhaps 200."

The stars whose parallaxes have been sought have mostly been either bright stars or stars with large proper-motion, the probability of obtaining an appreciable parallax being largest for such stars, which are likely, on the average, to be the nearest to us. The Greenwich observing programme includes all stars within 35° of the Pole known to have a proper-motion greater than 20″ per century. The determination of stellar distances may be said now to have been placed upon so sure a footing that it has been felt desirable to organise a scheme of co-operation between the various observatories who are devoting much time to this work (these include Yerkes, Swarthmore, Dearborn, McCormick and Allegheny in America, Greenwich and, later, others in Europe) so as to avoid unconscious duplication. A committee has been formed for this purpose with F. Schlesinger, Director of the Allegheny Observatory, as chairman. The large number of accurate results which will soon be available will form very valuable material for many astronomical investigations.

PHYSICS. By James Rice, M.A., University, Liverpool.

Recent Work on Atom-Structure.—At present two fairly definite atomic models are being subjected to theoretical discussion and experimental test by scientific workers. There is the Thomson model, in which the atom is conceived as a sphere of positive electricity in which negatively charged electrons rotate in coplanar rings about the centre of the sphere. There is also the Rutherford model, in which the electrically

positive part is not diffused throughout the volume of the atom, but is highly concentrated in a nucleus which acts as a centre of force, attracting, according to the inverse square law, the electrons which are rotating around it in rings. The advantage of the Thomson atom is that its stability is assured on the basis of ordinary dynamical theory. As regards the Rutherford atom, it can be shown that, although the electrons are in stable rotation so long as disturbances are at right angles to the orbital plane, this is not in general the case for disturbances in the plane of the orbits. However, the difficulty of reconciling the Thomson model with large-angle scattering of α-rays has led to a considerable discussion of the Rutherford atom on account of the ease with which its structure accounts for such large-angle scattering. The question of its dynamical stability has recently been treated from a new point of view by Dr. Bohr, who, by an application of the Quantum Theory of Radiation, has indicated an avenue of discussion and research along which workers may proceed, and possibly succeed in formulating dynamical principles still more fundamental than the Newtonian, and capable of summarising not only our knowledge concerning the large-scale motions treated in ordinary mechanics, but also our gradually increasing information about such small-scale motions as are involved in the electronic orbits of atoms. On this account a good deal of attention is being paid at present to the Rutherford-Bohr model.

Both models, of course, have rings of electrons as a common postulate; the number of electrons is closely connected with the atomic weight; on one view it is identified with the atomic number of the element in the periodic series. Many ingenious explanations have been forthcoming seeking to correlate the numbers of the electrons, the size and number of the rings, and their rate of rotation with chemical properties and with the line spectra and characteristic X-ray radiation of the element considered. In a recent paper (*Proc. Phys. Soc.*, December 1914) Prof. Thomson points out that the characteristic Röntgen radiations with which we are now experimentally acquainted (usually called K and L radiations) may be identified as due to vibration of the two rings of electrons nearest the centre of the atom, whilst light of the visible spectrum is due to the outermost ring. The properties of these rings may therefore be studied by making use of Röntgen radiation ranging from the

hardest which we can produce down to that characteristic of aluminium on the one hand, and by using visible and ultra-violet light on the other. He further shows that the existence of rings intermediate between the outermost and the K and L rings must be detected and their properties studied by the use of radiation of wave-length between that of the K and L radiations and that of the highest ultra-violet yet obtained, that is radiation in a gap of about eight octaves between $3{\cdot}6 \times 10^{-8}$ cm. and 9×10^{-6} cm. This paper then proceeds to describe attempts which he has made to produce such extremely soft Röntgen radiation by the impact of positive rays and slow cathode rays on a platinum plate. After taking all possible precautions to eliminate spurious effects, he has succeeded in detecting such radiation by its action on a photographic plate. He finds that even the thinnest films of such substances as collodion, mica, wax, aluminium are opaque to it, and he suggests how the ordinary theory of dispersion may be adapted to explain this opacity; that, in fact, an atom will scatter strongly, and therefore be impervious to, rays whose wave-lengths lie between limits which depend upon the density with which the electrons are packed within the atom, and the natural periods of these electrons.

Two very interesting letters from Prof. Barkla to *Nature* (February 18 and March 4, 1915) deal with the relations of X-radiation and corpuscular radiation to the K and L rings. Prof. Barkla supplies experimental evidence favouring Bohr's theory, and states that further experiments are in progress to elucidate some points in connection with the ejection of electrons from the atom produced by those disturbances of the rings which also produce the radiation.

Accounts have been published recently of experimental work connected with series spectra which is crucial as regards the validity of Bohr's Theory. According to Bohr's Theory, certain spectral lines which have generally been attributed to hydrogen ought to be attributed to helium, and on his view also certain other lines previously unobserved should make their appearance in the spectrum of helium. In the February *Phil. Mag.* there is an account of some experiments by Mr. Evans which to some extent justifies these conclusions. Mr. Merton, however, in a letter to *Nature*, March 18, combats this view, and while maintaining that Mr. Evans's experimental work is not really

sufficient to establish the origin of the lines to be helium, refers briefly to some work of his own, now in progress, which would allocate some of these lines to hydrogen, and so support the traditional view.

INORGANIC CHEMISTRY. By C. Scott Garrett, B.Sc., University, Liverpool.

Owing to the present great crisis in the affairs of the European nations it is only to be expected that the world's output of purely scientific communications will in the meantime be considerably curtailed. This state of things is unhappily being realised at the present juncture, for, with a notable exception in the case of America, the energies of men of science are being more and more diverted from their accustomed channels into those of direct national utility. It is for these reasons that we are unable to record any very striking advances in the domain of pure inorganic chemistry during the past quarter.

Of the work emanating from American schools that dealing with the atomic weight of lead is probably first in point of importance. In view of the extensive new field opened up by Soddy's recent work on the end-products of the radioactive degradation of the elements in which lead figures very largely, and which has enabled him as well as Fajans to propound the theory of Isotopism, this work of Baxter and his colleagues makes a very timely appearance (*Jour. Am. Chem. Soc.* 1915, **37**, 1020).

The investigations, carried out with all the precision and refinement of the Harvard school, have resulted in an even value for the atomic weight of common lead amounting to 207·20 if Ag = 107·880, or 207·18 if Ag = 107·870.

Eleven different mineralogical sources, as wide apart as Australia, Germany, and the United States, provided the starting material, and the different specimens of lead obtained from the minerals, when converted into chloride or bromide, all gave identical spectra. Further, no appreciable radioactivity could be detected in any of the specimens, and the authors conclude that there is no evidence that common lead contains, either wholly or in part, isotopes of different atomic weight. It should be noted, however, that while this work increases slightly the generally accepted value for the atomic weight of

lead, it in no way alters the validity of the high value found by Richards and Lambert for lead from definitely radioactive minerals.

In the same school the atomic weights of cadmium, praseodymium, carbon, and sulphur have also been the subjects of revisory investigations.

An electrolytic determination of cadmium in cadmium chloride led to a slightly higher value than the present international figure, and a value 0·3 higher than the accepted value was obtained for praseodymium as a result of the analysis of the pure chloride. Prof. Richards (*Jour. Am. Chem. Soc.* 1915, **37**, 95) has obtained the value 12·000 for carbon (Ag = 107·880, Na = 22·995), as compared with the standard value 12·005, by means of the reactions between sodium carbonate and hydrobromic acid on the one hand, and between hydrobromic acid and silver nitrate on the other. A value 32·065 for sulphur, which he regards as the most trustworthy value hitherto recorded, was obtained by means of the reaction between sodium carbonate and sulphuric acid.

The extended search which is being made by the Dutch school of chemists, led by Cohen, for the presence of allotropic modifications in metals hitherto unsuspected of possessing this property has been extended to potassium. From observations (*Kgl. Akad. Wet. Amst.* 1915, **17**, 1115) on its coefficient of expansion, it is concluded that a modification, β potassium, is present in ordinary samples of the metal. The new modification passes into the ordinary form at about 59°·5 C., so that the metal as it is commonly encountered must be regarded as metastable. Advance in a similar field is due to Angel (*Bull. Soc. Chim.* 1915, **17**, 10), who has succeeded in producing a new violet-grey form of selenium by rapid cooling of the vitreous variety after heating to 220° C. This new form possesses a remarkable photoelectric sensitivity, but, as might be expected, is very unstable.

Of exceptional interest is the thorough though fruitless search for an unknown alkali metal to fill the vacant place in the Periodic Table below cæsium. Prolonged fractional crystallisation of a large quantity of cæsium nitrate obtained from the mineral polluxite (Baxter, *Jour. Am. Chem. Soc.* 1915, **37**, 286) failed to give any indications of the presence of an allied metal of higher atomic weight in the least soluble fractions.

From the few papers on inorganic chemistry appearing in

the British journals, that of Chattaway on the preparation of perhaloid derivatives of ammonium may be selected as of general interest (*Trans. Chem. Soc.* 1915, **107**, 105). Under suitable conditions the addition of a chlorine and an iodine atom to ammonium chloride can be brought about, giving scarlet prisms possessing the formula NH_4Cl_2I. Other compounds, such as NH_4Cl_4I (golden-yellow prisms) and NH_4ClBrI (garnet-red prisms), were prepared in the solid state. These compounds appear to be fairly stable; they can be heated to 200° C. without decomposition, and are not attacked by water, in which they are readily soluble. It is noteworthy that the introduction of the extra halogen atoms at the same time brings about the phenomenon of colour.

Quite a remarkable advance in our ideas on the chemical reactivity of iodine is the result of the recent work of Kappeler and Fichter on the formation of iodine salts. Considering the halogen family of elements, it might have been expected that if metallic properties were exhibited at all by this typically acidic natural group, they would appear in iodine as the heaviest member of the group. This has now been realised, and in the latest paper dealing with the subject (*Zeit. anorg. Chem.* 1915, **91**, 134) such compounds as iodine sulphate, $I_2(SO_4)_3$, iodine perchlorate, $I(ClO_4)_3 . 2H_2O$, and iodine periodate, $I(IO_4)_3$ are described, in which the versatile iodine atom functions as a trivalent metallic radical.

The extensive work of Franklin and his collaborators on reactions in liquid ammonia solutions have been further extended in several papers published in the *Proceedings of the National Academy, U.S.A.* This field of work has been extremely fruitful in producing long series of inorganic compounds in which the amino- and imino-groups function largely. The work has now been extended to include several organic derivatives, and may in future lead to important methods for the production of amino-acids.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School.

The working out of processes for the synthetic production of compounds from new or unfamiliar sources is always to be regarded as a valuable contribution to science, but at the present time, when the customary sources of supply of any substances

are liable to be cut off, such processes may become of paramount importance. Examined in this light, the patents recently taken out by German firms for the synthesis of acetic acid from acetylene (*J. Soc. Chem. Ind.* 1914, **33**, 830) and from acetic aldehyde (*loc. cit.*) acquire a special interest. Thus it is claimed that if acetylene is passed into a mixture of 250 parts of 30 per cent. sulphuric acid with 100 parts of 95 per cent. ammonium persulphate and 5 to 10 parts of mercuric oxide at 30 to 40° C. from 24 to 25 parts of acetic acid are obtained. A similar change may be effected by anodic oxidation of acetylene if this gas is conducted into the anodic compartment of an electrolytic cell filled with 30 per cent. sulphuric acid containing 1 to 2 per cent. of mercuric oxide. On the other hand the oxidation of acetic aldehyde to acetic acid is found to be catalytically accelerated by manganese acetate; thus a current of dry oxygen passed into 300 kilos of pure acetic aldehyde containing 2 kilos of manganese acetate is absorbed so energetically that external cooling has to be applied. According to yet another patent (*loc. cit.* p. 961) acetic acid may be obtained in a yield of 95 per cent. of the theoretical by passing oxygen under pressure of about 2 atmospheres into acetic aldehyde containing 1 per cent. of ceric oxide, and cooling the mixture when the temperature has risen to 50 to 60°.

An investigation of some topical interest is one by Wilcox[1] upon the physiological effect of tetrachlorethane or acetylene tetrachloride, one of those highly chlorinated compounds which are less familiar to the scientific chemist than to the works chemist. This substance, which is also known by the trade name of Cellon, forms, owing to its solvent action on cellulose acetate, an important constituent of "dope" used for coating the wings of aeroplanes. During last September and October a number of workmen engaged in the manufacture of aeroplanes were taken ill with various symptoms of general malaise and drowsiness, followed by more or less severe toxic jaundice. This condition has been proved from experiments on animals to be due to tetrachlorethane, which is a powerful poison to the liver and kidneys; the Home Office have now laid down regulations for ensuring the proper ventilation of the workrooms and the efficient removal of the toxic vapours. At a time when so much is being said and written about the

[1] A paper read before the Medical Society of London, March 1, 1915.

manufacture of dye-stuffs in this country, an historical *resumé* of the discovery of all the more important dyes by such an eminent authority as Prof. Noelting (*Arch. sci. phys. nat. Genève*, 1914 [iv], **38**, 244 and 337) makes very interesting reading. According to him the number of dyes which have been synthesised runs into tens of thousands if not hundreds of thousands, by far the greater number of which are of course not used industrially; but it is no exaggeration to say that the number of definite individuals actually on the market extends to more than 2,000. We are once more told the old tale that the aniline-dye industry originated in England, partly from the researches of Perkin and partly from the researches of German chemists, such as Hoffmann, Caro, and Martius, working in this country. Much of the earlier work was also done in France by both French and Alsatian chemists; yet, in spite of these facts, by far the greater proportion of dyes are manufactured in Germany and Switzerland, which latter country, however, produces only about one-tenth the amount that Germany produces. The reasons for the pre-eminence of these two countries in the manufacture of dyes is, according to the author, easy to comprehend. He says: "Nowhere has organic chemistry been held in greater esteem than in these two countries—nowhere have young men found such facilities for study, both theoretical and practical, which has enabled them to apply their knowledge industrially. Nowhere have the manufacturers shown such intelligence, breadth of outlook and enterprise, shirking no expense and considering no difficulty as insurmountable, and leaving no question without having exhausted it—'nil actum reputans dum quid superesset agendum'—or, in other words, considering nothing completed so long as something remained to be done." These views, coming from such an expert as Prof. Noelting, are of considerable interest, as it will be seen that he nowhere suggests that the success of foreign countries is due to any innate gift or peculiarities of disposition or even originality, but attributes success merely to facilities for study and business enterprise—a fact which should give heart to those in this country who are anxious to reorganise matters with a view of putting our chemical industries on a better footing.

Before concluding this review, the attention of English readers should be drawn to an article by Willstätter on

"Chlorophyll," in the *Journal of the American Chemical Society*, 1915, 37, 323. Those who wish to obtain first-hand knowledge on this very complicated subject would do well to read through this review.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., F.G.S., University, Glasgow.

A. HOLMES has written an important paper on Radioactivity and the Earth's Thermal History (*Geol. Mag.*, February and March 1915). The speculation that the radioactive elements are concentrated towards the surface of the earth's crust is supported by their known distribution amongst the various igneous rock types, and the variation of these types in depth. The average content of radioactive elements is greater in acid rocks than in the basic, and these in turn are richer than stony and iron meteorites, which may be supposed to represent the material of the interior of the earth. The strongly supported opinion that the igneous material of the crust is stratified in order of density then necessitates the conclusion that the radioactive elements are concentrated towards the surface. This conclusion is supported by several other lines of evidence. A further conclusion that there is nothing in the distribution of radioactive elements to forbid belief in an earth which began with a molten surface, and which has gradually cooled down to its present condition, revives a view which has been temporarily pushed aside by the planetesimal hypothesis of the origin of the earth.

In his Presidential Address to the Geological Society of America (*Bulletin*, April 1915), Dr. G. F. Becker writes on the relation of Isostasy and Radioactivity. He states that the geodetic evidence for isostasy is so manifold and consistent as to amount to proof. He believes that the level of compensation, estimated by geodesists at between 110 and 140 kms. from the surface, is incompatible with the immense age for the earth as estimated on the basis of the uranium-helium and uranium-lead ratios in certain minerals.

Since the broader lines of stratigraphy have been laid down in nearly all areas, this branch of geology necessarily tends now to the accumulation of local details, which are naturally only of local interest; and it is comparatively rare for new principles or ideas of general interest to emerge from the very numerous papers on the subject. Hence in these short

notes stratigraphical papers will only be mentioned when they deal with points of especial geological interest.

The *Uintacrinus* band of the Upper Chalk has been identified by Mr. R. M. Brydone in the Brighton cliffs (*Geol. Mag.*, January 1915).

The Survey Memoir on Sheet 74 of Scotland describes an area of 432 square miles in mid-Strathspey and Strathdearn. The country rock consists mainly of various phases of the Moine Gneiss, which is intruded by three great granite masses, those of the Cairngorm Mountains, the Monadhliath Mountains, and of Strathdearn. These apparently separate intrusions are believed to be continuous under a thin roof of schist, a view supported by the petrographical identity of the granites and the extensive granitisation of the intervening schists. The area presents a magnificent range of glacial phenomena, which are illustrated by the six fine plates that ornament this excellent memoir.

The first quarter of the year seems to have been unusually prolific in important petrological memoirs. Mr. Sargent (*Geol. Mag.*, January 1915) describes the Penmaenmawr intrusions as "bronzite-porphyrite," thus reviving an old field name which is gradually falling into desuetude as modern petrography gains in precision. To judge from the number of different names given to these rocks by petrographers they must be of puzzling character. To the reviewer it seems that they closely resemble the quartz-dolerites of the great east and west dykes of North Britain and their accompanying intrusions, a conclusion supported by the occasional presence of micropegmatitic material in the ground-mass, and by the great number of light-coloured segregation veins which also carry micropegmatite.

S. Powers (*Jour. Geol.*, January to March 1915) has collected a great number of cases of inclusions in dykes. On the whole these are rare, and are due to the shattering of the fissure-walls by mechanical agencies. Inclusions may rise, sink, or remain stationary when included in the magma. The direction and degree of movement appear to depend as much on the mechanics of intrusion and the accidents of magmatic circulation as on the density of the inclusions relative to that of the magma.

S. R. Capps (*Jour. Geol.*, January and February 1915) describes excellent examples of pillow-lavas, probably of Mesozoic

age, on Prince William Sound, Alaska, and adduces evidence which conclusively proves that these lavas were subaqueous flows. Each flow has a flat base, with an abundance of mud-filled cracks.

In the *Bulletin* of the Geological Society of America for December 1914, J. V. Lewis reviews all known occurrences of pillow-lavas, and compares the structure with the lava structures known as "pahoehoe" and "aa." He propounds a theory of "bulbous budding" in explanation of the origin of the curious pillow or sack-like forms of these lavas. In the declining stages of a large flow a lava breaks up into a multitude of small flows, each of which forms an elongated bulbous mass. On exposure to the atmosphere a tough elastic skin of solidified lava forms on each of these masses. The further expansion of the lava within produces the concentric flow structures frequently seen in the "pillows." Continued expansion may rupture the crust and give rise to another little pillow-form extravasation. The flow of a lava may thus continue through a series of pillows connected by very short necks, which are indeed seen in many occurrences. This theory seems to explain satisfactorily most of the peculiar features of these flows.

A most important piece of experimental work by N. L. Bowen ("Crystallisation-Differentiation in Silicate Liquids," *Amer. Jour. Science*, February 1915) demonstrates the effectiveness of gravity-separation of crystals in silicate melts, and furnishes strong evidence in favour of the process of gravity-differentiation as a factor in producing the observed diversity of igneous rocks. Olivine crystals were produced in a molten glass at a temperature of 1,460° C., contained in a crucible 15 mm. in depth. After 80 minutes all the olivine crystals were collected in a layer 1·5 mm. thick at the bottom of the crucible. Similar results were obtained for pyroxene crystals, but tridymite, when formed, tended to rise in the melts. These results are applied to the case of an olivine-rich layer in the quartz-diabase sill of the Palisades of the Hudson River, and it is concluded that the accumulation of the olivine crystals may have been accomplished in 200 to 300 hours. This experimental demonstration of sinking of crystals is welcome in view of the many recently described cases of ultrabasic layers in igneous rocks which have been ascribed to some form of gravity-differentiation.

Dr. Leigh Fermor (*Geol. Mag.*, January and February 1915)

contributes useful notes on Prof. Lacroix's great memoir on the Laterites of French Guinea (*Nouv. Arch. du Mus.*, 1914). Prof. Lacroix had the advantage of examining a large number of fresh railway sections showing the complete passage from the underlying unaltered rock, through the various stages of laterisation, to the hard surface crust or *cuirasse*. With regard to the origin of laterite he comes to the conclusion that it is not due to the direct attack of the atmosphere or surface-water, but that the process of laterisation is intense wherever the slope of the ground is such as to permit the infiltration of water and allow it to remain for a long time in contact with the rocks. The reactions of which laterite is the outcome occur with greater intensity in tropical countries because of special climatic conditions, but go on, although not to the same degree, under other climates. This, of course, accounts for the distribution of laterite only within or near tropical regions.

The closely related substance, bauxite, is dealt with by W. J. Mead in a paper on "The Bauxite Deposits of Arkansas" (*Economic Geology*, January 1915). The bauxite has been developed by the surface weathering of a nepheline-syenite, and occurs either *in situ* or as lenses intercalated with Tertiary sediments, and deposited by Tertiary streams. The open texture of the rock produced by the decomposition of nepheline is held to be essential to the change of the kaolinised felspar of the syenite into bauxite. It is also shown that alumina is somewhat concentrated towards the base of the deposits by solution of the bauxite and its subsequent redeposition at a lower level.

BOTANY. By PROF. F. CAVERS, D.Sc., Goldsmiths' College, London.

IN plant physiology, Prof. H. H. Dixon and his collaborators have published further work on the various problems connected with ascent of sap in trees (*Sci. Proc. Roy. Dublin Soc.* **15**). By determining (by the method of weighing the parts of cut-out photomicrographs) the percentage of the total cross-section of the wood occupied respectively by vessels, fibres, cells, walls, etc., they claim to have disproved the view that protoplasmic streaming in the living wood-cells may cause the transpiration current. They find also that large amounts of sugars are present in the transpiration stream, their concentration being greater than that of the electrolytes present, and suggest that the distribution of carbohydrates is a function of transpiration no less

important than the conveyance of nutritive salts, also that the living cells around the vessels form a glandular sheath for secreting carbohydrates into the stream, the medullary rays serving to store these and convey them to the sheaths. They also give the results of cryoscopic and conductivity measurements of sap pressed from the leaves, roots, etc., of trees after treatment with liquid air, at intervals through the year, and find that the osmotic pressure is mainly due to dissolved carbohydrates, the concentration in deciduous leaves rising from bud-opening to a midsummer maximum, while in evergreen leaves the highest pressures occur in winter. Stiles (*Ann. of Bot.* **29**) claims to have disproved the view that the concentration of the nutrient solution greatly influences the rate of growth of plants, and finds that variation over a fairly wide range of concentration has relatively little effect on the amount of dry matter produced in rye and barley, also that plants grow quite healthily in a culture of even lower concentration than that attributed to the soil solution by Cameron. Osterhout has published two further papers on protoplasmic permeability, using the method by which he had previously shown that determinations of the electric resistance of living tissues afford an accurate measure of permeability. He finds (*Bot. Gaz.* **59**; *Amer. Journ. of Bot.* **2**) that permeability of protoplasm may be greatly increased or diminished without injury, none being produced even with rapid alternation of 20 per cent. above and 40 per cent. below normal, also that while NaCl and other salts of monovalent metals (and indeed all the monovalent kations except H) increase permeability, all the bivalent kations investigated cause a marked decrease. Ehlers (*Amer. Journ. of Bot.* **2**) finds that evergreen conifer leaves in winter maintain temperatures from 2° to 10° C. higher than the surrounding air, relates his observations to data regarding photosynthesis at low temperatures, and suggests that in them lies an explanation of the accumulation of winter-manufactured reserve food material by evergreen trees. Two interesting papers on "symbiosis" may be mentioned here, both in *Ann. of Bot.* **29**. Rayner's valuable work on the mycorhiza of *Calluna* (heather) has shown that we have here the first known case of a green plant whose green as well as non-chlorophyllous tissues harbour a symbiotic fungus; a similar distribution of fungus mycelium, with ovarial infection of the seeds, occurs in the darnel-grass, but this plant does not form mycorhiza nor has

the relation with the fungus been shown to be obligate as in *Calluna*. Baden has shown that the spores of an agaric (*Coprinus* sp.) are incapable of germinating unless accompanied by bacteria—the first recorded case, outside of the Myxomycetes (Mycetozoa), of this remarkable and as yet unexplained phenomenon.

Selecting only such anatomical investigations as appear of special interest in bearing upon the phylogeny (relationships) of plants and plant-groups, the following may be noted. Kashyap (*New Phyt.* **14**), in the third of his series of papers on new and little-known West Himalayan liverworts, describes among other new forms a new genus *Sewardiella* which is related to *Fossombronia*, but not differentiated into stem and leaves, and suggests the somewhat heterodox view that leafy forms may have given rise by reduction to non-leafy (thalloid) ones among the liverworts. Bryan (*Bot. Gaz.* **59**) describes in great detail the development of the female organ (archegonium) of the bog-moss (*Sphagnum*) and finds that the apical-cell growth hitherto regarded as distinguishing this organ in mosses does not occur in *Sphagnum*; his results confirm Cavers' conclusion from study of other forms that the usually accepted division of the sub-kingdom Bryophyta into two primary classes (liverworts and mosses) cannot longer be maintained. Hodgetts (*New Phyt.* **14**) describes the occurrence, as outgrowths from the stem, of flattened juvenile stages (protonema) in the moss *Tetraphis*, these being exactly similar to the protonema arising from the germinating spore in this moss and affording additional support for the new classification of the Bryophyta proposed by Cavers. Lang has added to his previous elaborate studies of the morphology and anatomy of the fern family Ophioglossaceæ a detailed paper on *Helminthostachys* (*Ann. of Bot.* **29**); his earlier conclusion, that this family is related to the ancient fern stock including the relatively primitive fossil and recent fern groups, is strengthened by the results here presented. Burlinghame (*Bot. Gaz.* **59**) has added to his earlier studies of the interesting monkey-puzzle genus (*Araucaria*) a paper dealing with fertilisation, embryo and seed; his main conclusions are that the seed structure and pollination apparatus of the Araucarians could readily be derived from the type of seeds or ovules represented by fossil Lycopods such as *Miadesmia*, but not from an Abietinean (pine) stock, and that modern Conifers may have arisen from a Mesozoic stock

with Araucarian ovules and pollination apparatus. Sinnott and Bailey, in the fifth paper of their series on the phylogeny of Angiosperms (*Amer. Journ. of Bot.* 2), deal with the evidence afforded by the leaf as to the ancestry and early climatic environment of the Angiosperms, bringing together, as in their earlier papers, a large mass of data and concluding that the primitive Angiosperm leaf was palmate and provided with three main bundles arising separately from the stem-node, that transitions from the palmate type to all other leaf-forms can be readily traced, and that the Angiosperms probably sprang from a coniferous (palmate) rather than a cycadean (pinnate) stock and first appeared in a climate more temperate than tropical—a climate which in the Mesozoic was doubtless found only in the uplands, a fact which would explain the scarcity of fossil Angiosperms in rocks of that age.

In ecology we may note three papers dealing with the vegetation of high altitudes. Rydberg (*Bull. Torrey Bot. Club* 42) describes the forests of the subalpine and alpine zones of the Rocky Mountains; Smiley (*Bot. Gaz.* 59) the alpine and subalpine vegetation of the Lake Tahoe region, Nevada; and Gates (*Journ. of Ecol.* 3) the somewhat unusual occurrence of a typical and well-developed *Sphagnum* bog in the tropics, in this case on the summit of Mt. San Cristobal, Philippine Islands.

ZOOLOGY. By C. H. O'Donoghue, D.Sc., University College, London.

Protozoa.—One of the most interesting and useful contributions to zoology during the quarter under review is undoubtedly the paper by Minchin and Thomson on "The Rat Trypanosome, *Trypanosoma lewisi*, in its Relation to the Rat Flea, *Ceratophyllus fasciatus*" (*Quart. Jour. Micro. Sci.*, vol. 60, pt. 4, January). It is the result of five years' study on the part of the authors, and in it we have an account of the life history of a Trypanosome very nearly as complete as that of the malarial parasite. The importance of this is obvious. The problems have been attacked from the experimental as well as the morphological point of view, and it is interesting to note that the "direct transmission" of the parasite does not occur, but that, on the other hand, it is necessary for it to undergo a developmental cycle in the alimentary canal of the flea before the flea can infect another host. Smith (*Ann. and Mag.* No. 87) treats of the Forams in the Upper Silurian formations of Gothland.

Invertebrata.—"A New Freshwater Medusa from the Limpopo River System" is recorded by Arnold and Boulenger (*Proc. Zool. Soc.*). Before the same Society papers have been read on: Cestoda, by Beddard, continuing his previous studies by observations on the genera *Amabilia* and *Dasyurotænia*; new Pentastomids obtained from the Society's gardens in a useful paper by Hett; the adaptation of the ciliary mechanism to nutrition in certain asterids and examples of *Porania pulvillus* with actinially placed gills, by Gemmil. Certain annelids form the subject of "Notes from the Gatty Marine Laboratory" by M'Intosh, and Pryde records the polychætes obtained by the *Goldseeker* (*Ann. and Mag.* Nos. 85 and 86).

The Insecta as usual form the subject of a series of papers. Cummings records two new species of lice (Polyplax) (*Proc. Zool. Soc.*), and deals with their mouth parts (*Ann. and Mag.* No. 86). Turner describes Australian Fossorial wasps (*Proc. Zool. Soc.*), and also Australian and Tasmanian species (*Ann. and Mag.* No. 85). Dragon-flies from Borneo are treated by Laidlaw (*Proc. Zool. Soc.*), and from Sierra Leone by Ris (*Ann. and Mag.* No. 86). The Coleoptera, Diptera, Odonata, and Vermes collected by the Wollaston expedition to Dutch Guinea have been worked out by different authorities (*Proc. Zool. Soc.*). Ants from Australia, Christmas Islands, Straits Settlements, are described by Crawley (*Ann. and Mag.* Nos. 85 and 86). *Delias* forms the subject of notes by Rothschild and Joicey and Noakes (*Ann. and Mag.* No. 85), the last authorities also dealing with a new Ornithoptera from New Guinea. The characters and relationships of British species of Haliplus are given by Balfour-Browne (*Ann. and Mag.* No. 85). Bees are dealt with by Cokerell, and those in the British Museum collection by Meade-Waldo (*Ann. and Mag.* No. 87). In the *Annals and Magazine of Natural History* a new Indian scorpion, *Charmus indicus*, is recorded by Hirst (No. 86), new species of Thysanoptera by Bagnall (No. 87), new species of Heterocera from Dutch New Guinea by Joicey and Talbot (No. 87), notes on Carides by Borradaile (No. 86), and Australian Tabanidæ by Ricardo (No. 87). Hogson gives a preliminary account of the Pycnogonida obtained by the "Gauss" in the Antarctic, and Calman discusses the Holotype of *Ammothea carolinensis* (*Ann. and Mag.* Nos. 85 and 87). Early stages in one of the net-winged midges, *Paltostoma schineri*, are described by Scott, and

the female of the same species by Lamb (*Ann. and Mag.* No. 86).

Vertebrata.—Collections of fishes are described, one of fresh-water species from Sierra Leone by Boulenger, and one from Lagos by Regan (*Ann. and Mag.* Nos. 86 and 85). Thomas has a series of papers on new species of *Emballonura*, bats of the genera *Nyctalus*, *Tylonycteris*, and *Pipistrellus*, new species of Lencouve, and a new shrew, *Blarinella wardi* (*Ann. and Mag.* Nos. 85, 86, and 87). Dollman revises the East African Swamp-Rats, otomys (*Ann. and Mag.* No. 85).

Lydekker in a paper investigates the problem of "The True Coracoid," and homologises the bone of that name in birds and post-Triassic reptiles with the coracoid process in man and the coracoid of Monotremes (*Proc. Zool. Soc.*). The feet and glands of Vivirrinæ are described by Pocock (*Proc. Zool. Soc.*). Degeneration in the teeth of oxen and sheep is dealt with by Jackson (*Ann. and Mag.* No. 87).

A preliminary notice of a new longirostral mastodon is given by Barbour, and Gardner discusses the relation of the late Tertiary faunas of the Yorktown and Duplin formations (*Amer. Jour. Sci.*). South Africa has provided us with many valuable fossil forms, and Haughton has continued his "Investigations in South African Fossil Reptiles and Amphibia" by a series of four papers (*Annals of S. A. Museum*, January 1915), in which new species of Tremalosaurus, Dinocephalia, Therocephalia, and Anomodontia are described.

Cunningham describes certain rather remarkable forms that may be produced by pouring molten paraffin wax on to water, and bear some resemblance to the shells of molluscs.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc., Public Museum, Gloucester.

LAST summer yet another lower jaw of the Neandertal type was discovered, and the specimen was to have been exhibited to a congress of anthropologists at Hildesheim in August, but the congress had to be abandoned owing to the war. Descriptions of the fossil have, however, now been published. An account has been written by Prof. G. Schwalbe of Strassburg (*Anat. Anzeiger*, vol. 47, p. 337, 1914), and a short but excellent article on the subject, with illustrations, is contributed to the *American Anthropologist* (vol. 17, No. 1, January to March 1915),

by G. G. Maccurdy of Yale. The mandible was found at Ehringsdorf, near Weimar, a locality long famous for relics of ancient man, and the bone is now in the Weimar Museum. The jaw adds somewhat to our meagre knowledge of the range of variation in *Homo neandertalensis*. The left ascending ramus and the upper half of the right ascending ramus are missing, but the lower portion of the jaw is nearly perfect, all the teeth except the two right incisors being present. The absence of a chin-prominence is a marked characteristic, and the inner surface of the anterior wall is very sloping (a point of resemblance to the Heidelberg jaw), but on the other hand the third molars are remarkably small. From the stratum in which the fossil was found, one must infer that the owner of the jaw lived in the early part of the third interglacial epoch, and he was no doubt of Mousterian culture. Schwalbe proposes that the new specimen should be known to science as the "Weimar Jaw."

Among the other papers in the same number of the *American Anthropologist*, two deserve special notice. F. H. Sterns, of Harvard, writes an article on "A Stratification of Cultures in Eastern Nebraska." A succession of aboriginal cultures is not often discoverable in America, but Sterns describes a sequence of three cultures, all prehistoric, which he was able to make out at a certain spot (which he calls the "Walter Gilmore site") in the Missouri Valley, in Cass County, Nebraska. He thinks, however, that the entire succession of types may have occupied less than one thousand years. Another interesting essay is one by Elsie Clews Parsons on "Links between Religion and Morality in Early Cultures." The article (which is evidently written from the Rationalistic standpoint) aims at controverting the idea that a connection between supernaturalism and morality is "a late cultural fact," and strong evidence is adduced that such a connection does exist among modern savages, with the proviso, however, that the ethical code of savages is not the code of white men. One must protest against the use here of the words "early" and "primitive" as descriptive of modern savages. Living barbarians differ much from one another; and although no doubt their respective cultures all resemble in certain negative features the state of life in which primitive *H. sapiens* found himself, and in these same negative points (such as the absence of spectroscopes and Zeppelin air-ships) differ from the so-called civilised nations, yet savages have

certainly evolved far in other respects. The probability is that most barbarous peoples have diverged almost as much mentally as they have physically from that hypothetical being, the common ancestor of all the races of *Homo sapiens*.

Among the contributions to *Man* for the first three months of the current year, anthropologists will probably find the book-reviews the most valuable and interesting, since the original articles suffer, as is not infrequently the case in this publication, from their extreme brevity. The March number contains, however, an important note by M. W. H. Beech on pre-Bantu inhabitants of the Kikuyu district of East Africa, where native tradition speaks of an early race of earth-gnomes, probably either Pygmies or Bushmen. To the same number of the magazine Miss A. C. Breton contributes some careful notes on the stone implements to be seen in the chief Australian museums.

The second part of vol. xliv. (July to December 1914) of the *Journal of the Royal Anthropological Institute of Great Britain* has now been published. The first paper is a detailed study of the famous "Cheddar Man," by Professors Seligman and Parsons. This is the skeleton found in Gough's Cave, Cheddar, Somersetshire, in 1903, and both the skull and the artifacts associated with it are here fully described. The original excavations were carried out very crudely (the cave itself was first discovered as long ago as 1877) and the exact positions of most of the flints and mammalian remains are consequently in doubt, but the authors conclude that the man belonged to the Magdalenian division of the Late Paleolithic (Deutolithic) period, and that he was closely akin to that minor craniological variety known as the "River Bed Type." These opinions are probably correct, but there is nothing characteristically Magdalenian about the mammi-fauna. Another paper of extreme interest is one by the Hon. J. Abercromby on "The Prehistoric Pottery of the Canary Islands and its Makers." When the Canaries were discovered at the beginning of the fifteenth century, the inhabitants of the different islands were almost completely isolated from one another, the art of navigation having been lost. It is supposed that the first colonisation took place in mid-Neolithic times, and the aboriginals fall into three chief races, one of which is said to show certain points of resemblance to the tall Cro-Magnon race known from the Late Pleistocene of

Europe, a second is described as Hamitic, the third form being a short brachycephalic people. Each island has a type of prehistoric pottery peculiar to itself, owing to the absence of intercourse already mentioned. Other papers in the same journal are "Les Touareg du Sud," by Fr. de Zeltner; "The Experimental Investigation of Flint-fracture and its application to Problems of Human Implements," by S. Hazzledine Warren; and a number of shorter articles.

It is announced in *Man* that the International Congress ot Americanists which was cancelled last year owing to the war is to be held at Washington next September, conjointly with the Pan-American Scientific Congress.

NOTES

Richard Lydekker, F.R.S., J.P.

The sister sciences of Zoology and Palæontology have suffered a severe loss in the death of Mr. Richard Lydekker, which took place at his residence at Harpenden on April 16.

His capacity for work was astonishing, and his output even more so. To attempt, indeed, to compile a complete record of all his writings would be to attempt the impossible, since for nearly forty years contributions from his pen, signed and unsigned, appeared in a continuous stream. But though much of what he wrote was but of ephemeral interest, and consciously so, his more serious work will form an enduring monument to his memory, proclaiming him as one of the foremost zoologists of his generation. This much indeed was accorded him in his lifetime.

It cannot be said of him that he was antagonistic to the evolution theory, but this had no attraction for him, and he rarely referred to it. He wrote a few essays, for example, on the coloration of animals, and innumerable papers on new species and sub-species of mammals, which gave one the impression that he was a whole-hearted supporter of Darwin, but on not a few occasions he openly expressed his doubts as to the merits of "Darwinism."

Born in 1849, he graduated at Cambridge in 1871, gaining second place in the first-class Natural Science Tripos. In 1874 he was appointed to the staff of the Geological Survey of India, where he did pioneer work in the mountains of Kashmir. Here it was that his awakening interest in palæontology was quickened. So captivating did this become that in 1882 he resigned his post on the Survey in order that he might devote the whole of his energies to the study of the Sivalik and Pre-Tertiary vertebrates of India, of which the Indian Museum at Calcutta had a great collection. Realising the impossibility of successfully accomplishing this task in India, he came to England and arranged to have the Indian fossils from the Indian Museum

sent to him ·t the British Museum in order that he might there work them out under the most favourable conditions, since the larger collections and libraries in London not only greatly reduced his labours, but materially enhanced their value. Working, as was his wont, with extraordinary speed, by 1887 he had completed a fine series of volumes on these remains. But more than this, between 1885–87 he had prepared catalogues of the fossil mammals in the British Museum, followed, between 1888–91, by catalogues on the fossil reptiles and amphibia, and birds.

But besides his work on extinct types, of which no more than a bare mention is possible here, he was responsible for a series of volumes on *Big Game Animals*, on which he spent an immense amount of hard work. He also wrote three extremely valuable books on *The Horse and his Relatives*, *The Sheep and its Cousins*, and *The Ox and its Kindred.* He was joint author with the late Sir William Flower of a book on mammals which has become a classic, and he contributed almost all that is to be found on mammals in the *Encyclopædia Britannica.*

One of the most valuable of his contributions to zoological literature was his volume on the *Geographical History of Mammals* written for the Cambridge Geographical Series. To this task he brought an unrivalled knowledge, and he marshalled his facts with consummate skill. The great merit of this work lies in the fact that it embraced both living and extinct types, and disclosed a peculiarly keen insight into the problems bearing on the distribution of land and water in past times, a factor of paramount importance in all matters concerning the theme of distribution.

Of no less value to Science were his periodical summaries of zoological work which he contributed to the pages of SCIENCE PROGRESS,[1] and the constant stream of notes on new species which appeared in the Proceedings of the Zoological Society and the columns of the *Field.*

Over and above his contributions to Science Mr. Lydekker accomplished more perhaps than any man of his time for the popularisation of natural history. During a long series of years he contributed to every venture which catered for the amateur naturalist, and his most notable achievement in this direction

[1] Yearly accounts of progress in Vertebrate Palæontology, in the January or April numbers from 1908 to 1915 inclusive.

was the *Royal Natural History*, published in six v umes. Of this most admirable work he was editor, and he himself wrote very nearly half.

Much of his work at the British Museum of Natural History during the last twenty years had this same end in view. He was in charge of the public galleries devoted to the exhibition of mammals, including the Cetacea, and the reptiles: and the fine collection of domesticated animals also grew up under his fostering care. He was also in charge of the Anthropological Gallery. As was but natural, he also wrote the guides to the collections under his charge, and since these have sold in their thousands they must have done not a little to foster the study of natural history in all classes of the community.

To chance acquaintances he probably seemed almost taciturn; but those who were privileged to enjoy his friendship regarded him with real affection. The writer of these lines, for nearly twenty years, was among those thus favoured. Often we worked together, and always harmoniously.

He possessed a keen sense of humour. And he needed it. For some of the requests he received through the post, in his official capacity at the British Museum, were astonishing in their impudence. As for example when he was requested to furnish full details of the seasonal coloration of Arctic animals for the benefit of a correspondent who had been asked to write an article on this theme by the editor of a popular magazine, but who, as he confessed, "knew absolutely nothing about the subject." In return the writer promised faithfully to send proofs for correction to the Museum, and to make no mention of the source from whence he had obtained his information, as he knew that Fellows of the Royal Society did not like mention of this sort! As no answer was vouchsafed to this remarkable request an indignant letter followed, requesting an answer, with the necessary data, by return of post!

Holidays bored him. His only recreation was gardening, perhaps because this entailed *work*. And for a like reason, one may surmise, he served for many years, and up to the time of his death, as a Justice of the Peace.

Of him, indeed, it may be said that he lived laborious days, and he died in harness; for only a few hours before the end he was dictating an article to his brother, being too weak to write himself.

It may well be that his ceaseless energy shortened his days, but his was a nature which found "leisure" irksome. Had he striven to husband his strength he would have achieved less; but this would, to him, have seemed unworthy, for in work he found the main incentive to life.

W. P. PYCRAFT.

The British Science Guild

The important pronouncement by the President of the Board of Education in the House of Commons on May 13 last, regarding the proposed formation of an Advisory Council concerned with industrial and scientific research, has aroused widespread interest. The British Science Guild, since its inception in 1905, has been working assiduously and with little encouragement to organise scientific work in the interests of national welfare, and the proposal of the Board of Education may be regarded as a practical endorsement of the work of the Guild.

It is interesting to note that during the past three months the work of the Guild and its Committees has covered a wide scope of subjects, including Anti-typhoid Inoculation, the Shortage of Drugs, the Manufacture of British Microscopes, the Remuneration of Scientific Workers, the Manufacture of Optical and Chemical Glass and Porcelain Apparatus, etc., etc.

The publication of an advertisement in an attempt to dissuade British soldiers from undergoing the inoculation against typhoid, recommended by the military medical authorities, led the Medical Committee of the Guild to issue a statement warmly approving of the practice of inoculation against typhoid, and condemning in strong terms the unpatriotic action of the authors of the advertisement. Copies of this statement were forwarded to the Prime Minister, the War Office, the General Medical Council, medical societies and schools, and to the commanding officers of British regiments. From many of these the Guild received expressions of cordial agreement and applications for additional copies of the statement.

The Education and Technical Education Joint Committee considered the best means of overcoming the difficulty which was being experienced in educational institutions in this country through the cutting-off of the usual supplies of glass and porcelain apparatus from Germany and Austria. An inquiry

was instituted, and subsequently a Report was issued and circulated to the Press, the principal educational institutions, the leading dealers in scientific apparatus, glass manufacturers, and to the Secretary of the Business Committee of the Members of the House of Commons. As a result of the Guild's action there is reason to believe that a considerable stimulus has been given to the movement for the establishment, in this country, of the manufacture of glass and porcelain apparatus for scientific purposes.

Following a request from Lord Moulton, the Chairman of the Technical Sub-Committee of the Committee appointed by the Board of Trade in connection with the extension of the manufacture of glass in this country, the Guild appointed a Special Committee, consisting of scientific experts and representatives of the trade, to consider and collect information as to the best means to adopt in connection with extending the manufacture of optical glass. A Report was duly prepared and forwarded to Lord Moulton and to the Permanent Secretary of the Board of Trade. Copies were also circulated to the Press, the principal photographic societies, the members of the Optical Convention, the corresponding members of the British Association, the members of the London County Council, leading opticians, glass manufacturers, etc. The Secretary of the Business Committee of the Members of the House of Commons also asked for copies of the Report for the use of the members of the Committee.

The attention of the Technical Optics Committee of the Guild was drawn to the fact that as the German trade in microscopes has now ceased owing to the war, the present is an opportune time for an effort to be made to secure the trade for British manufacturers, and it was pointed out that the British Science Guild could render a great service by endeavouring to arrange for the co-ordination of the efforts of the makers in this matter. The Committee decided to institute an inquiry with a view to helping forward the proposal, the following representatives of manufacturing opticians being invited to assist the Committee: Mr. C. Lees Curties (of Mr. C. Baker), Mr. R. Mansell Swift, Junr. (of Messrs. J. Swift & Sons), and Mr. F. Watson Baker (of Messrs. W. Watson & Sons, Ltd.).

The matter is at present under consideration, and the Report of the Guild will be issued in due course.

The Guild appointed a Special Committee also to consider and report upon the best means to be adopted for securing (i) increased financial support from the Government for higher forms of intellectual effort, and (ii) the adequate remuneration of scientific workers and learned societies for services rendered in connection with Royal Commissions, Departmental and other Government or public committees, the remuneration of representatives of the Government at Congresses, etc., and of expert advisers and witnesses in Government inquiries, lawsuits, etc. The Committee instituted an inquiry as to the practice adopted by the principal Government Departments, municipalities, and other public bodies, and, following the receipt of replies, the Guild issued a general letter asking the heads of such departments and authorities to recognise the principle that no expert advice be taken without the payment of some kind of fee in addition to travelling and maintenance charges.

The Committee is at present making inquiries in regard to the remuneration and terms of employment, etc., of scientific workers in universities, university colleges, etc., with a view to securing some improvement in the same, and a report will be issued in due course.

J. C. B.

(Copies of any of the Reports and statements referred to above may be had on application to the Secretary of the British Science Guild, 199, Piccadilly, W.)

War Inventions

It is difficult for those who cannot possibly know the actual facts of the case to imagine what the Governments now engaged in the war are doing to encourage, consider, and utilise inventions which may quite possibly finally prove decisive in the struggle. The forces opposed to each other are so evenly matched that any invention which at first sight appears to be quite trivial may turn the tables. Men of science, who know from experience how much often depends upon trifling improvements of instruments or of technique, will know what this remark implies. Thus Tycho Brahe's improvements of astronomical instruments almost founded modern astronomy, and the discovery of oil-immersion lenses almost created modern pathology. Similarly, modern warfare is really based upon mechanical inventions, and even strategy has been forced to conform itself to them. May not further mechanical inven-

tions produce other and perhaps quite unlooked-for changes? Some time ago an Italian advertised an invention which he claimed was able to explode shells, cartridges, and even magazines at a distance by means of certain rays. We understand that this claim proved to be unfounded when rigorous tests were insisted upon; but what a revolution in warfare it would have made! Unless some means could have been devised to exclude the rays, we should have gone back immediately to the conditions of the Middle Ages, and would again have witnessed the spectacle of knights and men-at-arms clad in mail and fighting with spears, swords, and "morning stars." Certainly such warfare was more picturesque than the miserable subterranean rat-like struggles of our soldiers to-day —though, we believe, the mortality in the old times sometimes reached 80 or 90 per cent. of the vanquished forces in one day! In those times there was no escape by hiding in holes, and the man who got his adversary down generally finished him in the heat of the combat, unless he could conveniently take him a prisoner and hold him to ransom. At all events, the old methods of fighting, plus the modern universal service, would probably have decided the present war in a few weeks.

We do not feel convinced that our Government is at present doing all that it might do to stimulate such inventions. Is it quite impossible for ships to be really protected against modern torpedoes? Is it quite impossible to construct submarines for the purpose of hunting other submarines under the surface? Cannot men-of-war be provided with some large buoyant rafts which will serve to save a larger proportion of their crews when the ships are torpedoed? Some time ago we believe that Sir Hiram Maxim suggested that solid bodies may emit certain rays which could be detected from afar and which would serve to notify ships of the presence of other ships, including submarines, at a distance. Has this idea fallen through? Is it quite impossible for infantry to be provided with some kind of metallic shield which, though penetrable by a rifle bullet that impinges upon it at a right-angle, will serve to deflect a proportion at least of the projectiles which strike it at a lower angle, and could not such shields be made light enough to be used in short rushes from trench to trench? And many more ideas of the same kind may be mooted.

Even if the answer to all these questions is in the negative, yet surely it would be wise to encourage as much as possible all the inventive genius in the country. Could not special committees, or even departments, be formed for doing this? If, as many suppose, the war may last for years, it will yet be not too late to work in this direction. Had we as a nation done more to organise research, we should probably have already been in a position to utilise this kind of genius to its utmost. But stupidity always punishes itself, and it is quite possible that many of our losses are, to speak frankly, chiefly due to this quality.

A Letter from the Front

How much the wretched payment given to men of science in this country is exercising their minds may be gathered from a letter which we have received from a very capable junior worker now at the front. He says: "I have been hoping that the war might have as one good result the better treatment of the scientist in England, but judging from the Aniline Dye affair, as I read of it in the papers, things are if anything a little worse than ever, and the Government will make no attempt to utilise the scientific ability of the country, neither will it give the scientist any opportunity of working out his own salvation. I hoped to return, if I return, to more favourable conditions; but, as far as I can see, with the Universities probably poorer and meaner than ever, there will be no hope for a young research worker to make the scantiest living after the war, and I presume the country will economise by cutting off the grant of five thousand pounds a year (is it not?) to the R.S. for research work. . . . Is anything being done to further the interests of the research worker (which are the interests of the country) at home? . . . Naturally, when there is a lull out here I cannot help wondering whether I have any prospects at all to return to, or whether I shall come back to find the few underpaid posts that might be open filled by the stay-at-homes. . . . A lot of us out here feel that Germans are having a pretty good time in England now, and will have a better one when the war is over. The men are worried by hearing of the things like Canon Lyttelton's speech, the reports that German agents are stirring up strikes (why are Germans at large?), and the very favourable treatment of German officers in England.

They have mostly seen more of German methods than I have." Our sentimentalists are, as Swinburne said, lovers of every country but their own, and it is instructive to learn what the young men who are fighting for us actually think about them. While the enemy destroy our sons by every means, fair or foul, from honest shooting to the use of poisonous gases, the treacherous employment of the white flag, and other gentle artifices of their own, our "stay-at-homes" are exercising their delicate souls to find excuses for their action, and sweet words of forgiveness which they fondly hope may turn away the wrath of the brigands who are trying to rob the world. Similarly our anti-scientific cranks are continuing to do their utmost to help the enemy by tying our hands against typhoid in order to prove (such is their notion of proof) their ridiculous hypotheses about anti-vivisection and anti-vaccination. All these people belong to the same class. It seems to be a weak spot in the British intellect, to help enemies of all kinds as much as possible in order to demonstrate one's own wisdom and magnanimity.

The Real Order of Merit

Viscount Haldane, who prepared the nation so admirably for the present war, has been given the Order of Merit on retirement from the Ministry; but there is another Order of Merit which is seldom bestowed on such occasions. Our soldiers who have been disabled at the front receive it, however, with a lavish hand. From the numerous letters which appear in the press upon the subject, this Order seems to amount to a maximum sum of seventeen shillings and sixpence a week given in full recognition of the fact that such soldiers have been incapacitated for all work by their sense of duty to their country and by the terrible wounds which have been inflicted upon them in its defence. On the whole this latter order of merit appears to us to be on all fours with the similar orders of merit given to so many of the benefactors of the human race—to which we have often referred. How grateful our soldiers must be to their fatherland. Perhaps there are a few grumblers—but what then? One poor fellow is reputed to have said, "They seem to look on us as they do on their guns—when worn out to be cast aside as old iron." But surely he has forgotten the honour which he will always receive in the workhouse in

which he is destined to spend the rest of his life. Doubtless the Bumbles who manage these costly but almost useless concerns will take off their hats to him whenever they see him—just as the whole nation does to those who give it benefits in the lines of art and science. Even the *Times* exclaims to the effect that there is no class which has a greater claim on the nation than disabled warriors, and we read that the nation is burning to acknowledge the claim and to do its duty by these splendid heroes. Precisely: but the values of things are usually measured in currency. Let us see by how much cash this ardent spirit will be measured. Supposing that the two or three hundred thousand pounds a year which Parliament takes out of the exchequer for its admirable government of the country should be cut up amongst our disabled soldiers—how would that strike the nation? and why should not our Ministers of State work merely for the honour of the thing, just as do our best scientific investigators, poets, philosophers, and other people who perhaps confer even larger benefits on the public? Still further, if we can pay thousands a year to any political adventurer for signing his name to documents about which he knows little or nothing, surely we might spare a little more money to those whose arms and legs are blown off them in the trenches. We suspect that at the end of the war this question will be raised somewhat more insistently than it has been raised up to the present.

A Hoped-for Revolution in Britain

On May 11 Lord C. Beresford asked the Prime Minister in the House of Commons, "Whether he would consider the desirability of arranging that every man who had volunteered for the war, and so risked his life in the service of, and defence of the country, should be entitled at the age of twenty-one to a Parliamentary vote during his lifetime, irrespective of other qualifications?" At the same time Mr. H. Terrell asked the Prime Minister, "Whether he would give the House an opportunity of discussing the question whether all men who had recognised their duty to the country by serving in His Majesty's naval and military forces during the war ought to be given a voice in the management of the affairs of the country?" To these Mr. Asquith replied, "that he could only repeat that the whole question of franchise and registration is receiving careful and detailed consideration, and he hoped shortly to be

in a position to make a statement with regard to it." Thereupon Mr. Snowden, the Labour Member for Blackburn, asked, "Does the right honourable gentleman accept the implication in the second question that only those persons recognise their duty to their country who are engaged in naval or military work?" To this Mr. Asquith replied, "No, sir; if there were any such implication I should repudiate it." Mr. Snowden's question is an example of our false ideals. Undoubtedly all those who are working for their country during the war, or are even working honestly for themselves and their families, do in a sense recognise their duty; but there is a quantitative difference between their recognition of duty and that of the men who are suffering so enormously and dying so frequently in the trenches. Does Mr. Snowden mean to imply that every person who does anything serves his country as much as the man who gives his country everything, including his life? According to him, there is no difference between an ounce of duty and a ton of duty, because both are duty in some quantity! His hypothesis is of course pleasing to those who remain at home; and Mr. Asquith's reply is typical of the politician. Neither of them seems to understand that the time for such sophistries has gone by. When our brave soldiers return from the war (if any of them do return) other questions will be raised—whether, for example, those who should have gone to the front but have not done so are deserving of retaining any vote at all. In the opinion of many they are not entitled to one unless they can show good reason for their want of action, and there are many who would gladly see the Houses of Parliament themselves rid of members who in this crisis of their country have done little to help it. We suspect that the war will be followed by a revolution in England, and one which will be directed principally against the sort of people who now rule us and whose frequent neglect of the advice of experts and of the evident facts of the case has been largely responsible for the war. In a letter to the *Times* of May 18, Mr. Robert Yerburgh, M.P., writes: "When detained at Nauheim I asked, shortly after the breaking out of the war, a German whom I knew well, what would have been the position if we had had national service. 'There would have been no war,' was his reply." But our politicians and their subservient press ignore this point altogether; nor have we heard a single apology from the men whose obstinate preference

of their own interests to the interests of the country was so undoubtedly a factor in provoking the great disaster. The captain of a ship who persists in a certain course in spite of the advice of his pilot and consequently runs his vessel upon the rocks is deprived of his certificate and dismissed from his appointment; but politicians who run their country and the world upon the rocks of war are still allowed to govern, to advise, to talk platitudes, and to draw their salaries! We want new captains for our political navy.

Party Impolitics

In the Press of May 29, a letter appeared from Mr. Asquith to Mr. J. W. Gulland, Parliamentary Secretary to the Treasury, in which he announces the formation of the present Coalition Government. In this he says that "The transformation implies a temporary abandonment of the system of party government, which has ever since 1832 dominated our political arrangements and which I hold to be, under normal conditions, the best adapted to our national requirements." He continues to say that "It was only because the conviction was forced upon me that a non-party Government would prove the most efficient instrument for the successful prosecution of the war that I have taken a step which has caused me infinite personal pain." Surely there is something very extraordinary in this point of view. If Coalition Government is the best for times of stress, surely it is likely to be always the best. On the other hand, if Party Government is ever a good system for enabling the nation to come to a conclusion regarding any vexed question, it should also be a good one for enabling a nation to decide between issues in war-time. But now we find that Mr. Asquith has suddenly abandoned it in emergency. If there is a better form of government, why is it not always used, and not only in war-time? If the horse is so lame that Mr. Asquith must ride another in order to cross the torrent, why does he not ride this other one always?

The country is doubtless grateful to Mr. Asquith and our numerous other politicians for the undoubted work which they have individually put into the matter of the war; but this does not mean that the country any longer approves of the system of government which Mr. Asquith still favours—and abandons. The fact is that throughout the country, among all educated men

who are not deflected from correct reasoning by party bigotry, an overmastering conviction is now growing that we have had enough, not only of party politics, but of the kind of men who have been playing an amusing game of football with the national interests for nearly a century. In the January number of Science Progress it was argued that party politics are scarcely reliable as a method of eliciting the truth in any subject; and scientific men, who are accustomed to dissect evidence in the most difficult of all fields, will scarcely share Mr. Asquith's sufferings at having to desert this quaint fad of the British nation. People are asking throughout the nation whether indeed party politics have not been very largely responsible for the slaughter of our sons, brothers, and husbands; whether the party politicians did not culpably shut their eyes to the German menace; and even, when that menace was fully brought home to us, whether they did more than a tithe of what they ought to have done to prepare the nation to meet it. Instead of guiding the nation in the paths of wisdom, they were so solicitous for the welfare of their parties (that is, of themselves) that they made no genuine effort to draw the British people away from an attitude which many think was one of colossal stupidity accompanied by a very real indifference to the duties which every person owes to the State. We have heard it said, not once nor twice, that the time is come when the nation should root out this particular form of imposture, which is of benefit principally to those who indulge in it. A visitor from Mars might criticise modern Britain by saying that it is chiefly a nation of quack medicines and quack politics. But just as quack medicines are abandoned when the illness is severe, so it seems that quack politics are to be abandoned when the nation is in real danger.

In these and in preceding notes and articles we have often thought it right to express not a little criticism, both of our methods of government and of the men who govern us and also of certain intellectual, or rather non-intellectual, qualities amongst our own people. Science is not only a fairy godmother to humanity: she is also herself a goddess whose great religion and commandment to all is to think the truth. To her and, we believe, to the vast majority of her genuine votaries, the whole system of party politics is based upon a false political hypothesis and is conducted by means of wilful distortion of facts and prepense employment of the lying argument; and we fear that

a nation which indulges in this evil must certainly have sunk to a somewhat low intellectual level. But we allow ourselves to speak in this manner in a moment of national danger only because we believe that the time is ripe for such utterance. On the other hand, if we deplore such intellectual failing in the country, we are glad to recognise with enthusiasm and pride the great moral altitude to which the nation has risen in this crisis. The censure which the brigands of Potsdam have endeavoured to fasten upon us in order to justify their own real or pretended hatred of us arises from the fact that they have made the common mistake of judging others by themselves. It is not true that the British nation have ever felt any jealousy of the Germans or the slightest enmity against them. Before the war we always showed them the greatest goodwill, we admired their science, their organisation, and their prosperity; we taught them all that we had to teach them, and we welcomed them in our midst. At the beginning of the war our attitude, if not always wise in our own interests, was always absolutely just and proper; and during the conduct of the war we have honourably fought without hate in a cause which the whole world except our enemies acknowledges to be a just one. We are certain as to the verdict which posterity will pronounce upon our moral attitude throughout. If our intellectual capacity has not been so high as our moral one, that is a matter which concerns ourselves, and which must be amended by ourselves; and an offence in this category is not so heinous in the eyes of that great Order of Things which controls us. Whatever our intellectual failings may have been, at least we feel that our rulers and ourselves have always borne within us during the whole of this unspeakable war the absolution and the sanction of a pure conscience.

The National Efficiency Committee

Readers of Science Progress will be glad to hear that a committee has been formed with the title National Efficiency Committee. Its original object was to perform the function of a Vigilance Committee with regard to the recent temperance legislature proposed by Mr. Asquith's Government, which, as every one knows, came to very little, partly owing to the opposition of the Irish members, and partly to indifference. The

National Efficiency Committee has published several statements of policy on this subject and, it is to be hoped, will turn its attention to many other subjects requiring attention which have hitherto been left too exclusively in the hands of politicians.

The Napier Tercentenary, and the Invention of Logarithms

We regret that we have not been able to publish in this number a very interesting article upon the International Congress held last year in connection with the Napier Tercentenary, but hope to be able to give it to our readers in the October number. The article is written by Dr. C. G. Knott, the Secretary of the Congress and the Editor of the Memorial Volume which will be produced in the autumn (and copies of which may be obtained by non-members of the Congress from Messrs. Longmans, Green & Co., Paternoster Row, London, E.C.). Copies of the Handbook to the Exhibition may also be obtained through Messrs. G. Bell & Sons, York House, Portugal Street, London, W.C.

"The Monist"

The number of this quarterly for last April contains some interesting matter. Mr. P. E. B. Jourdain has made a special study of the historical aspects of Newton's work, which is of perennial interest to all men of science. He now continues this historical study in connection with Newton's hypotheses of ether and of gravitation from 1679–93. There are also very interesting articles on the Disciples of John and the Odes of Solomon by Preserved Smith; on the Methods of Theoretical Physics by Ludwig Boltzmann; on the Experience of Time by Bertrand Russell; many useful discussions, and the reprint of an extract from a paper by Sir John Herschell on Hindoo Mathematics. We are specially struck by a very beautiful poem called "The Over God" by Mr. Paul Carus—beautiful because of its simple sincerity and the essential grandeur of its subject. Much of the poetry of the day consists merely of little verbal prettinesses, has no subject of importance, and is backed by no experience of real things or genuine knowledge. More poems by men of science would probably serve to set a higher tone in literature.

"Scientia"

Another excellent monthly is *Scientia* (Williams & Norgate, London), an Italian periodical which in fact has much the same mission as SCIENCE PROGRESS, except that the articles are given in four European languages. The number for last November contains an excellent article by Sir Ernest Rutherford on the Structure of the Atom—one of the most lucid resumés which we have seen—and another by Prof. C. Golgi. More recent numbers have, however, been largely occupied with the war. Sir Oliver Lodge writes a paper on the war from a British point of view, but German authors are allowed to express their opinions also. Professors Pareto and Landry contribute an Italian and French article respectively on the same subject which are worth reading.

The Hindustan Review

Certainly one of the most interesting reviews now being published is the *Hindustan Review* edited by Sachchidananda Sinha, Barrister-at-Law, and the most interesting articles in it are by Indians. Prof. Dr. Ramdas Khan gave a good article in the January number upon the question whether Germany can be a World-Power. And the same number contains other excellent articles on Indian orators and on the question whether Western civilisation will survive. There is even a selection of "Humour in the Army" and another of "Poems of the Great War." The review is one which should be encouraged as much as possible in Britain.

The Medical Report of the American United Fruit Company

Not many large British business concerns are known to issue Annual Medical Reports, and it is certainly a sign of progressiveness that the American United Fruit Company does so. As a matter of fact few British Colonial Governments have, until quite recently, published anything so good. The operations of the Fruit Company evidently cover a very large area in the tropics, and this report contains several observations which will interest students of tropical medicine and sanitation.

REVIEWS

MATHEMATICS

A Course of Pure Mathematics. By G. H. HARDY, M.A., F.R.S. Second edition. [Pp. xii+442.] (Cambridge: at the University Press. Price 12*s.* net.)

A COMPARISON between the treatises on the Differential and Integral Calculus of Dr. Todhunter and the book before us would convince even a superficial observer of the change which has revolutionised mathematical instruction in this country in the last quarter of a century. The writers of text-books at the beginning of that period believed that the subject, as they expounded it, was as firmly established and as logically deduced from its premises as geometry. Their practice had the authority of great names, and the form of their books reflected their confidence. Proofs which had been devised by Euler and Leibnitz seemed above challenge. If cracks were visible in the armour in which these authors did their work, they were not pointed out; the brilliant results achieved sufficed to give sanction to the theory. Under such circumstances it was unnecessary to examine foundations too closely; the theory expounded fitted certain classes of functions, and by established custom the student's attention was confined to such functions. A good deal of comfort was then, as it is to-day in another branch of philosophy, administered to doubting disciples by the use of the mysterious word *continuous*. Even in universities it was no uncommon thing to hear a lecturer declare that a function was continuous, and therefore could be differentiated. English mathematicians did not concern themselves with functions which declined to obey their rules, indeed, such functions were regarded as freaks. The work of Weierstrass, communicated in the lecture room to his students, made its way slowly across the Channel, while Cantor and Dedekind found few disciples in our midst. But the work of these great thinkers has as certainly changed the light in which every mathematician regards the Infinitesimal Calculus, as Newton's discoveries affected the outlook of the natural philosopher upon the material universe.

Now Mr. Hardy's book is almost the first attempt to bring before junior English mathematical students at the outset of their career the rigorous methods by which alone this subject can be safely established. Mr. Hardy's task is one of no common difficulty; he can at the best select. It is impossible for him to present the theory in its entirety even to clever undergraduates. No reader can, however, finish the course of Pure Mathematics without knowing a good deal about limits, infinity, functions, continuity, and without wishing to know more of these subjects. Such a reader will have no excuse for thinking that functions which are continuous are necessarily differentiable, and he may even have learnt that continuous functions can be integrated. It is an excellent thing for mathematics and its progress in this country that the book was written, and it is a sign, full of promise, that a new edition has been demanded so soon. In the second edition the author has made certain excisions and some additions, the result of which, as he warns us, is to make the book a little more difficult.

In making the changes he seems to have acted with judgment: a brief account of Dedekind's theory of numbers has been introduced; it might indeed have been extended with advantage. Again, he has given a more complete account of integration which has involved several preliminary propositions, including the Heine-Borel theorem. Perhaps in a future edition the author will consider the advantage of proceeding even farther with the real variable, even if the complex variable has to suffer. An alternative suggestion is that the course should be divided into two parts, in which the real and the complex are separately discussed, a division which has the sanction of Goursat's example in his *Cours d'Analyse*. In English teaching there is a thoroughly unsound practice with regard to complex numbers, and the only correction possible seems to be a complete division between the study of the two fields of the real and the complex. Such a change would no doubt involve additional space, but if it effected a reform which resulted in leading students and teachers into more logical methods, it would be well worth the trouble of making.

Mr. Hardy writes well, and never shirks telling us how very little he is doing: he has also a pleasant habit of interspersing matter which is not germane to his subject. Such rubbing-posts are very welcome to the restless student in a journey which is necessarily at times tedious. But it is doubtful whether he was well advised in trotting out Mr. B. Russell's trite paradox on the value of mathematics: we are sure that he was unwise in attempting to dissect it. The wit of a philosopher is often worthy of admiration, but it should always be admired from a distance. To base upon a paradox "highly important truths" is certainly out of place in a treatise in which above all a plea is made for secure foundations.

C.

A New Analysis of Plane Geometry, Finite and Differential. With numerous examples. By A. W. H. THOMPSON, B.A. [Pp. xvi + 120.] (Cambridge: at the University Press. Price 7*s.* net.)

THE choice of a title is by no means the easiest part of an author's task. In the present instance the author can hardly have been satisfied with the short title with which this book is labelled. "Plane Geometry" in no way prepares the reader for the scope of the book. It is true that the full title does suggest that the author is making a new attack upon an old fortress, but how novel the method is can hardly be explained in a title, or even in a short review. The method of treatment is so entirely the author's own that the critic has to keep in mind that few things are so hard to judge as the power and adaptability of a new notation; it is only by forgetting old habits and associations of thought that he is able to estimate at all such novel methods as those introduced in this book. The author too who devises new attacks upon old problems does not easily comprehend the difficulties which he will have in changing the habits of his critics and readers; there is a certain fascination about new methods of locomotion which unsettles the judgment of the inventor and leads him to underestimate the perils of the first performance of a process which he has himself learnt to execute with ease and safety. Mr. Thompson has perhaps erred in the presentation of his new method inasmuch as he has not sought to convince us of its charm; he may, however, claim that if he has erred, he has done so in good company, for our greatest thinkers have too often failed to present their best thoughts in an attractive form.

The elements of which the book treats are lines and points. The author defines the measure of a pair of elements as the quantity determined by the pair, and

expresses geometrical properties as equations in these measures. Now between the measures of the six distances determined by four points and between the measures determined by two points and two lines there are relations: they are called eliminants. Thus geometrical properties are reduced to algebraical equations involving eliminants.

After the statement in the briefest terms of the object and scope of the book given partly in a preface and partly in an introduction of seven pages, Mr. Thompson commences his task of elaborating finite and differential geometry, and in performing this gives us about 110 pages of solid, serried ranks of symbols. A little more bread and a little less sack would have formed a more palatable fare. The 110 pages too, are not of ordinary strength: they are composed largely of that very concentrated essence which the Cambridge mathematician dignifies as examples.

The book is strictly confined to two dimensions and the author gives no indication of any attempt which he may have made to extend the method to three dimensions. Indeed he has deliberately cut himself off from the ordinary notation by using $a, b, \ldots$ for points and $\alpha, \beta, \ldots$ for lines, thus leaving no simple choice of symbols for planes. It is perhaps a little unfair to criticise an author for not including matter which he has distinctly excluded by his title, but it would be unwise to expect a very wide acceptance of a novel notation in plane geometry which does not allow of extension to three dimensions.

C.

PHYSICS

The Principle of Relativity. By E. J. CUNNINGHAM, M.A. [Pp. xiv + 221.] (Cambridge: at the University Press, 1914. Price 9*s.* net.)

THE belief in the existence of a medium for the propagation of electromagnetic radiation has become so definite, that when the Principle of Relativity was first propounded about ten years ago, a so-called insuperable objection was at once raised to it, viz. that it did away with the possibility of an objective ether. Further it was asserted that the principle introduced views about space and time much too artificial to be reconciled with the intuitive notions which we already possessed concerning those two modes of perception.

It must be granted that the present volume goes a long way towards destroying the force of both these objections. Perhaps its greatest merit is the very careful analysis to which are subjected all such concepts as mass, force, momentum, energy, with which dynamical and electrical theory are concerned. The author shows that the standards of space and time as employed in dynamics are not, as frequently supposed, absolutely defined for us apart from natural phenomena. The existence of such absolute standards is an *assumption* of Newtonian theory, only justifiable by the simplicity which it introduces into the co-ordination of phenomena, and not based on any philosophic doctrine. As far as dynamical and electrical theory is concerned, if there comes to us knowledge of phenomena which cannot be fitted into the Newtonian scheme, except by hypotheses concerning the constitution of matter, hitherto unsuspected and having no *à priori* justification, there is nothing contrary to scientific procedure in laying aside any attempt to formulate such hypotheses, and in endeavouring to correlate our new knowledge by a revisal of our concepts of space and time and by an acceptance of standards of those which are not absolute, but vary with the frame of reference in which measurements are being carried out.

Briefly the new knowledge has been gained by all those experiments which

have failed to reveal to us any motion of the earth relative to an assumed fixed ether, a failure incomprehensible, as far as Newtonian dynamics is concerned, except by the adoption of some such hypothesis as the Lorentz-Fitzgerald contraction of bodies produced by their motion through this ether, a hypothesis which, as has been shown by Lorentz, must be carried down to the electron itself, if consistent results are to be obtained.

The first four chapters of this book deal with these experiments, and the views advanced, prior to Einstein's work, to account for their failure. The fifth and sixth chapters treat of Einstein's ideas concerning the relativity of space and time measurements to the frame of reference, and the modifications introduced by such ideas into kinematical theory, so as to correlate phenomena as far as possible without any special theories of matter. There follows an account of the application of four-dimensional calculus which has been made by Minkowski to the Relativity Principle. In it space and time are considered as "complementary aspects of an underlying unity," and just as in the older dynamics and the electrical theory based on it, there was a certain amount of arbitrariness in the choice of axes, in view of the fact that the various relations existed between three-dimensional vectors, so in the Minkowski four-dimensional "World," which embodies ordinary space dimensions and a time-dimension, agreement with Einstein's Principle is ensured by a similar arbitrariness in the choice of the four axes, so as to preserve a vectorial form for all relations. The author does not deal with Minkowski's four-dimensional hyperbolic geometry, but with his geometric method which makes the various transformations formally analogous to the customary change of axes in space by a rotation. Minkowski's method cannot, to be sure, be used directly to devise theories of the constitution of matter, or invent analytical changes to be made in our equations of motion; but it serves as a criterion for all attempts in those directions. Such attempts, if they are to be consistent with the relativity of phenomena, must be capable of formulation as relations between the four-vectors and six-vectors of Minkowski's "World." The remainder of the book deals with the recent changes which have been made in pure dynamical theory, in elastic solid theory, in the electrodynamics of moving bodies, and in thermodynamics. A chapter is devoted to the first objection mentioned above, concerning the existence of an objective ether, and is an extension of a paper by the author in volume 83 of the *Proc. Roy. Soc.* It would appear that while the existence of a *fixed* ether is impossible on the relativity view, yet it is not inconsistent with that view to assume the existence of an ether which is moving relatively to any observer with the speed of light, the direction of motion at any point depending upon the values of the electric and magnetic vectors at that point. One striking result of this analysis is the conclusion that in the neighbourhood of a stationary point charge, the flow of the ether is radially outwards from the point, inasmuch as it recalls the "Ether-Squirt" theory of matter propounded some years ago by Prof. Karl Pearson.

The mathematical treatment given in this volume is adequate and complete without undue elaboration, vectorial methods being employed throughout. In fact there is a very judicious division of space between the mathematical development and a philosophic discussion of the results and concepts obtained from and embodied in the analysis. It seems a pity that some kind of symbolism cannot be devised which, while preserving for the reader an easy method of perceiving that he is dealing now with scalars, now with three-dimensional vectors, and now with four- and six-vectors, would at the same time remove from the printed page all the weird and wonderful types that have recently invaded Vectorial Analysis.

The book should be read by all those who desire to become acquainted not only with the fundamental changes which the Relativity Principle introduces into our conceptions, but also with the problems which still await solution and the attempts which have been made in that direction.

J. R.

Molecular Physics. By JAMES ARNOLD CROWTHER, M.A. Reprinted from the *Chemical World*. [Pp. viii + 167, with diagrams and illustrations.] (London: J. & A. Churchill, 1914. Price 3*s*. 6*d*. net.)

THIS little volume is reprinted from a series of articles which appeared in the *Chemical World*, and forms one of the publishers' Text-books of Chemical Research and Engineering. Although named *molecular* physics it of necessity deals extensively with what we may call *atomic* physics. The book serves quite a useful purpose, more especially for chemical investigators and engineers, in that it gives a very succinct account of the latest work and conceptions regarding the structure of atoms and molecules, without venturing too deeply into a mass of mathematical detail. Chap. I. is historical and introductory and leads up to Chap. II., where the physics of the electron are treated. The cathode rays are described and the various methods of evaluating the ratio m/e. Then follows a description of Wilson's expansion experiments, and the measurement of e and finally of m. Two very fine photographs due to Wilson are included in the chapter showing the tracts of α particles and of the ions produced by the passage of Röntgen rays through the expansion chamber.

In Chap. III. the positive particle is introduced, and in Chap. IV. a very concise account is given of Thomson's new method of chemical analysis by the photography of these rays from different substances. The apparatus used is clearly described, and by the courtesy of Prof. Sir J. J. Thomson the author has been able to illustrate the text with photographs made from some of the original experimental negatives. Apart from Prof. Thomson's own book on the subject, this is the best presentation of this new and extremely fine method of analysis which we have seen. One interesting feature found here, as indeed throughout the whole book, is the way in which the author looks ahead and delights the imagination with a peep into the region of future investigation and possibilities. The next chapter deals more fully with the electron—the tubes of force surrounding it at rest and in motion—the variability of its mass at high speeds, and the purely "electrical" conception of mass.

The Chemistry of the Model Atom (Chap. VI.) is based on the original Thomson idea of concentric spheres of increasing numbers of electrons.

A description of these views is interesting in itself, but it must be remembered that here the author departs from the solid ground of experimental fact into the domain of speculative although somewhat plausible theory.

Under the Vibration of the Atom (Chap. VII.), the probable electro-magnetic nature of light, the Zeeman effect and line spectra are shortly reviewed.

Chap. VIII. considers various phenomena of matter such as viscosity, vaporisation, conduction of heat and electricity in the light of what has been previously learnt regarding electrons, atoms, and molecules, while the final chapter is taken up with a brief account of radioactivity.

For the benefit of those readers who wish to revise the derivation of some of the more important formulæ, there are short appendices on electrostatic and magnetic deflection, the electro-magnetic mass of an electron and the Zeeman

effect, where the mathematical proofs are worked out. The volume closes with a very useful table of the most recent determinations of atomic data and a bibliography. J. R.

Practical Heat, Light, and Sound. By T. Picton, M.A., B.Sc. [Pp. xv + 150 with diagrams.] (London: C. Bell & Sons, 1915. Price 1*s*. 6*d*. net.)

This little volume is the second of a Science Series for Schools and Colleges issued by Messrs. Bell & Sons. It would appear that the series is being edited and written by a number of schoolmasters, who have some experience in the special difficulties attendant on the teaching of science in schools. If this book is to be taken as typical of the series, then it will certainly be welcome. The author, who is on the staff of St. Paul's School, has evidently his own ideas on the teaching of Physics and the means by which the performance of an experiment may be made neither a meaningless piece of mechanical work for the boy nor an opportunity solely for a display of neatness in keeping notes with a profusion of "red ink." The experiments are well chosen, clearly described and illustrated with good, but not too elaborate diagrams. An excellent feature is the provision of a number of questions which are associated with each experiment and which, by the suggestion of the author, ought to be answered by the pupil before he proceeds to the following experiment. That the author is alive to the bane of useless and frequently inaccurate elaboration of apparatus is shown by his avoidance of the usual so-called "water-trap" device in latent heat of steam experiments, which is more usually a method for obtaining wetter steam than would issue from a simple delivery tube. That he is also aware of the lack of conviction which certain well-known and customary experiments carry to the mind of the beginner appears in his abandonment of the "Pin" methods in Light. He substitutes for them the use of a simple, ingenious, and inexpensive "Dark Box," by means of which all the usual experiments are performed with pencils of light, whose luminous tracks on paper are made apparent without the use of a darkened laboratory. A well-chosen set of examination questions is contained in the volume, which is to be recommended heartily. J. R.

CHEMISTRY

Advanced Inorganic Chemistry. By P. W. Oscroft, M.A. [Pp. viii + 504, with 158 illustrations.] (London: G. Bell & Sons, Ltd., 1915. Price 5*s*. net.)

This volume is a text-book intended for the use of boys in the upper forms of schools, and the contents can only be described as "advanced" on the assumption made by the author that its users have already taken a year's course in chemistry, both in practical work and theory. It is unfortunate that the author does not express any views as to the form such a first year's course might best take, in order that the use of the present volume should preserve the necessary degree of continuity in the exposition of the subject.

The author does not claim any originality in the arrangement of the chapters, and a perusal of the volume shows that the general treatment of the subject is along lines which have long been characteristic of a certain class of text-book.

Chapters I.—XVI. include the general principles of chemistry illustrated by reference to the composition of the atmosphere, properties of gases, water, etc. In this portion the atomic theory, the ionic hypothesis, thermochemistry, etc., are dealt with. Chapters XVII.—XXXVI. are descriptive of the more important

elements and their compounds. There is a short account of the phenomena of radioactivity, and also a chapter on Spectrum Analysis. Details of practical exercises and numerical problems are appended to most of the chapters.

In view of the assumption made by the author that the pupil has some knowledge of the principles of chemistry before the book is put into his hands, such features as the mention of molecular weights and the extensive use of chemical equations in the early chapters of the book without any real explanation of their significance are perhaps less open to criticism. For the same reason, however, the inclusion of many minor and elementary details would seem to be unnecessary.

The text is characterised by a regrettable lack of preciseness. There is a general looseness in the use of nomenclature, and the phraseology is in many places unscientific. These faults are particularly marked in the early chapters. For example, we find the term "carbonic acid gas," used exclusively in the first few pages, giving place to "carbon dioxide" without any reason being put forward for this change of style. The employment of such archaic terminology as "potassic chloride" might be overlooked if the author were merely consistent, but when the pupil is directed (p. 32) "to find the percentage of oxygen in potassic chlorate," and in the next paragraph is instructed to "crush some potassium chlorate in a mortar," he may be forgiven if some doubt arises in his mind as to the real identity of this substance.

In dealing with the detection of impurities (p. 3), the statement that "a pure solid will show under the microscope all its grains to be alike in colour, texture and *shape*," when there is no previous mention of crystalline and amorphous forms, is likely to lead to confusion.

These defects may be set down to a clumsiness in style on the part of the author; but the text contains in addition certain grave inaccuracies, which, in some cases, amount to positive misstatements of fact. For example, in a paragraph on the use of indicators (p. 47) phenol phthalein is stated to possess the advantage, like methyl orange, of not changing colour when carbonic acid is present.

With the author's personal views it is not always possible to agree. After devoting less than a page to the Periodic Law, he is of opinion that the periodic classification "is regarded more or less as a scientific curiosity, and really serves very little useful purpose." We would suggest that at least one useful purpose served by this classification is to form a sound basis for the comparative descriptive treatment of the elements and their compounds, and one on which the writers of several modern text-books for the use of young students have relied with marked advantage from the point of view of the teacher.

We fail to see that this book in its present form can be of real service to the class of pupil for which it is intended, especially in view of the existence of text-books of the same scope but written on sound modern lines. Very considerable amplification of the introductory portion is desirable, and the whole text requires most careful revision with a view to the removal of such inaccuracies as have been indicated.

ALEX. RULE.

A First Course in Practical Chemistry for Rural Secondary Schools. By WILLIAM ALDRIDGE, B.A., B.Sc. [Pp. xii+122, with diagrams.] (London: G. Bell & Sons, 1915. Price 1*s*. 6*d*.)

THIS is the first volume to be published of Messrs. Bell's Science Series for Schools and Colleges, and it is a happy augury for the whole series. The course

outlined is designed to teach the pupil the elements of General Chemistry through the medium of the rural phenomena which he finds around him. The setting-up of each experiment is clearly detailed and illustrated by a diagram where necessary, and when the scholar has successfully performed the experiment he is led to draw conclusions for himself. The author rightly lays stress on the necessity for making the pupil write a full description of each experiment from his rough laboratory notes and then stating his deductions, giving his reasons for arriving at such deductions.

Useful paragraphs in italics are inserted throughout the text with the object of drawing the teacher's attention to suitable elaborations of the experiments and of indicating the lines for their subsequent discussion.

The book is eminently practical throughout, and is obviously based on sound teaching experience. If the teacher follows out conscientiously the general principles of the course he cannot fail to inculcate in young students the proper scientific spirit of experiment and observation. The ground covered is considerable, the latter sections including elementary biological chemistry, but ample latitude as regards the sequence of experiments is allowed to the teacher. We doubt whether the whole course could be completed within the ordinary school year, when allowance is made for the mishaps and want of confidence of pupils new to scientific experimentation.

ALEX. RULE.

Practical Physical Chemistry. By ALEXANDER FINDLAY, M.A., Ph.D., D.Sc. [Pp. xvi + 327, with 104 figures in the text. Third edition, enlarged.] (London: Longmans, Green & Co., 1914. Price 4*s*. 6*d*. net.)

PROFESSOR FINDLAY'S text-book of practical physical chemistry is so well known to teachers and students of physical chemistry in this country that little more need be done than to draw attention to the fact that a new edition—the third—has appeared.

Several important additions have been made, such as the determination of vapour densities by Blackman's and by Menzies' methods, the analysis of binary mixtures by vapour density methods, measurement of molecular weights of dissolved substances by the lowering of vapour pressure, the determination of the solubility and hydrolysis of salts by conductivity measurements, decomposition and ionic potentials, and the solubility of gases in liquids. Several references are given to original papers.

A very important question is that of the heating of thermostats, and the author has devoted considerable space to its discussion. There can no longer be any doubt that for efficiency and regularity the electrical method surpasses the old gas-heating method. It is more costly to run, but on the other hand the life of the thermostat itself is prolonged indefinitely, and above all the control is more sensitive.

As regards the conductivity method of determining the solubility of sparingly soluble salts, the reviewer has found that silver chromate is rather useful, since it is sufficiently soluble to give good conductivity readings and at the same time its hydrolysis is negligible, which is not the case with the more frequently employed lead sulphate.

The book has already stood the test of extensive use, and with the extra matter now incorporated can be heartily recommended to all teachers of physical chemistry. The ideal combination appears to be "Findlay" for the actual course followed, and "Ostwald-Luther" as an indispensable work of reference.

W. C. McC. LEWIS.

Molecular Association. By W. E. S. TURNER, D.Sc. (Monographs on Inorganic and Physical Chemistry. Edited by ALEXANDER FINDLAY, M.A., D.Sc., F.I.C.) [Pp. viii + 170, with 6 figures.] (London: Longmans, Green & Co., 1915. Price 5*s*. net.)

THIS book contains the first comprehensive treatment in English of the important subject of molecular association in gases, solutions, and liquids. The mode of presentation exhibits a fine example of critical exposition, only possible indeed when the writer is himself intimately acquainted with his subject. There are eleven chapters, the contents of which may be roughly divided as follows: Molecular Association in Gases; Molecular Complexity of Dissolved Salts; Molecular Complexity in the Pure Liquid State and the Various Methods, Qualitative and Quantitatives Employed to Detect such Complexity; The Specially Important Case of the Molecular Complexity of Water, particularly in Relation to the Theory of Dynamic Allotropy; The Connection between Molecular Association and Physical Properties on the One Hand and Chemical Combination on the other.

As regards the question of association in gases there is little comment to make, the striking feature being the simplicity of the molecular state except in the case of sulphur, selenium, arsenic, and phosphorus.

Dr. Turner has rightly insisted throughout the book upon the true relation of association to molecular "normality" and to dissociation. One not inconsiderable feature is the impression made on the reader's mind that association is by no means to be regarded as an abnormal and exceptional state of things negligible in generality and importance compared, say, with the phenomenon of dissociation. On reviewing the subject as a whole in the light of this book, the wonder is rather that normal molecular state is realised as frequently as it is in view of the conclusion brought home again and again that whatever property is taken to be truly characteristic of a given substance, its molecular weight at any rate, in spite of its apparently specific nature, cannot any longer be regarded as such.

In the chapter upon the molecular complexity of dissolved substances we have a comprehensive and ordered review of a set of phenomena varying much in detail and, one would think, not easy to co-ordinate. Dr. Turner has made it one of the best chapters in the book. Several quite surprising conclusions are arrived at, for example, that electrolytes as a class (organic ammonium salts in particular in various solvents) are strongly associated substances. We have become so accustomed to think of dissociation in connection with electrolytes that association has never received the attention it deserves. Another striking point made by the author is the resemblance between association and the colloidal state. In view of Perrin's work we have been forced to conclude that the distinction between a homogeneous and a heterogeneous solution is one of degree and not of kind, and it would seem that in the phenomenon of molecular association there exists the connecting link making the transition a gradual one. This is borne out by the fact that association of a substance usually occurs in a solvent in which the substance is but little soluble. A further conclusion, which, if substantiated by subsequent work, ought to prove of very great importance, is that the degree of association of a salt is *increased* by the addition of a second salt, a phenomenon, as the author points out, having something in common with the effect of electrolytes on colloids.

In dealing with the question of the variation of the apparent molecular weight with concentration it is shown that five different effects may be anticipated (and have actually been realised) supposing molecular combination of solute and solvent to be possible as well as association of the solute. To judge what is really

happening in such molecular weight measurements the author very properly insists upon the necessity of examining the solid which separates out, as otherwise quite erroneous conclusions may be drawn. As regards the physical properties of solvents in relation to association of the solute it is pointed out that although association is almost general in solvents of low dielectric constant, it is far from absent when the dielectric constant is high. Thus quite a number of substances, such as certain acids, phenols, amides, and anilides, are believed to be markedly associated in water, a solvent of exceptionally high dielectric constant; and again it is found that those substances which are least soluble in water exhibit the greatest tendency to associate. This chapter is extended by a very complete list of molecular weight determinations given in the form of an appendix.

When we come to the problem of molecular complexity in the pure liquid state we enter the field of greatest uncertainty. Dr. Turner has made the best of it, but the impression which remains is that our knowledge here is utterly unsatisfactory. About a dozen methods which have been employed are quoted and illustrated. In many cases the results are roughly concordant, in many cases they are not. The methods are empirical or at best semi-empirical. The reviewer is inclined to think that by paying less attention to capillary methods (with the exception possibly of Bennett's) and by concentrating on latent heat measurements some hope of success is held out, but even here, as Dr. Turner points out, before any real advance can be made we must have a clearer conception of the phenomenon of condensation. "To accumulate empirical methods of testing liquids is fruitless." The chapters devoted to this subject are, however, full of suggestiveness and the general review with which they include is specially important.

Several other points might be commented upon, but enough has been said to illustrate the scope and nature of the book.

W. C. McC. Lewis.

Directions for a Practical Course in Chemical Physiology. By W. Cramer, Ph.D., D.Sc. [Pp. viii + 102. Second edition.] (London: Longmans, Green & Co., 1915. Price 3*s*. net.)

This is an essentially practical book, for, as explained in the preface, the subject-matter is limited to experiments and deductions from the experiments, and is not meant to supply the full and ordered information obtainable from a text-book of physiological chemistry. The book is divided into three parts dealing respectively with animal and vegetable tissues and fluids, digestion, and metabolism; the experiments are well chosen and instructive, and are very clearly set forth, and even the complex question of the coagulation of blood has been dealt with in a lucid manner. The plan adopted by the author of following the descriptions of experiments by questions supposed to be answered by the student is disappointing; would it not have been better to supply the correct answers as well?—for the student who can do this for himself is not likely to be benefited much by the questions, whereas the other type of student would have acquired useful instruction from the solution of problems which he himself was unable to sölve.

P. H.

The Chemistry of Colloids and some Technical Applications. By W. W. Taylor, M.A., D.Sc. [Pp. viii + 328, with 7 illustrations.] (London: Edward Arnold, 1915. Price 7*s*. 6*d*. net.)

Although much has been written about colloids, especially during the last few years, some confusion still remains with regard to the true conception of the term

colloid, and in order to clear the ground at the outset the author emphasises the fact that Graham's original idea of crystalloids and colloids as being different *kinds* of matter is erroneous, and that in reality they are only different *states* of matter. That this latter view is the correct one is evidenced by the fact that so many substances can occur in either of the two forms according to the conditions under which they are produced, and, indeed, von Weimarn has mathematically formulated the factors which cause a substance to assume the one or other state. In dealing with colloids we have to do with a two-phase system, one of which is composed of small separate volumes and is known as the disperse phase, while the other, which is continuous, is known as the dispersion medium; supposing that both the disperse phase and dispersion medium are liquid, we have what is known as an emulsion, whereas when the disperse phase is solid we have a suspension; as special cases of these two types we have the so-called emulsion and suspension colloids, or emulsoids and suspensoids, in which the dimensions of the disperse phase are very small indeed, in fact ultramicroscopic.

Concerning the misconception which still not infrequently exists with regard to the diffusion of colloids, the author says: "It should be particularly observed that Graham's results do not show that, as has been occasionally assumed, there is *no* diffusion and dialysis of sols. Though his classification into crystalloids and colloids is based upon the differences between them in this respect, he everywhere gives figures for the dialysis and diffusion of colloids. . . ."

The book is divided into four parts dealing respectively with the general properties of colloids, methods of preparation, adsorption, and applications of colloid chemistry. In the second part will be found a description and discussion of von Weimarn's theory, as well as practical details for preparing a number of typical colloidal solutions. Many interesting applications of colloidal chemistry to sewage and water purification, soil, tanning and dyeing are given in Part IV.; but with regard to the theory of dyeing we find the somewhat discouraging statement that "the main effect of the advent of colloid chemistry has been to render the question still more complicated by providing yet another set of explanations to those already existing, without increasing materially the prospect of a final answer." In conclusion it should be stated that the book is replete with information set forth in a lucid manner, and will be welcomed by students and teachers alike as a valuable contribution to the somewhat sparing list of text-books on colloids in this language.

P. H.

GEOLOGY

The Problem of Volcanism. By JOSEPH P. IDDINGS, Ph.B., Sc.D. [Pp. xvi + 273, with a folding map and numerous plates.] (Newhaven: Yale University Press; London: H. Milford, Oxford University Press, 1914. Price 21*s*. net.)

UNDER a bequest in memory of Mrs. Hepsa Ely Silliman, lectures are delivered annually in Yale College on some branch of natural science, a wide view having been taken by the testator as to the moral effect of such discourses. Prof. Iddings, in 1914, chose volcanism as his subject, and the present handsome volume embodies his account of modern research on igneous phenomena, and his reasoned argument as to their causes.

It is not unfair to mention the illustrations at the outset, since we owe them in their present form to the author's judgment and selection. They mostly

represent unfamiliar scenes, such as the four views of Taal in Luzon, or the superb cone of Mayon in the same island, or the fascinating details of Tengger Crater, Java (fig. 79), or the beautifully moulded cliffs of dissected rhyolites in Colorado (fig. 77). We are spared the formal repetition of examples known to Scrope and Lyell, and even our old friend, the nebula in Andromeda (fig. 25), gains in glory by its representation in a fine photograph from the Yerkes Observatory. The massive cloud above Mount Pelée (fig. 4) is perhaps not correctly described as consisting only of "heated gases"; but this reproduction from the work of Lacroix is very welcome. The book is a treatise for readers who have got beyond the stage of regarding "earthquakes and volcanoes" as inseparable occurrences, designed to provide "copy" for the newspapers. It takes us back to origins, and far below the surface on which lavas are outpoured. The planetesimal hypothesis, which has so justly appealed to the scientific imagination, is traced, in its varied aspects, from Kant in 1785 to Lockyer in 1890 and T. C. Chamberlin in 1897. The possibility that the primordial crust of the earth was never hot is faced on p. 51, the accretion of nebular particles having perhaps progressed at such a rate that the temperature of the surface of the growing sphere was not raised by the process.

The observable facts of volcanism are features of the "lithosphere," which is defined as "so much of the outer portion of the earth as is composed of rock material having the physical properties of the rocks exposed to view at its surface.' The author warns us (p. 90) that the fusibility of the surface-rocks, as determined experimentally, is not a safe guide in estimating their behaviour underground. The liquid and gaseous components that may be present "materially modify the solubility of the whole system." We think that such considerations offer an answer to the doubts of Iddings, Harker, and others as to the likelihood of large areas of the lithosphere becoming melted up by attack from igneous cauldrons; but Prof. Iddings himself is not willing to be persuaded (pp. 113 and 213). He repeats in two places the statement that the composition of granite is not altered by its being intruded into different kinds of rocks, such as limestone, sandstone, shale, etc. This argument has been met by Goodchild's suggestion of "stoping," which is strongly developed by Daly, and of which there is extensive evidence in the field; but work along the margin of a granite mass that crosses strata of varied nature surely reveals features of actual absorption, such as local enrichment in quartz from quartzite or biotite from hornblende-schist, which suggest that extensive diffusion may have gone on.

This, however, is a by-path of volcanism, and we may return to the point (p. 103) that a small amount of water in rock magmas renders "them fluid at temperatures considerably below the melting point of the rock minerals; and slightly more water probably renders them highly liquid and capable of penetrating extremely thin fissures in heated rocks." When anhydrous minerals separate out in the cooling magma, water accumulates in still larger proportion in the uncrystallised material that remains, retarding crystallisation until comparatively low temperatures are reached. Melts of quartz, orthoclase, or albite (p. 106) are so viscous near the temperature of crystallisation that something which increases molecular mobility is necessary, to avoid the production of a mere glass. The liquidity of lime-soda and lime felspars, on the other hand and of augite and olivine, at temperatures just above their melting points, allows of their ready crystallisation.

The heterogeneous composition of that part of the lithosphere from which we draw our lavas is presumed from their local variety, and absorption of differen-

tiated material on their upward path is not regarded as a probability. The differences in density of underlying magmas are correlated, on the isostatic principle, with the larger hollows and elevations of the earth's surface.

The influence of radium in heating the earth's crust comes naturally under discussion, and it is pointed out (p. 141) that there is not a proportionate rise of temperature in the rocks of a district such as Joachimstal, where radioactive minerals are abundant. We have touched on a few of the interesting problems that must be faced, as Prof. Iddings shows, by the modern student of volcanoes. The superficial manifestations, where molten silicates at temperatures of 1100° C. appear among the outer rocks of the crust, point to the insecurity of the solid lithosphere, and serve in part to explain its fracturing in the present and in the past.

G. A. J. COLE.

An Amateur's Introduction to Crystallography (from morphological observations). BY SIR WILLIAM PHIPSON BEALE, Bart., K.C., M.P., Treasurer of the Mineralogical Society. [Pp. viii + 220.] (London: Longmans, Green & Co., 1915. Price 4*s*. 6*d*. net.)

It has often been remarked that the efficient scientific amateur is more common in the British Isles than in countries where scientific knowledge is more generally diffused. The explanation seems to be that in the latter countries men of scientific tastes find a wide field of appreciative employers, and so drift into the professional class. But the amateur, the man who works because he is fascinated by his subject, plays a very great part in the mental uplifting of his State. He is less likely to dogmatise, he is more open to new conceptions, than those who develop their theses among the traditions of the schools. On the other hand, he is not likely to be so resourceful in experimental methods as those who have ample laboratories at command, and he usually turns to natural history rather than to the physical sciences.

Sir William Beale rightly regards mineralogy as a branch of natural history, and he remarks that crystallography is to mineralogy what anatomy is to zoology. He attracts the reader at the outset (p. 8) by developing crystallographic principles from a fragmental specimen of a homogeneous substance, on which only a few faces can be seen. He proceeds to refer these faces to a system of three axes drawn respectively parallel to "any three edges between these faces not in one plane and no two of the edges being parallel." The point of intersection of these axes is shown to be quite independent of the centre of a symmetrical crystal. The natural development of crystal-faces of the same form on very different scales thus presents no initial difficulty. The crystal selected is triclinic, and forms of greater symmetry are approached later in the volume. Those of us who were brought up on other methods, and were led to regard the axes of reference as something possessed by the crystal about which it was built up, will welcome the author's natural and morphological treatment (p. 54). The insistence on the derivation of the crystallographic axes from observed edges renders the use of three equally inclined axes in the hexagonal and trigonal systems easily understood. Many teachers must have found the immense convenience of starting with forms consisting of two parallel faces only (p. 59), or even with the "pedions" of P. Groth. The older mineralogists were attracted by the wonderful symmetry of the cube and the forms derived from it by suitable truncation; and their method of approach has hampered students far into our own time.

The possibility of the determination of the system of a crystal by symmetry, in

place of the discovery of edges which indicate axes of reference, is properly pointed out on p. 66. It is questionable if the treatment of forms of lower symmetry within a system as "merohedral" types is wise from the point of view of natural history. Sir William Beale clearly recognises (p. 65) that the constituents of such crystals are incapable of producing more symmetrical forms. While "hemimorphism" remains a useful term to express dissimilar termination at opposite ends of a crystallographic axis, the conception of "hemihedrism" and "tetartohedrism" (p. 113) may well be abandoned, when we consider external form as dominated by the internal space-lattice. Hemihedrism thus seems to belong to geometry rather than to natural history; and, as the author points out (p. 115), pioneers like Romé de l'Isle and Hauy dwelt on the importance of the type of homogeneous arrangement throughout a crystal as controlling the faces that could be produced.

Two appendices are added, one on methods of calculation and one on the drawing of crystals. The author's illustrations are clear, and the forms are accompanied throughout by their stereographic projections. Something seems to have gone wrong in fig. 65*a*; but it may be presumed that the student will be encouraged to use models as well as drawings, and to read the symmetry from the disposition and number of the planes. The book, as might be expected, is handsomely produced, and we notice very few misprints. The round O on p. 10 to express an infinitely small index should be 0, and, conversely, Naumann's symbol on p. 32 should be 0P instead of OP, while O and not 0 should stand for the octahedron. The author employs "pinakoid" for a single plane only (pp. 21 and 32), and individual planes should not, we think (pp. 32 and 33), be described as "prisms," "domes," and "pyramids." The naturalist would like to see more emphasis, with the printer's aid or otherwise, laid on the law of rational indices on p. 10. The student, however, who has progressed for some distance on more conventional lines will welcome this book as lifting him out of many difficulties.

G. A. J. COLE.

PALÆONTOLOGY

American Permian Vertebrates. By SAMUEL W. WILLISTON, Professor of Palæontology in the University of Chicago. [Pp. v + 145, with 32 text-figures, frontispiece, and 38 plates.] (London: Cambridge University Press. Price 10*s*. net.)

IN such a rapidly advancing science as vertebrate palæontology it is not a little difficult to review a book which has already been in circulation for four years, especially when, as in the present instance, the author has subsequently issued what is in many respects a companion volume (*Water-Reptiles*, Chicago, 1914), which to some extent covers part of the same ground, and contains modifications or expansions of views tentatively enumerated in the original work. The reason for such delay is that the edition for the United Kingdom has only just been issued by the Cambridge University Press, to which the editor is indebted for a copy.

As stated in a paragraph in my article on vertebrate palæontology for 1911, published in the April number of SCIENCE PROGRESS for 1912, Prof. Williston's volume contains what were at the time of its original publication several emendations in regard to the classification of primitive amphibians and reptiles, notably the separation of all the groups of the latter with complete skull-roofs, such as the North American *Pariotichus* and the South African *Pariasaurus* and *Cistecephalus* from the typical mammal-like reptiles and their inclusion in the

Cotylosauria. And it is important to notice that these departures from earlier classifications have stood the test of time, and have not only been maintained and amplified by Prof. Williston himself, but have been adopted by other writers. Points in osteology which were left uncertain in the volume before us have received a definite interpretation in its successor, as exemplified by the determination of the homology of the two coracoidal elements in the shoulder-girdle of Permian reptiles and monotreme mammals.

It is perhaps hard to decide whether the Permian land-vertebrates of North America or those of South Africa have thrown most light on the relationships between mammals and reptiles on the one hand, and between reptiles and stegocephalian amphibians (and thus with fringe-finned fishes) on the other; but it is safe to say that each fauna is complementary to the other; and that without the evidence afforded by the former, that of the latter would be incomplete and in many respects indecisive. A feature common to the South African and North American deposits is the enormous abundance in certain spots of the skeletons and bones of the reptiles; a notable instance of this occurring in the so-called *Cacops* bone-bed of Wichita Valley, where in one spot skeletons were found packed like sardines in a tin for a thickness of two feet.

The value of American Permian vertebrates in helping to solve the puzzle of mammalian descent is incalculable: and of those who have tried to fit a key to the lock, no one has been more successful than Prof. Williston.

(*The late*) RICHARD LYDEKKER.

ZOOLOGY

Behavior: an Introduction to Comparative Psychology. By Prof. J. B. WATSON. [Pp. xii + 439, with 72 figures.] (New York: Henry Holt & Co., 1914.)

THE author of this volume is very firmly convinced that human psychology "has failed to make good its claim as a natural science," and does his best to convince the reader that this is so. This statement will certainly not meet with general acceptance without challenge, but we are not prepared to enter into it here. The author has certainly gone far towards showing that psychology, as a study of behaviour, opens a wide and fascinating field of work for psychologist and biologist alike, and this book will serve a useful purpose if it calls attention to a subject not much studied in this country.

The aims of the "behaviourist" and the type of problems that he encounters are clearly set out. Considerable space is devoted to the kinds of apparatus used in the experiments, and although this does not make interesting reading it is unquestionably useful if one wishes to repeat or extend the investigations, as are also the short bibliographies given at the end of each chapter. The succeeding part of the book is devoted to studies on instinct, habit formation learning, training, and the activities of the various senses.

To the biologist the chapter devoted to the origin of instincts is of particular interest, involving as it does a brief inquiry into heredity, inheritance of acquired characters, etc., and the author concludes that since "the activities of organisms must be considered as the functioning of definite structural elements, the problem of the origin of new reflexes, of new instincts, and of new possibilities of habit formation become (*sic*—becomes) one with the problem of the evolution of morphological characters in general." This conclusion is interesting in view of the much discussed question whether function preceded structure in the course of evolution or *vice versa*, and the author adduces good evidence in support of his view.

The chapter on the limits of training in animals also calls for attention, although no definite conclusions are reached in it. Clever Hans and the horses of Elberfeld have attracted a good deal of public notice, and the investigations of Prof. Stumpf on the former and Claparède on the latter are ably discussed. It seems highly probable in both cases that a great number of the correct answers to arithmetical problems, etc., are due to the trainer giving some sign to the horses. The signs may not be given consciously or intentionally, but it is certainly not to be regarded as accidental that the percentage of correct answers drops from between eighty-five and ninety to between eight and twelve when the questioner himself does not know the result. Making due allowance for this, however, we can still see that the horse is capable of a fair amount of "intelligent" work when dealt with sympathetically. Such is the case, though more marked, in monkeys, and a very high state of training was reached in the case of the well-known chimpanzee Peter.

With the exception of those chapters that are intended for reference, and so indicated in the introduction, the book is very interesting. It is a thoughtful and stimulating work, and in it the biologist will find many matters dealt with from a point of view that is generally entirely overlooked in ordinary works of biology or natural history.

C. H. O'D.

The Determination of Sex. BY L. DONCASTER, Sc.D. [Pp. xii + 172, with 22 plates.] (Cambridge: at the University Press, 1914. Price 7*s.* 6*d.* net.)

THE fascination of the problems suggested by the title of this work is, to the biologist, perhaps only equalled by that of the origin of living matter itself. Both are constantly before him, and the present one meets him in some form or other at practically every turn.

One thing emerges quite clearly from reading this suggestive and useful book, and that is, that at the present time there is no theory that will fit all the facts now available. Thus we find in the Crustacea an alteration in the physiological condition of the body appears to be able to bring about an alteration in the primary sexual organs, while in the Vertebrata the reverse seems to be the case, and it is probable that an alteration of the primary organ of sex by means of experiments will bring about an alteration in the general condition of the body. The removal of ovaries and substitution of testes in a mammal produce a marked effect on the structure and behaviour of the individual, whereas the same operation in insects makes no appreciable alteration in either. Furthermore, while in certain cases the evidence undoubtedly points to the presence of two sorts of ova, and so throws the main part of sex determination upon the mother, in other cases the evidence just as strongly indicates two kinds of spermatozoa. All these and many more points are lucidly dealt with in the present book.

It is cautiously written; indeed, perhaps too much attention is paid to the exceptions, important though they be, for after all the main point is to find general statements that will apply to the majority of cases. A generalisation is not necessarily upset because of a few exceptions, and the reasons for their apparent breach of uniformity may be discovered when they are investigated more fully.

A few statements call for note. On p. 105 we find: "If the hormone hypothesis is correct, it should be possible to cause the appearance of the secondary sexual characters in castrated animals by injecting extract of the testis or ovary." This is not necessarily so, for it is conceivable that these organs contain only the mother

substance which requires to be released in some way or other before it can have any effect. It is stated on p. 125 that only among myxinoids is normal hermaphroditism met with in vertebrata. This is not strictly accurate, for in the Bufonidæ (Toads) the males possess a rudimentary ovary, Bidder's organ, which may be as large as the testis, although probably not functional. Throughout the whole of the Urodela and Anura some degree of hermaphroditism is by no means uncommon: ova may be developed in the substance of the testis, or one testis may be replaced by an ovary: and more or less well-developed oviducts are often met with in male Amphibia. These facts suggest that the balance between the tendencies to make the individual male or female in this group is fairly even and readily upset—a possibility further borne out by Hertwig's experiment wherein "indifferent" individuals were produced, and one that makes it impossible to press too far the application of the results obtained in this class to other classes of vertebrates.

The author concludes that "Sex is dependent on a physiological condition of the organism, a condition depending on the interaction of certain chromosomes with the protoplasm of the cells, and therefore determined, in the absence of other disturbing factors, by the presence or absence of these particular chromosomes." Before any advance is made on this, much work will have to be done, work that will doubtless be stimulated by Dr. Doncaster's well-arranged, thoughtful pages.

C. H. O'D.

Nerves. By David Fraser Harris, M.D., C.M., B.Sc., etc. [Pp. x + 256, with 8 figures.] (London: Williams & Norgate, 1915. Price *1s.* net.)

The Home University Library has already produced a number of good books in which specialists have set out in non-technical language the salient facts and theories of their subjects. The present volume is a worthy addition to the series and in it the functions of nerves and of the nervous system and the parts they play in the ordinary activities are dealt with in a clear and readable manner. Slips have crept into the early chapters here and there. On p. 12 we read that "there is no neural socialism," a quite meaningless phrase that is intended to suggest that the various nerve cells are functionally differentiated. As an example of a reflex action it is stated that if a frog has its brain destroyed it will still change its skin colour in response to differences in the intensities of the light reaching the eye. If this be a fact, then it is not a reflex action, as that part of the reflex arc intervening between the optic nerve and the cutaneous nerve would have been destroyed with the brain. On p. 107 it is stated that "Instincts are inherited habits." This is supported by no evidence whatever, and is a statement that has long since been discredited.

The last chapters are inteiesting and the book as a whole is well worth reading.

C. H. O'D.

BOTANY

Practical Field Botany. By A. R. Horwood, F.L.S. [Pp. xv + 193, with 20 plates and 26 other illustrations.] (London: Charles Griffin & Co., 1914. Price *5s.* net.)

One hardly knows in what category to place this book. The author is obviously, sincerely, and earnestly anxious to foster interest in the study of plants, and has apparently written this book in a great hurry. He has much to say about "the

New Botany," alludes mysteriously to a "British Empire Naturalists' Association" without satisfying the reader's natural curiosity to learn more about this body, and has so much to say about herbarium and museum methods that we are lost in wonder at his choice of a title for the book. Shorn of such irrevelant matter, including much that is perfectly pointless (though doubtless the Cambridge Botany School, the British Museum, and the numerous other institutions as well as persons whose names are scattered through the book will be grateful for the approval showered upon them), but also, it must be admitted, a certain amount of useful information brought together in this somewhat incoherent volume, the amount of letterpress that really deals with practical field botany is rather small and assuredly not such as is likely to be of interest or service to either of the two classes of readers for whom the author tells us the book is intended—"the amateur with little or no expert knowledge" and "the student who has some expert knowledge and an object in view." The "outline of plant formations" (pp. 122–80), which, together with certain parts of the first chapter, constitutes what we may call the residue that realises the book's title, can hardly meet the needs of the first class of reader, while those of the second class are already much better provided for elsewhere.

We suppose this book must be classed under the comprehensive title of "nature study," and though most books of this kind are not worth reviewing at all in a journal devoted to recording the progress of science, we may take this opportunity of protesting once more against the flooding of the market with trashy books on natural history by publishers—often enough of deservedly high repute in other respects—who will not take the precaution of consulting competent readers before accepting and publishing what is offered to them. One hesitates to say too harsh things about the authors of these books, who, on the ground of youth or inexperience or mistaken enthusiasm, or simply as victims of scribbling itch, may deserve pity rather than blame; or even about their publishers, who are simply turning out anything that looks likely to sell—partly owing to the indiscriminate praise lavished on every new book on "nature study" in the review (and advertisement) columns of the newspaper press, but mainly because of the apparently insatiable demand, arising from the increasing interest in natural history, for books on this subject. Surely, however, either author or publisher or both should see that the letterpress (the illustrations are usually good, often extremely so) is read and, if necessary, thoroughly revised by one or more competent referees. This policy would pay, too, for good books on natural history may be expected to find a continued sale long after bad ones have met their natural fate. And there is ample room for really good books on "nature study" topics; for instance, a good book on practical field botany is rather badly wanted.

F. Cavers.

Soil Conditions and Plant Growth. By Edward J. Russell, D.Sc. [Pp. viii + 190, with 9 illustrations. Second edition.] (London: Longmans, Green & Co., 1915. Price 5*s.* net.)

One learns with little surprise that a second impression of this admirable work was called for shortly after the first was published in 1912, and that the important recent additions that have been made to soil-science—largely as the outcome of the investigations of the author and his colleagues at Rothamsted—have necessitated the writing of a new chapter in the present third impression (new edition) on the relationship between the micro-organic population of the soil and

the growth of plants. The second edition differs from its predecessor mainly in the inclusion of this chapter, but various smaller sections have been interpolated dealing with recent developments of other parts of the subject.

Dr. Russell's book occupies a unique position in the literature of agriculture, while it has also become indispensable to workers on the ecology of plants, as well as to plant physiologists. It is impossible in the space available to analyse the contents of this invaluable monograph, which is, moreover, already too well known to those interested in soil investigation to require further commendation. The new chapter gives a detailed and lucid account of the knowledge already gained, besides indicating the gaps which remain to be filled by co-operative investigations from various sides, regarding the remarkable inter-relationships between the various classes of soil micro-organisms on one hand and the relations between the entire micro-organic soil population and the growth of plants. Recent work has opened up an attractive and important field of reseach and has indicated not merely the unexpectedly large number and variety of the constituents of the soil flora and fauna and their remarkably wide distribution in the soil of all parts of the world—the latter fact, not commented on by the author, has been proved particularly in the case of soil fungi as well as soil bacteria, and may well be found true of the soil flagellates, protozoa, etc.—but the extreme complexity of their relations to each other and to plant growth. These relations have been as yet only in small part worked out, partly owing to the difficulty in isolating the organisms and the greater difficulty in determining their actual behaviour in the soil itself and in applying the results obtained by their isolation and culture to the unravelling of the problems presented by the soil population. The author makes a provisional segregation of the organisms into three groups—those affecting plant growth directly or indirectly, those not acting on plants but on the organisms of the first group, and those (none yet known with certainty) which act neither on the plant nor on the first and second groups. The nature and behaviour of each group are discussed, and details are given of the remarkable results obtained by partial soil sterilisation.

It might be suggested that in the case of monographs like those of the series to which this book belongs a practicable plan would be to publish at fairly frequent intervals inexpensive unbound supplements giving additions and corrections resulting from current investigation of the subject, either instead of or (in fairness to possessors of the first issue of the book) in addition to a new edition. Such supplements would be in harmony with, and further considerably, the object aimed at by the editors of these invaluable monographs, and would afford a satisfactory solution of the question that must present itself in the case of branches undergoing rapid development—whether to allow the book to get out of date or to issue new editions at relatively short intervals.

F. Cavers.

All About Leaves. By the late Francis George Heath. [Pp. ix + 226, with 4 coloured plates and 89 other illustrations.] (London: Williams & Norgate, 1914. Price 4*s*. 6*d*. net.)

Like the rest of the late author's numerous books, this posthumously published volume indicates at any rate his ardent love of nature, his ingenuous and reverent frame of mind, and the tinge of mysticism which caused him to dwell at every turn on the mystery of things. The letterpress can hardly be made the subject of criticism, however, and we need only add that the book is attractively illustrated,

chiefly from photographs, while the type is of a strikingly clear full-face kind that one would like to see more extensively employed.

F. CAVERS.

MEDICINE

An Introduction to the Study of Colour Vision. By J. H. PARSONS, D.Sc., F.R.C.S. [Pp. viii + 308, with 75 illustrations.] (Cambridge: at the University Press, 1915. Price 12*s.* 6*d.* net.)

THIS book is a well-written compilation of attractive appearance. Its chief defect is that the author appears to possess so very little personal knowledge, as opposed to book knowledge, of the facts bearing on disputed points. This is particularly applicable when the author deals with my work. Though he has given my views correctly his comments are so wide of the mark and show such an entire lack of appreciation of the essential details that they are very misleading. The author could not have written as he had if he had spent a single hour in my laboratory. This even applies to his criticisms when he has adopted my conclusions in direct opposition to his previous statements, as, for instance, in the use of colour names. In a book published in 1912 he states, with reference to practical tests, "The fundamental axiom in colour testing is that no importance should be attached to the naming of colours." He now writes: "In practical testing the object aimed at is the determination of whether the examinee recognises red, green, and white lights as red, green, and white lights respectively, and it is obvious that the names which he applies are of great importance." No explanation is given of this change of front. Though as far as I am aware the author has never even looked through my spectrometer, he cavils at the observations made with this instrument and the classification of colour blindness made with it. This classification is independent of any theory of colour vision, and the facts ought to have been given a prominent place in the book. If a large number of persons be examined with the spectrometer a certain proportion will be found to see two colours only in the spectrum with an intervening colourless region which varies in size with different persons. The fact that so many degrees of dichromic vision exist is one of the milestones of the subject and requires explanation on any theory. It would be just as sensible to object to a classification of potatoes as such because they are not all of one size. The trichromic when examined with the spectrometer say they see three definite colours in the spectrum—red, green, and violet, the yellow and orange regions being designated as red-green and blue as green-violet. The spectrum is also divided into about ten monochromatic regions instead of eighteen. The monochromatic region is a fundamental physiological fact. The author would do well and would make the book of much greater value if he would for another edition ascertain for himself the truth or otherwise of the contradictory statements of different authors so that he can give the facts in accordance with his own knowledge. As far as I am concerned I shall be very pleased to give him every help in my power. It is hardly fair to object to any fact when it has not been examined by the critic. The intense reluctance which compilers have to ascertain for themselves the truth of the simplest statements is responsible for the continued propagation of error and the great difficulty which exists in the general recognition of the simplest new facts when these are opposed to previous statements.

There are some grave omissions: the facts of colour adaptation are not even mentioned. The book contains too much theory, and in some cases the theory

can be disproved when judged only by the facts given by the promulgator; as, for instance, when a case of so-called partial red blindness, ·2 red sensation, is described, and it is stated that any amount of red can be added without altering the appearance of an equation. If the theory were true a definite amount of red should give a sensation similar to one-fifth of this amount to the normal sighted.

F. W. EDRIDGE-GREEN.

Malay Poisons and Charm Cures. BY JOHN D. GIMLETTE, M.R.C.S., L.R.C.P., Resident Surgeon of Kelantan, one of the Protected Malay States. [Pp. vi + 127.] (London: J. & A. Churchill. Price 3*s*. 6*d*. net.)

THE workings of the human mind have a perennial interest. Men form beliefs, and change them for others, only to be discarded in their turn. And when we weary of trying to fathom the humanity around us we turn to the contemplation of men in far-off climes and in far-off times. It is this that gives the interest to the book before us, which tells of the superstitions of the half-savage Malay. What to him is of common and ordinary occurrence has for us the charm of the fantastic, because it is so remote from our own experience. It shows how the Malay physician, whom he calls "Bomor," seeks to cure his patients by the recitation of spells set in rude verse and displays the cunning with which he manufactures from plant and fish subtle poisons for the purposes of crime. He seemingly thinks nothing of using every art to introduce deadly decoctions into a neighbour's food so that he can afterwards commit a petty theft. Some of the prescriptions for medicines, used sometimes alone and sometimes as an accompaniment to the all-important charm, have to our ears a spice of the ludicrous. To quote one of these:—"Take the bones of a whale, the solid casque of a horn-bill, a sea porcupine's spine, a stag's horn, and the horn of a rhinoceros, and rub them down together in hot water; the draught is then administered internally." Another sovereign antidote for poison is "liquid opium mixed with the ashes of a cat's whiskers"! Fancy poor puss having to part with her most cherished ornament for such a futile end.

> "Verily, I know Smallpox! that in the beginning
> Thou didst dwell formless in the depths of Hell,
> Whence issuing thou didst enter the children of Adam in visible form,
> Seven brothers were ye in all,"

is the beginning of a lengthy charm to cure smallpox. But while one smiles at the superstitions of the savage, one is sometimes tempted to wonder if civilisation has progressed so very much after all, for it was not so very long ago that doctors attempted to cure plague by laying pigeon's flesh on the buboes. Like a kaleidoscope we produce ever-fresh forms by the clashing together always of the same pieces.

But however entertaining this little work may be to the ordinary reader, it is primarily for the medical man that it is designed. As the author says in the closing words of his Preface, "The witchcraft of the 'medicine-man' is always of general interest, but the investigation of Malay medicines, poisons, and their antidotes is of special scientific interest. It presents a large field for medical research, the ground of which is hardly broken in the following pages." In spite of the modesty of the last sentence, much has been compressed into the compass of so small a work, and the author has classified his material admirably. After the opening chapter in which he describes the work of the native medicine-man in

relation to clinical medicines, the other chapters are devoted to the setting forth of the different poisons manufactured from fish, animals, plants, and inorganic matter respectively. The scientist will no doubt derive from these pages the "special interest" he predicts.

Leper Houses and Mediæval Hospitals.. Being the FitzPatrick Lectures delivered before the Royal College of Physicians, London, November 5 and 10, 1914. By CHARLES A. MERCIER, M.D., Fellow of the College. [Pp. 47] (London: H. K. Lewis. Price 1*s*. net.)

THESE two lectures are reprints from the *Glasgow Medical Journal* of February 1915, and appear together as one pamphlet. The little work will appeal to a wide public because the lecturer has taken the subject more from the historical than the medical standpoint and has omitted everything of a technical nature. He traces the growth of our modern and specialised hospitals from their early beginning four centuries B.C., when their object was primarily one of hospitality, as the word itself implies. He shows us how they were afterwards more used as leper houses, that disease being so rife in the early ages, and when leprosy died out in Europe about the sixteenth century were converted into houses for the poor and those sick of various illnesses. He puts forward a new view of the mysterious disappearance of that fell disease that has puzzled so many, which is that tuberculosis, as prevalent in these days as leprosy was then, is but another form of the same complaint. The whole subject at first sight would appear to the lay mind as rather a specialised and uninteresting subject, but Dr. Mercier has contrived most cleverly to give his readers quite a graphic picture of the mode of life in bygone ages, and has enlivened the matter with occasional touches of sarcastic humour. For instance, in explaining how the ancient hospitals were always provided with plenty of priests to look after the welfare of the souls of the sick, he first quotes from an old manuscript: "This" (the Hospital of the Blessed Virgin Mary in Leicestershire) "was a sumptuous monastery, entirely built by the said Duke for canons regulars, with an hospital well disposed and distinguished for men and women and so well provided for, that no hospital in England was more commodious," and then adds, "We may well believe this, for its inmates were a dean, twelve canons prebendaries, twelve vicars, with other necessary attendants, as also ten infirm poor people and ten lusty women to serve the infirm. It seems an intolerable deal of ecclesiastics to a poor halfpennyworth of infirm." On the whole these two lectures provide much useful information in an easily digested form.

A Treatise on Hygiene and Public Health, with special reference to the Tropics. By BIRENDRA NATH GHOSH, F.R.F.P.S., and JAHAR LAL DAS, L.M.S., with an Introduction by Col. KENNETH MACLEOD, M.D., LL.D., F.R.C.S. [Pp. xvi+394. Second Edition.] (Calcutta: Hilton & Co. 1915. Price 4 Rs. or 6*s*. net.)

WE are glad to see that this useful little book has already reached a second edition. It is a work of the usual type and contains the usual matter, but is certainly comparatively well done. It will be of distinct value to Sanitary Officers, Assistant Surgeons, Hospital Assistants, private persons, and, indeed, to many who have not acquired a complete professional training on the subject. To those who have, the work is of course somewhat too short to add much to their knowledge. The work makes no pretence to deal with the larger theory of

the subject of hygiene—which, indeed, is scarcely adequately dealt with in any books that we have seen. It is an extraordinary fact that scarcely any effort has yet been made to investigate a matter of such importance to humanity as epidemics, for instance; and, as a consequence, sanitary works of this nature resolve themselves into discourses on appliances and the elementary principles of hygiene. Such books are useful; but it is really high time that a more exalted science of public health should be established.

An Index of Symptoms with Diagnostic Methods. By RALPH WINNINGTON LEFTWICH, M.D., Late Assistant-Physician to the East London Children's Hospital. [Pp. xii + 516. Fifth Edition.] (London: Smith, Elder & Co., 1915.)

THE Introduction of this work opens with the words "Diagnosis is the most difficult part of the art of Medicine, and he who would excel in it must be well equipped both intellectually and physically." In view of this well-known fact, this work should prove of great assistance to medical men. It is well arranged for easy reference and covers a wide field of disease. The Introduction touches on the Classification of Patients, Fallacies, and Classification of Symptoms, while the symptoms themselves are tabulated under general headings such as Interrogation, Inspection, Palpation, Percussion, Auscultation, and Miscellaneous. The fact that the work has already run into a fifth edition shows how much it has already been appreciated.

The Medical Annual. A Year Book of Treatment and Practitioner's Index. [Pp. cxx + 959, with illustrations and 71 plates. Thirty-third year.] (Bristol: John Wright & Sons. London: Simpkin, Marshall, Hamilton, Kent & Co., 1915. Price 10*s*. net.)

THIS is another useful book for the Medical Faculty, containing contributions in the shape of original articles by thirty-two of the most eminent specialists from all parts of the world. Such well-known names as G. Lenthal Cheatle, King's College Hospital, John S. Fraser of Edinburgh, Sir Leonard Rogers of Calcutta, Colonel Louis La Garde of New York, figure in this long list. The volume treats not only of the department of medicine but includes special articles on Naval and Military Surgery, with personal experiences in the treatment of wounds. It is so arranged that the practitioner can refer instantly to any disease or drug, and obtain the latest knowledge available about it. The Glossary containing most of the newer terms in this and recent volumes of the publication contribute to this end, and the numerous illustrations and beautiful plates add to the value of the work.

GENERAL

The Instinct of Workmanship and the State of the Industrial Arts. By THORSTEIN VEBLEN. [Pp. viii + 355.] (New York: The Macmillan Co., 1914. Price 6*s*. 6*d*. net.)

MR. VEBLEN here puts before the reader a survey not of the detailed progress of the industrial arts from prehistoric times, but of the spirit and aims which have governed them, of the habits of thought with which they have been connected, of the social and political contests through which they have forced their way, stage after stage, and moulded the life of the most progressive communities of the world.

Instincts are defined to be hereditary "specific and determinate propensities, proclivities, aptitudes, that are, within the purview of the social sciences, to be handled as irreductible traits of human nature." "Instinctive action is teleological, consciously so; and the teleological scope and aim of each instinctive propensity differs characteristically from all the rest." It is thus guided by intelligence and subject to the diversity of human characters. But through all the diversity there is recognisable a generic human type of spiritual endowment.

The two chief instincts that contribute directly to the material well-being of the race, and therefore to its biological success, are the sense of workmanship and the parental bent. They have much in common, and are inextricably mixed. The definition of the parental bent is far wider than the procreation of offspring. It includes much more than care for the welfare of one's own children: it "has a large part in the sentimental concern entertained by nearly all persons for the life and comfort of the community at large, and particularly for the community's future welfare." The instinct of workmanship is not necessarily simple. It may be taken "to signify a concurrence of several instinctive aptitudes," each of which may or may not prove to be simple and irreducible under psychological or physiological analysis. It "occupies the interest with practical expedients, ways and means, devices and contrivances of efficiency and economy, proficiency, creative work and technological mastery of facts." Much of its functional content is "a proclivity for taking pains."

This instinct of workmanship the author traces in the industrial arts of savage societies, up through the predatory culture that he holds succeeded the savage state, into the era of handicraft that blossomed in the fulness of the Middle Ages, with its competition, its pecuniary standard and its individualism, and on into the machine industry which began (roughly) in the third quarter of the eighteenth century, and has not yet wrought out the full measure of its influence, either on industry or on thought. The subject is one of profound interest, and it is treated with conspicuous ability. Owing, however, probably to the pressure of space in a small volume, the want of illustrations of the author's points clouds his meaning too often and prevents the reader from following the argument with full intelligence. His style, too, is somewhat heavy: the sentences are long and involved. And there is no adequate means of checking his generalisations, for want of exact references to the authorities he has used.

This is particularly irritating in the earlier chapters, dealing with prehistoric conditions and the status of savagery and the lower barbarism. Here many statements and inferences are made that would require careful examination before acceptance. In the later chapters Mr. Veblen is on more assured ground. The discussion of the theology and metaphysics of the Handicraft and Machine eras is excellent, and brings out with telling effect their relation to the condition of industry, and to the social and economic position which resulted from the industrial development. In the explanation of the rapid progress of Britain towards the end of the era of Handicraft and onwards there is a curious omission of all mention of coal. Even when comparing Britain with Scandinavia, where the geographic isolation, reckoned by Mr. Veblen as the principal factor, was similar, coal is still ignored. And yet it was one of the most important elements contributory to the advance. The author looks forward to a day when the machine industry will work itself fully out, when efficiency will no longer be counted in the terms of pecuniary gain, when business men will no longer control industry for their own advantage, and when Cotton Kings and Railway Kings

shall be no more. But how industry is to be then organised he only hints; how its products are to be distributed, and what form society is to take, he leaves in darkness. His business is not with the future; it is with the past and the immediate present.

E. SIDNEY HARTLAND.

The Indian Museum, 1814–1914. [Pp. 154 + lxxxvii, with 11 illustrations.] (Calcutta: Published by the Trustees of the Indian Museum.)

THIS volume has been published in commemoration of the centenary of the foundation of the Museum of the Asiatic Society of Bengal, which was brought under Government control in 1866, and thus became the Imperial Museum of India as it now exists. The Indian Museum is, however, scarcely a single institution, but rather a federation of institutions. The trustees and the superintendent directly control only the zoological and anthropological section, and they have but slight powers over the geological, archæological, artistic, and industrial sections, which are administered by other bodies. This anomalous arrangement is said to work well, but whether it can be permanent seems doubtful. The Museum is not a mere place of preservation for specimens representing the natural history and culture of India, but is an educational centre for Calcutta, and hence foreign objects, such as marsupials, are exhibited, and there are about 800,000 visitors a year. The staff also undertake original investigations, and zoologists the world over are familiar with the Museum's publications. The different chapters (which are written by different authors) describe fully the history of the institution and of its separate sections, and there are several appendices giving the Museum Acts, statistics, and so forth; but we think the book would have gained in interest if the account of the actual contents of the Museum had been less meagre. It is no reflection on the small staff of the Museum to say that the anthropological department is at present very unequal to the great opportunities of the Government of India.

A. G. T.

Handbook of Photomicrography. By H. LLOYD HIND, B.Sc., F.T.C., and BROUGH RANDLES, B.Sc. [Pp. x + 292, with 44 Plates and 71 Text-Illustrations.] (London: George Routledge & Sons. Price 7*s.* 6*d.* net.)

THIS book is said to be "the outcome of a series of articles originally published in the *Photographic Monthly*, and intended as an introduction to photomicrography from a photographer's point of view." In it the subject has been approached from its two aspects, with descriptions of both—photographic and microscopic technique; and "it is hoped that experts in either field will look with indulgence on an elementary treatment of the subjects with which they are familiar. At the same time, however, the processes are discussed in sufficient detail to be of use in research, for which the full possibilities of the combination of microscopy and photography are scarcely yet realised." The authors have well justified their design, and the book will prove very useful even to such "experts" as they refer to. The chapters deal with Photomicrographic Apparatus, the Microscope, Lamps and Illuminants, Low-Power Photomicrography, Critical Photomicrography, Colour Screens and Colour Sensitive Plates, Exposure, Oblique and Dark-ground Illumination, Opaque Objects, Metallography, Colour Photomicrography, Photographic Operations, and Some Applications of Photomicrography. There are numerous good plates and an Appendix and Formulæ. The work will be almost a necessity in most laboratories.

Evolution and the War. By P. CHALMERS MITCHELL, M.A., D.Sc., LL.D., F.R.S., F.L.S., F.Z.S. [Pp. xxv + 114.] (London: John Murray, 1915. Price 2*s*. 6*d*. net.)

MR. MURRAY has recently published quite a number of books on the matter of the war. Men of science will be especially interested in Dr. Chalmers Mitchell's work, and it can be warmly recommended for their perusal. His style is an admirable one; always intelligible, simple and yet not bald; well punctuated and cleanly paragraphed; and illuminated by the frequent use of the correct phrase and the decisive metaphor. What will charm every one from the beginning is the fact that the author has not scrupled to introduce some short biographical details, such as his experiences in Germany and in other countries, his reading, and his point of view. It is a pleasure to be able to record that in the opinion of the writer of this short review, Dr. Chalmers Mitchell is a genuine man of science—that is, one who not only uses science as an instrument, but also lives within the clear atmosphere of the scientific spirit and discipline. In this book we find none of the hypotheses converted into dogmas and fancies converted into facts which disfigure the works, not only of the pseudo-philosophers, but even of those who claim the rank of men of science. That is to say, the author is always unwilling to advance his feet beyond the stepping-stones of the most probable truths, and shows no desire to leap into the fog under the illusion that he can fly.

But the book is a short one, and deals only with evolution and the present war, without making any effort to deal completely with evolution and war in general—a much larger and very important subject which has not yet received full analysis, even by a Darwin and a Wallace. In fact, Dr. Chalmers Mitchell's book is chiefly concerned with the refutation of the German proposition that their burglarious attacks on surrounding populations are sanctioned by the scientific theory of the struggle for existence. His first chapters are devoted to showing (what is already evident) that Darwin's phrase, "struggle for existence," is merely a metaphorical one—and he cites several interesting facts for the purpose of rendering this position quite clear. His third chapter deals with Nationality and Race, and gives us several interesting summaries of recent work on the study of the Cephalic Index, and the distribution of stature and of coloration in the people of Europe. According to him Europe is peopled by three principal races; the Nordic or tall, fair, long-headed race, chiefly of Scandinavia, the shores of the Baltic, Holland, and Britain; the short, dark, long-headed race of the Mediterranean litoral; and the round-headed, "stocky" people of the centre of Europe and Russia. These, of course, are only averages; but, according to the author, the Germans are principally of the same race as the Austrians and Russians. But the matter cannot be sufficiently argued in a short work of this nature. The fourth and fifth chapters deal with the "Production of Nationality," and give many wise statements on the matter, showing that nationality is not constructed only from inherited traits.

The subject of evolution has become not a little wearisome at the present day owing to the immense diffuseness and often the rather misty outlook of writers. The inheritance of acquired characters and the endless talk about Lamarckism, Darwinism, Neo-Lamarckism and Neo-Darwinism have become, to write plainly, not a little of a bore. But, fortunately, Dr. Chalmers Mitchell does not cover us with much of this mud. Is he quite sound when he says that "The German claim that 'the natural law to which all the laws of nature can be reduced is the law of struggle' fails, first because, even if it were a scientific law, it does not follow that a law derived from a consideration of animals and plants applies to

human beings, and second, because it does not happen to be a law, but a hypothesis much in debate?" After all the words hypothesis and law can scarcely be demarcated by so exact a line. Hypothesis really covers almost the entire region of human reasoning, and reaches the domain of law when the integrated sum of argument reaches infinity; but we cannot state exactly when this happens. On the whole, in the reviewer's opinion, the theory of evolution has scarcely been shaken by recent criticisms, including those of the Mendelians. To him, natural selection still remains by far the most probable explanation of species. Perhaps it cannot be claimed to have passed into the region of absolute law, but it is not far from there. At any rate no other hypothesis seems to him to be within a measurable proportion of it in strength. But he agrees with Dr. Chalmers Mitchell that the German interpretation of it does not apply.

When he likes Dr. Chalmers Mitchell speaks very plainly. In SCIENCE PROGRESS for January 1915, Mr. Hugh Elliot gave a very decided review of Mr. H. S. Chamberlain's recent work on Immanuel Kant; but our present author is still more decided when he talks of "pretentious blunderers like Houston Chamberlain, the latter probably the least reliable of all who have peddled so-called philosophy to the public." Later on he says, "I trace back to Kant the dreaming megalomania that has destroyed the German sense of reality, and that has made German 'Kultur' the enemy of the human race. Back to Kant, for *corruptio optimi pessima*. Nietzsche, of whom so much has been made, is a terminal flower of the tree of idealistic thought, beautiful, poisonous and sterile. No doubt he has got into the newspapers through Mr. Bernard Shaw, a very competent publicist whose antics were agreeable in times of peace. But even Mr. Shaw is only Nietzsche grinning through a horse-collar, a spectacle that his old patrons find indecent when there are serious affairs on hand." It must be admitted that these so-called philosophers have been largely to blame for this war, and are, therefore, directly deserving of such censure. In fact, for a long time past the cult of untruth has become a frequent and a profitable one. But nations no more than individuals can lie with impunity.

EDITOR.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Experimental Electricity and Magnetism. By M. Finn, M.Sc., Senior Mathematics and Physics Master, Southend High School for Boys. Illustrated. London: G. Bell & Sons, Ltd., 1915. (Pp. x + 436.) Price 4*s*. 6*d*.

Proceedings of the Third International Congress of Tropical Agriculture held at the Imperial Institute, London, S.W., June 23 to 30, 1914. Including Abstracts of the Papers, supplied by the Authors, and Reports of the Discussions. International Association for Tropical Agriculture (L'Association Scientifique Internationale D'Agronomie Coloniale et Tropicale). Edited by the Honorary Secretaries. London: John Bale, Sons & Danielsson, Ltd., Oxford House, 83–91, Great Titchfield Street, Oxford Street, W. 1914. (Pp. xi + 407.)

Emma Darwin, A Century of Family Letters, 1792–1896. Edited by her daughter, Henrietta Litchfield. In Two Volumes. Illustrated. London: John Murray, Albemarle Street, W., 1915. (Pp Vol. I. xxxi + 289; Vol. II. xxv + 325.) Price 21*s*. net.

Volumetric Analysis. By A. J. Berry, M.A., Fellow of Downing College, Cambridge. Cambridge: at the University Press, 1915. (Pp. 137.) Price 6*s*. 6*d*. net.

Report on Fishery Investigations in Bengal, and Bihar and Orissa. With Recommendations for Future Work. Bulletin No. 5. By T. Southwell, A.R.C.Sc., F.L.S., F.Z.S., Deputy Director of Fisheries, Bengal, and Bihar and Orissa, and Honorary Assistant, Indian Museum. Calcutta: The Bengal Secretariat Book Depôt, 1915. (Pp. 87.) Price, Indian 6 annas, English 6*d*.

Annual Magazine Subject-Index, 1914. A Subject-Index to a Selected List of American and English Periodicals and Society Publications. Edited by Frederick Winthrop Faxon, A.B. Compiled with the co-operation of Librarians. Boston: The Boston Book Company, 1915. (Pp. 264.)

This is the eighth volume of a useful reference book, the aim of which is to furnish a subject-index to American and English periodicals not elsewhere indexed. Whilst excluding all fiction except that of very well-known authors, it makes a speciality of History, Travel, Mountaineering, Exploration, Outdoor Life, Fine Arts and Architecture. The names of all articles are carefully classified in subjects in a way that makes reference easy. It ought to find a ready sale in this country, as it is the only index covering the popular periodicals of Great Britain.

English, French, and German Vocabulary for Water Supply in the Field. By Philip Parker, M.I.C.E. London: 25, Victoria Street, S.W. (Pp. 36.)

Since the beginning of the war many vocabularies in English and French,

with or without German, have been published for the use of engineers and other technical workers in the field. An excellent vocabulary of this kind by Mr. Philip Parker has been specially designed for the use of men who supply water to the British troops. It is in the three languages and will certainly be useful.

The Minor Horrors of War. By A. E. Shipley, Sc.D., Hon. Sc. D. Princeton, F.R.S., Master of Christ's College, Cambridge, and Reader in Zoology in the University. Illustrated. Second Edition. London: Smith, Elder & Co. 15, Waterloo Place, 1915. (Pp. xi + 178.)

The World's Cotton Crops. By John A. Todd, B.L., Professor of Economics, University College, Nottingham, Formerly of the Khedivial School of Law, Cairo. With Thirty-two Page Illustrations. Also sixteen Maps and Diagrams. London: A. & C. Black, Ltd., 4, 5 & 6, Soho Square, W., 1915. (Pp. xii + 460.) Price 10*s*. net.

Discoveries and Inventions of the Twentieth Century. By Edward Cressy. Profusely Illustrated. London: George Routledge & Sons, Ltd., Broadway House, 68-74, Carter Lane, E.C., 1914. (Pp. xvi + 398.) Price 7*s*. 6*d*. net.

Improved Four-Figure Logarithm Table. Multiplication and Division made Easy. By George C. McLaren, Fellow of the Faculty of Actuaries, Scotland. Cambridge: at the University Press, 1915. (Pp. 27.) Price 1*s*. 6*d*. net.

Will be very useful to many and especially applicable for teaching children the use of logarithms—a thing which ought to be taught much more frequently than it is, even in elementary schools. Perhaps the preliminary explanation of the tables ought to have contained a few more examples. The principle adopted is to get rid of the "characteristic," and to place the decimal point by means of ordinary arithmetical considerations, and we think that this principle is quite justified by the results. As a matter of fact the characteristic often gives more trouble than the rest of the logarithmic calculation. The remainder of the logarithm after the first four figures is given by the use of a full stop for a fraction of about one-third and a colon for a fraction of about two-thirds, and this easily suffices for the objects of this little table—which we can strongly commend.

Combinatory Analysis. By Major Percy A. MacMahon, F.R.S., D.Sc., LL.D. (late Royal Artillery), of St. John's College, Cambridge. Vol. I. Cambridge: at the University Press, 1915. (Pp. xix + 300.) Price 15*s*. net.

Symbiogenesis: The Universal Law of Progressive Evolution. By Hermann Reinheimer. 1915, Knapp, Drewett & Sons, Ltd., 30, Victoria Street, Westminster, S.W., and Clarence Street, Kingston-on-Thames. (Pp. xxiii + 425.) Price 10*s*. 6*d*. net.

A History of Botany in the United Kingdom from the Earliest Times to the End of the 19th Century. By J. Reynolds Green, Sc.D., F.R.S., Fellow and Lecturer of Downing College, Cambridge. London and Toronto: J. M. Dent & Sons, Ltd.; New York: E. P. Dutton & Co., 1914. (Pp. xii + 648.) Price 10*s*. 6*d*. net.

Towards Racial Health. A Handbook for Parents, Teachers and Social Workers on the Training of Boys and Girls. By Norah H. March, B.Sc., M.R.San. I. With a Foreword by J. Arthur Thomson, M.A., LL.D., Professor of Natural

History in the University of Aberdeen. And Illustrations by Jessie M. Lawson, B.I., A.R.C.A. London: George Routledge & Sons, Ltd., Broadway House, 68–74, Carter Lane, E.C., 1915. (Pp. vii + 326.) Price 3*s*. 6*d*. net.

Histoire de l'Involution Naturelle. Traduite de L'Italien par Me. Ida Mori-Dupont. Avec 125 Figures dans le Texte. Par Henri Marconi. Paris: A. Maloine, Editeur, 25–27, Rue de l'École de Médicine; Lugano: Maison d'Éditions du "Cænobium," 1915. (Pp. xii + 505.) Price 15 francs.

Essays Towards a Theory of Knowledge. By Alexander Philip, F.R.S.E. London: George Routledge & Sons, Ltd.; New York: E. P. Dutton & Co., 1915. (Pp. 126.) Price 2*s*. 6*d*. net.

Ancient Hunters and their Modern Representatives. By W. J. Sollas, D.Sc., LL.D., M.A., Ph.D., F.R.S., Fellow of University College, and Professor of Geology and Palæontology in the University of Oxford. With 314 figures and 2 plates. London: Macmillan & Co., Ltd., St. Martin's Street, 1915. (Pp. xxiii + 591.) Price 15*s*. net.

Descriptive Geometry for Students in Engineering Science and Architecture. A Carefully Graded Course of Instruction. By Henry F. Armstrong, Associate Professor of Descriptive Geometry and Drawing, McGill University. First Edition. New York: John Wiley & Sons, Inc. London: Chapman & Hall, Ltd., 1915. (Pp. vi + 125.) Price 8*s*. 6*d*. net.

The Gospel of Healing. By Rev. A. B. Simpson, D.D. New Edition. London: Morgan & Scott, Ltd., 12, Paternoster Buildings, E.C., 1915. (Pp. 154.) Price 2*s*. net.

Flies in Relation to Disease. Bloodsucking Flies. By Edward Hindle, B.A., Ph.D., Assistant to the Quick Professor of Biology, Cambridge. Cambridge: at the University Press, 1914. (Pp. xv + 398.) Price 12*s*. 6*d*. net.

A Catalogue of Current Mathematical Journals, etc. With the Names of the Libraries in which they may be found. Compiled for the Mathematical Association. London: G. Bell & Sons, Ltd., Portugal Street, Kingsway; and Bombay, 1913. (Pp. 39.) Price 2*s*. 6*d*.

ANNOUNCEMENT

The following are a few details regarding the forthcoming meeting of the *British Association.* It will be held this year at Manchester from Tuesday, September 7, to Saturday, September 11, under the Presidency of Prof. Arthur Schuster, Sec. R.S., whose Inaugural Address will be delivered at 8.30 p.m. in the Free Trade Hall. The names of the Presidents of the different Sections are as follows:

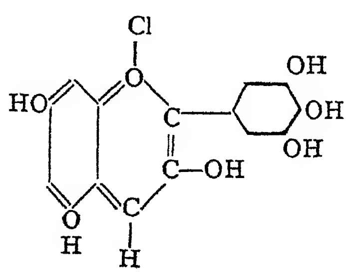
Section	A.	Mathematical and Physical Science	Sir F. W. Dyson, M.A., LL.D., F.R.S.
„	B.	Chemistry	Prof. H. B. Baker, M.A., D.Sc., F.R.S.
„	C.	Geology	Prof. Grenville A. J. Cole.
„	D.	Zoology	Prof. E. A. Minchin, M.A., F.R.S.
„	E.	Geography	Capt. H. G. Lyons, D.Sc., F.R.S.
„	F.	Economic Science and Statistics	W. R. Scott, M.A., Litt.D.
„	G.	Engineering	H. S. Hele-Shaw, D.Sc., LL.D., F.R.S.
„	H.	Anthropology	C. G. Seligman, M.D.
„	I.	Physiology	Prof. W. M. Bayliss, M.A., D.Sc., F.R.S.
„	K.	Botany	Prof. W. H. Lang, M.B., D.Sc., F.R.S.
„	L.	Educational Science	Mrs. Henry Sidgwick.
„	M.	Agriculture	R. H. Rew, C.B.

Tickets may be obtained from the Assistant Secretary, British Association, Burlington House, Piccadilly, London, W.

CORRECTION

THE ANTHOCYAN PIGMENTS

Formula III. on p. 610 of the April 1915 issue should appear



Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

THE NAPIER TERCENTENARY AND THE INVENTION OF LOGARITHMS

By C. G. KNOTT, D.Sc.
General Secretary, Royal Society of Edinburgh

On Friday, July 24, 1914, there assembled in the Debating Hall of the Students' Union of the University of Edinburgh a cosmopolitan gathering, which proved to be the last International Congress before the Great War. Favoured with bright weather, Scotland's picturesque capital looked her best; and her citizens welcomed visitors and delegates from all parts of the Old and New World.

In response to the invitation of the Royal Society of Edinburgh, under whose auspices the Napier Tercentenary Celebration was held, representatives of almost every nationality of the civilised world enrolled their names as Members of the Congress, and a large percentage of these were able to appear in person. The meeting of the British Association in Australia robbed the Edinburgh gathering of a goodly contingent of eminent British mathematicians who were keenly interested in Napier and Napier's work; and there is little doubt now that delegates from the south and east of Europe were at the last moment prevented from travelling by the ominous outlook in Austria and Serbia.

Fortunately the political situation did not become acute till the last day of the Congress, and representatives from Russia, Poland, France, Hungary, Austria, Germany, and Turkey, met their British friends in brotherly conference along with Danes, Dutchmen, Canadians, and other Americans, their one purpose being the commemoration of the publication in 1614 of the first book of logarithms, *Mirifici Logarithmorum Canonis Descriptio*, as Napier himself somewhat audaciously called it.

To most minds the word Logarithm connotes a system of calculations in which addition and subtraction take the place of multiplication and division, and in which still more difficult

arithmetical operations are amazingly simplified. To the mathematician the logarithm means infinitely more; for it has a far-reaching significance in algebra, and is constantly turning up in all kinds of mathematical analysis, like the head of King Charles in Mr. Dick's disquisitions. It also marks the beginning of an epoch of scientific development, in which with ever-increasing accuracy man has commandeered the hidden forces of Nature to his will.

Directly and indirectly Napier's discovery of the logarithmic function and his invention of the logarithm of calculation has indeed had a profound influence on scientific thought and method. Many of the lines of this influence were shown in the exhibition of books, instruments, and models which constituted the most popular feature of the Congress. A Handbook to the Exhibition was issued by the Committee, with E. M. Horsburgh as editor-in-chief. It was presented to all members of the Congress, and was indispensable as a guide to the exhibits. It has been widely recognised as a book of unique interest and permanent value.[1] What do we not find within the compass of its three hundred odd pages? Calculating machines of every type are explained in word and illustrated in picture. These range through all stages of complexity from Napier's "Bones" and the Abacus of the East to modern arithmometers and comptometers, planimeters and integraphs, harmonic analysers and tide-predictors. Even calculating prodigies find a place within its pages.

This handbook was placed in the hands of the visitors on the opening day and could not of course give any account of the Proceedings of the Congress. These however will ere long be set forth in the Memorial Volume of the Napier Tercentenary.[2]

As secretary of the Congress and editor of this Memorial Volume I have had and continue to have the privilege and honour of being in close touch with those who have enriched it by their contributions. The majority of these contributions were communicated in whole or in abstract to the Congress, a few, in the enforced absence of their authors, being taken as read. The contributions fall naturally into two main groups, namely,

[1] Copies may still be obtained through G. Bell & Sons, York House, Portugal Street, London, W.C.

[2] Non-Members of the Tercentenary Celebration may order copies through Longmans, Green & Co., Paternoster Row, London.

those which treat directly of Napier himself or of his work, and those which have regard to mathematical developments and applications more or less closely associated with the logarithmic idea connected with the name of Napier.

When the Royal Society of Edinburgh announced the intention of holding a Tercentenary Celebration, a widespread interest was aroused all over the civilised globe. A few biographical sketches of real value were prepared by mathematicians of reputation; and in many journals and newspapers there appeared articles on the life, character, and deeds of the ingenious Baron of Merchiston. These but served to increase the interest in the coming celebration, in spite of the general feeling that all that was worth saying had been said long ago concerning Napier and logarithms. It was difficult to imagine that after three hundred years there could be anything to add to our knowledge of what Napier did and how he did it.

Nevertheless in his opening address Lord Moulton showed that there was still a problem to solve in regard to the genesis and growth of the conception of the logarithm. In other words, how are we to connect the very remarkable manner in which Napier presented his definition of the logarithm with the mathematical conceptions of his time? This was Lord Moulton's theme, and his treatment of it forms the first contribution to the Memorial Volume. Other papers, notably those by Dr. Glaisher, Prof. Eugene Smith, Prof. G. A. Gibson, and Prof. Florian Cajori, take up other historic aspects of the evolution of the logarithm. Though not intended by their authors to be in any strict sense novel, these are all fresh discussions by master hands; and we may safely regard this group of historical essays as containing the most accurate account yet presented of Napier's great work.

There had crept into the popular accounts of Napier and his work certain inaccuracies which it is hoped will be once and for ever disposed of. Such inaccuracies, copied slavishly by one "authority" after another, die hard; and it is only by going back to the originals that the real facts can be established.

Now there are three aspects in which Napier's invention should be considered, if a true idea of the greatness of his work is to be gained. First, there is the general conception, which Napier shared with others of his time, that arithmetical operations might be simplified by suitable tabulations. Secondly, there is the particular method by which Napier realised the first

table of logarithms. Third, there is the profound mathematical insight which enabled Napier not only to grasp the significance of a logarithmic function, but also to indicate with precision the great superiority of what is now known as the common logarithm.

In regard to the first of these, it must be remembered that considerable progress had been made before Napier's day in the recognition of the laws of exponents or indices. This is indeed the subject on which Prof. Eugene Smith discourses with erudition and lucidity. And we know that Buergi, following up the line of some of these conceptions, constructed a few years after Napier a practical table of what we would now call anti-logarithms, by which certain arithmetical short-cuts could be taken.

The fundamental idea was to establish a correspondence between the successive terms of two sets of increasing or diminishing numbers. We may take the example given long before Napier's day, and familiar enough in our elementary text-book.

The two sets or series of numbers

1	2	4	8	16	32	64	128	256	512	1024	etc.
0	1	2	3	4	5	6	7	8	9	10	etc.

are constructed on an obvious principle. The numbers in the upper row increase by successive *multiplications* by two, and form what is known as a geometrical progression. On the other hand the numbers in the lower row form an arithmetical progression, and increase by the successive *additions* of unity. The lower numbers serve to mark the positions in the upper row of the successive powers of two. They are, in fact, the numbering of the ratios, the meaning which lurks etymologically in the word Logarithm. To multiply together any two numbers in the first row we have simply to add their corresponding numbers in the second row, and above the sum we find the desired product. In like manner, to divide one number in the upper row by another we subtract the corresponding numbers in the lower row, and above the difference we find the quotient.

Such a table is obviously of no practical value, for the numbers it contains are too far apart and the interval between each contiguous pair grows rapidly in magnitude as we pass to higher terms. Nevertheless the idea of the logarithmic table is

there; and to us nowadays it seems extraordinary that the idea had not been developed in a practical form earlier than it was.

As we shall see later, Napier's method of constructing a serviceable logarithmic table for trigonometrical purposes was evolved along a different line. Jobst Buergi, however, an ingenious Swiss, constructed along the very lines of the example just given what he called *Aritmetische und Geometrische Progress Tabullen*. The book, which was printed six years after the publication of Napier's *Descriptio*, is very rare; but, through the kindness of the Town Library of Danzig, the members who attended the Napier Tercentenary Congress had the privilege of seeing one of the few copies extant. In the Bibliography of Books Exhibited at the Celebration which Prof. Sampson contributes to the Memorial Volume a clear account is given of the mode of construction of the *Progress Tabullen*. Beginning with the number 100,000,000, Buergi constructed a geometrical progression with constant ratio 1·0001. The corresponding terms in the associated arithmetical progression are 0, 10, 20, 30, etc., the constant difference being 10. The numbers of this latter series are distinguished by being printed red. They correspond to what we now call logarithms. Thus in Buergi's table we may regard the logarithms as proceeding by a constant difference ten (or by unity if we drop the unnecessary zero) and the corresponding numbers by a constant multiple 1·0001. By this means the tabulated numbers were near enough together to make the table useful as an antilogarithmic table through a considerable range. It has, however, conspicuous limitations in comparison even with Napier's original form.

When we turn to Napier's *Mirifici Logarithmorum Canonis Descriptio* (of which the title page is here reproduced) we come into touch with a new order of thought. In this book the word Logarithm is coined, its meaning explained, and its uses illustrated. But there is no account of the method of calculation of the tabulated numbers which give the logarithms of the trigonometrical sines of angles. For this information we must have recourse to the *Constructio*, which was published after Napier's death in 1819. It is abundantly evident that the *Constructio* was written before the *Descriptio*, probably many years earlier; and the calculations themselves must have been completed before even the *Constructio* was composed.

Now, according to Lord Moulton's theory, Napier seems to

have begun these calculations very much as Buergi some years later began his. He formed two series of numbers, the one in arithmetical progression, and the other in geometrical progression. Buergi began with 100,000,000 and called its "red number" zero; Napier began with 10,000,000 and called its logarithm zero. Napier, however, having in view a table of trigonometrical sines of angles in a circle of radius 10,000,000, proceeded to construct his geometrical progression by multiplying by a ratio slightly less than unity, the ratio being in the first instance unity diminished by the ten-millionth part. He also secured accuracy in the last unit by continuing the calculations as far as the seventh decimal place beyond the unit, using the very decimal notation which we employ to-day. So far it might be said there was not much difference between the two methods, except that by use of a ratio less than unity Napier got rid of the imperfection of a gradually increasing interval between successive numbers in the geometrical progression.

To have gone on diminishing the numbers in succession by the ten-millionth part until he had filled in the whole range from 10,000,000 to 300 (approximately the sine of one minute of arc on this scale) was of course an impossible undertaking. But when Napier had carried out this simple arithmetical operation a hundred times in succession he found that the 10,000,000 had been reduced to a number very slightly greater than 9,999,900, that is, less than the original number by the hundred-thousandth part. He was then able to make a new start with 10,000,000 and build upon it a new geometrical progression in which each new number was formed from its predecessor by subtracting the hundred-thousandth part of the latter. This very simple arithmetical operation was carried out fifty times in succession; and then it was found that the last term could be very nearly obtained from the original 10,000,000 by subtracting the two-thousandth part, which is equivalent to multiplying by the ratio of 9,995 to 10,000. Starting with this new ratio, Napier formed a third geometrical progression, which after proceeding for sixty-nine terms ended with a number slightly greater than 9,900,000. By repetition of this process the interval between 10,000,000 and 5,000,000 was filled in with numbers in geometrical progression, whose *numbered positions or logarithms* in the corresponding arithmetical progression were known.

It was no doubt while engaged in the laborious calculations

MIRIFICI

Logarithmorum

Canonis deſcriptio,

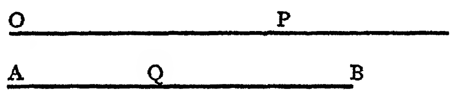
Ejuſque uſus, in utraque *Trigonometria; ut etiam in* omni Logiſtica Mathematica, *Amplißimi, Facillimi, & expeditißimi explicatio.*

Authore ac Inventore,
IOANNE NEPERO,
Barone Merchiſtonii,
&c. Scoto.

EDINBURGI,
Ex officinâ ANDREÆ HART
Bibliopôlæ, CIↃ. DC. XIV.

of these progressions that Napier's mind grasped a great mathematical principle, whose immediate application was invaluable to him in carrying his calculations to a fitting conclusion. As enunciated in the *Descriptio* this principle is that "the logarithms of Proportionall numbers and quantities are equally differing." This is the opening sentence of Chapter II of the *Descriptio*, Chapter I having been taken up with his remarkable kinematical definition of a logarithm in terms of the corresponding motions of two points. By this ingenious definition Napier virtually created the first transcendental function imagined by man. This is now known as the exponential or logarithmic function according as the one quantity or the other is taken as the independent variable. The whole question is very clearly discussed in the articles contributed to the Memorial Volume by Dr. J. W. L. Glaisher and Prof. G. A. Gibson.

Let us now return to the kinematical definition.



Imagine two points P and Q to move in such a way that while P, starting from O, describes its path with constant velocity, Q, starting at a point A with the same velocity, moves towards a goal B with a velocity at each point proportional to the distance QB still to be described. The speed of Q is thus constantly diminishing, and Q can never in a finite time reach B. According to Napier's first system of logarithms the distance OP travelled by P in the time taken by Q to reach any position is the logarithm of the distance QB still to be described. It is easy to see that in successive short equal intervals of time the total distances travelled over by P from the start form an arithmetical progression, while the corresponding distances QB form a geometrical progression. Napier's generalisation from a succession of numbers in geometrical progression to the conception of a continuously varying quantity whose rate of change is proportional to itself was a remarkable mathematical achievement in an age which possessed no notation for such a conception.

It is sometimes pointed out, somewhat critically, that what are usually called Napierian Logarithms in modern books of tables are not exactly those which Napier first tabulated in the

Descriptio. But the distinction is of no mathematical importance. The one follows from the other as soon as we determine to make the logarithm of unity zero. This step, which did not at first seem necessary either to Napier or Buergi, was indeed first suggested by Napier in his conversation with Henry Briggs as reported by Briggs himself, and is no doubt part of the improvement which Napier referred to in an Admonition in the *Descriptio.* Also, according to Briggs, it was Napier who suggested taking the logarithm of 10 to be equal to unity, as it is in all tables of what are known as common logarithms. This choice of a new base, as we now call it, effected a great practical improvement, the carrying out of which we owe to the devoted labours of Briggs.

A popular notion which long held sway was that Napier invented a system of logarithms which would have been of little use if Briggs had not thereafter imagined and calculated an infinitely superior system; and this tradition was no doubt fostered by the nomenclature adopted in many books. No candid reading of the original documents could ever have led to such a misconception. Napier not only gave to the world the mathematical conception of the logarithmic function with its practical corollary the Natural or Napierian Logarithm, but he also directed Briggs to calculate the common logarithm to base 10, which is what the ordinary computer is content to use without troubling his mind about the niceties of transcendental functions. In fact, we owe to Napier the only two systems of logarithms in use.

All this and much more will be found in the pages of the Memorial Volume; for the Congress did not limit its activities to consideration merely of things logarithmic. Although his chief fame rests on the discovery of the logarithm, John Napier's mathematical genius had other issues. He was led to construct his logarithmic tables so as to facilitate trigonometrical calculations; and although he soon found that the conception of the logarithm was far wider than the original problem he set before him, nevertheless in his *Descriptio* he demonstrates the value of his method by applying it to trigonometrical questions. Thus in the fourth chapter of the second book he gives his Rules for Circular Parts for right-angled and quadrantal spherical triangles. Again in the *Constructio* we find one of the celebrated propositions known as Napier's Analogies, in which the angles

of a spherical triangle are connected by formulæ suitable for logarithmic treatment. These trigonometrical theorems by themselves are sufficient to place Napier in the front rank of mathematicians. In this connection Prof. Sommerville contributes to the Memorial Volume a short paper in which he extends Napier's Rules and trigonometrically equivalent polygons to non-Euclidean geometry.

In still another direction the far-reaching influence of Napier's discoveries is clearly demonstrable. In an article on the Graphical Treatment of some crystallographical problems Dr. A. Hutchinson points out that, once the simple geometrical laws governing crystalline structure were recognised, "crystallographers found in the inventions and discoveries of Napier powerful tools ready forged to their hands." Dr. Hutchinson then proceeds to describe a form of logarithmic slide rule specially suitable for crystallographic investigations.

In everything mathematical which Napier himself placed before the world his great aim was to facilitate calculation. In the popular mind Napier is known chiefly for his logarithms and for his calculating rods, which were in great vogue during the seventeenth and part of the eighteenth century. These rods, or "Neper's Bones," as they were frequently called, were simply rods of wood or ivory inscribed with the multiples of the natural numbers. A complete set of rods was in fact a multiplication table, mechanically adjustable so as to reduce multiplication to simple addition. The *Rabdologia*, the small Latin treatise in which Napier describes the use of his rods, was translated into Italian in 1623, a fact which testifies to the early recognition of its practical value. As the memorising of the multiplication table became an essential part of education, Napier's Bones gradually fell into disuse.

In the *Rabdologia* other and more complicated methods of calculation by rods are described; but it is doubtful if these ever came into general use. The last section of the book is devoted to what Napier calls Local Arithmetic, in which the multiplication of two numbers is effected (first) by expressing each number in the scale of 2, (second) by representing them by means of counters on the spaces of an enlarged chess-board, (third) by manipulating them in a simple way so as to produce the product in the binary scale, (fourth) by re-translating the result finally in the ordinary denary scale. The whole process

is almost purely mechanical: but here again it could be of no real advantage to any one who knew the multiplication table for numbers up to nine.

To complete our conception of John Napier as a mathematician some reference must be made to the posthumous work *De Arte Logistica*, published in 1839 by Mark Napier. Through the kindness of its present possessor, John Spencer, F.I.A., London, the manuscript from which the book was printed was on view at the Tercentenary Exhibition. It appears to have been copied from the original by Robert Napier, John Napier's son. It bears no title; but on the outside leaf were inscribed by hand across the top the words "The Baron of Merchiston his booke of Arithmeticke and Algebra," and along the right-hand margin and at right-angles to the first inscription the words "For Mr Henrie Briggs, professor of Geometrie at Oxforde." Some other words had also been written but had been defaced. These hand-written inscriptions were chosen by Mark Napier as a second title to the book, the Latin title, *De Arte Logistica*, being constructed from the opening sentence. A detailed account of the contents of this remarkable work is given in the memorial volume by Prof. Steggall. Published as it was so long after Napier's death, this book had of course no direct influence on the development of algebra; but its contents show that Napier was in possession of mathematical knowledge and methods in advance of his time. Like all pioneers he had to invent his own notation. It is conceivable that had Henrie Briggs received the manuscript of *De Arte Logistica* (as was obviously intended) some of Napier's symbolism might have become the common property of the race, and the art of the ready reckoner been more rapidly developed. The Baron of Merchiston was undoubtedly himself a skilled arithmetician and had many tricks for shortening the processes of multiplication and division. His treatment of vulgar fractions and of decimal fractions is indeed most masterly and lucid. In his exposure of the disadvantages of the cumbrous astronomical notation of hours, minutes, seconds, as compared with the simple decimal system, John Napier voiced a much-needed reform which the intellectual inertia of the centuries still prevents.

Napier's treatment of powers and roots is remarkable and instructive. He makes use of what is virtually the binomial theorem up to the twelfth power and constructs an ingenious

triangular diagram of numbers to facilitate the calculation of the higher powers. The extraction of square and cube roots and of higher roots compounded of these is clearly expounded; and when a number will not "extract" perfectly Napier shows how to find limits between which the incommensurable value lies.

Perhaps, however, the most interesting feature is his recognition of the imaginary quantity, namely, the square root of a negative number. In particular the imaginary $\sqrt{-9}$ is introduced with the warning that the radicle and the sign must not be transposed. Prof. Steggall infers that something of value has been lost, to which Napier refers when he speaks of a "great algebraic secret, from which (although it has, as far as I know, not been revealed by any one) it will afterwards appear how great advantage will follow to this art and to the rest of mathematics."

We know on the authority of Robert Napier that the manuscript of *De Arte Logistica* was by a wonderful chance saved from destruction by a fire in which many of John Napier's papers perished. Among these there may well have been a fuller discussion of the imaginary. There is no doubt that Napier's reference to imaginaries is the first on record. To quote from Dr. George Philip: "Historians of algebra usually credit Girard with being the first to use imaginary roots of equations, but in view of the above the Flemish mathematician must waive his claim in favour of Napier. As Girard's most important work was published in 1629, there is no question of Napier having got the idea from him, and it is superfluous to remark that Girard could not have borrowed from Napier."

It is not possible within the limits of a short article to do more than touch upon the many points of history which were dealt with by the Congress. It may be of interest, however, to refer more particularly to one line of discussion which received attention at the Tercentenary Celebration, namely, the development of logarithmic and trigonometrical tables since the days of Napier.

Thus questions were raised and discussed as to the improvement of mathematical tables in general, the mode of arrangement, the economising of space, the advisability of publishing new logarithmic tables to 12 or 15 significant figures, and the like.

In connection with this last question of extended logarithmic tables, one of the most interesting exhibits at the Celebration

was the great collection of manuscript tables of logarithms and trigonometrical functions calculated by Dr. Edward Sang. These were some years ago gifted by Dr. Sang's daughters to the nation and were consigned to the care of the Royal Society of Edinburgh. It was the existence of this remarkable and truly monumental work which suggested to the Council of the Royal Society of Edinburgh the idea of celebrating in some appropriate way the three hundredth anniversary of the publication of the first book of logarithms.

In Edinburgh the first table of logarithms was calculated and published; and in Edinburgh there now lie thirty-seven large manuscript volumes full of tabulated numbers and logarithms. Can these great tables be placed at the service of mankind?

It is true that during the last few years calculating machines have come widely into use for arithmetical operations which were previously done by means of logarithms. The thought has even been expressed that the logarithm has had its day. The modern mechanical representatives of Napier's "bones" or calculating rods are believed in some quarters to have ousted the logarithm entirely. However this may be for certain of the simpler kinds of calculation, there still remain many calculations which are beyond the power of arithmometers and comptometers, and these need for their successful working the use of logarithmic tables.

This at least is the opinion of Prof. Andoyer, the well-known head of the Paris Observatory. As a member of the Tercentenary Congress he read an account of his recently published logarithmic cosines, tabulated to fourteen significant figures. The paper, which is one of the most valuable contributions to the Memorial Volume, discusses also this very question of the need for more extensive and absolutely accurate logarithmic tables. Now we have in Sang's manuscript volumes all that is necessary for providing the mathematical world with logarithms of all numbers from 100,000 to 370,000, calculated to fifteen figures. The ordinary calculator is generally content with logarithms to five places; and for specially accurate work in actuarial and geodesic problems seven-place logarithms are usually sufficient. Nevertheless there are problems in which a greater number of places would be a gain; and in any case it would be of advantage to the world of computers if they were able at a moment's notice to turn to a table of logarithms

accurate to 10, 12, or 14 figures. This has been supplied by Edward Sang within the limits just named.

It would not however be necessary, as pointed out by Prof. Andoyer, to publish more than a certain part of these tables in order to place in the hands of the mathematical world a fundamental table of logarithms to 12 or 14 figures. It would be sufficient to tabulate the logarithms of numbers from 100,000 to 200,000, for by simple division any number can be reduced to a number beginning with the figure "one."

The simplest and certainly the most accurate way to publish Sang's tables would be to reproduce the original manuscript pages as line engravings by photography. This was indeed the method which first suggested itself to Dr. Burgess and myself when Dr. Sang's manuscript volumes were consigned to the care of the Royal Society of Edinburgh. And various considerations have recently made some of us regard with increasing favour this method of utilising to the full the fundamentally important parts of Sang's manuscript volumes.

It should be noted in the first place that the manuscripts have been prepared with extreme care, the figures being beautifully written and entered with great neatness in appropriate spaces in specially ruled paper. The rulings are differently spaced according to the nature of the table. For example, in the table of the logarithms of integer numbers from 1 to 10,000, there are twenty-five numbers on each page with their logarithms to 28 figures, covering, therefore, 400 manuscript pages in all. This, which forms the basis of the whole, could be reproduced by a slight reduction in 100 pages of 100 numbers to the page.

The logarithms to 15 figures of numbers from 100,000 to 200,000 are also arranged in the manuscript volumes twenty-five to the page, and contain the first and second differences entered in specially prepared ruled paper. Here, also, by a similar reduction four of the manuscript pages could be reproduced as one page containing 100 numbers with their logarithms and first and second differences. The whole could be reproduced in a large quarto volume of 1,000 pages.

It should be noted that the final tabulations were made by Dr. Sang after three verifications, so that it is almost impossible for any error of tabulation to exist, the only possible errors being those which result from the method of interpolation, and which rarely reach three units in the fifteenth figure.

It would thus be possible to reproduce with an absolute accuracy the manuscript tables giving the 28-figure logarithms of the first 10,000 numbers and the 15-figure tables of numbers from 100,000 to 200,000, with the first and second differences, conveniently arranged in one volume of 1,100 pages. The *Auxiliary Table*, for convenient interpolation when more than 11 or 12 figures were being used, would occupy another hundred pages. In this way the most important part of Sang's logarithmic calculations would be made accessible to the whole mathematical world; and the work of calculating anew a fundamental table of logarithms need never again be undertaken. Such a table would form the source of all future tabulations of logarithms.

It has been estimated that the cost of reproducing by photography these tables would be about one-third or one-fourth the cost of setting them up in type in the usual way. Much time and risk of error in copying, in setting up, and in proof-correcting would be saved; and as regards the accuracy of the two methods there is of course no comparison.

Such, then, is the proposal which I desire to lay before the many interested in logarithms; and what more fitting outcome of the Napier Tercentenary could there be than making accessible to the civilised world the fundamental part of these great tables, calculated in the very city where Napier invented the logarithm and gained undying fame as a benefactor of his race?

A PLEA FOR ECONOMY OF THOUGHT AND LABOUR IN THE MATHEMATICAL SCIENCES BY THE STUDY OF THEIR HISTORY

By W. Stott

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Never in the world's history has it been more necessary for the British Empire to consider in what way it can economise its material resources, and never has it been more necessary for it also to consider how it can economise in the realms of pure thought; it is probable that no serious refutation will be given to the statement that no nation has ever had a greater number of scientific workers, of very great ability, and in no nation have the results of their labours been so consistently ignored.

Were the discoverer alone the subject of neglect, this, though discouraging to new workers, would not be so important, but unfortunately it is the result of his labours that has been neglected, and this is worse than a crime, it is a blunder.

The consequence of this neglect is that numerous theorems and processes in mathematics have been discovered, lost, rediscovered, and again lost; time after time has it been necessary for new workers to go over the ground, of which they could obtain no record that it had been previously explored. Sometimes, years after the printing of a paper, some unfortunate author has been overwhelmed by the discovery that the work on which he had spent so much time and thought had been previously given to the world; it may be that his own paper contained sufficient points of originality to exonerate him entirely from any charge of plagiarism, but there can be no compensation for the enormous loss of time involved in rediscovering ancient theorems.

As in war, so in science, the best progress is made by consolidating the positions already gained, and using them as a

jumping-off stage for the next advance; but such a method does not seem to harmonise with the British temperament, which prefers to ignore past discoveries, and to begin each time from the original position.

How much has been lost by British methods can hardly be estimated, but the present writer has in mind numerous cases which have occurred (in his own narrow experience) of logarithmic and trigonometric tables to many decimals, of tables of primes, interest and mortality tables, tables of Gamma, Bessel, and other functions which have been recalculated because the investigator had no means of finding out if the work had been previously done, and where it could be obtained.

Numerous cases will occur to the mind of those at all familiar with the history of mathematical progress. De Morgan in a letter to Hamilton says, "I am perfectly surprised to find a fundamental method of Newton followed up by Lagrange, unknown to everybody I ever come in contact with, whether in books, or speech, or writing."

There are people, it is true, who seem to be under the impression that all valuable matter is retained, and that everything important in foreign literature makes its way into English. But this is not so, and the production in English is so belated that the influence the work might have is lost. One hundred years after the publication of Laplace's *Essai philosophique sur les Probabilités*, it has been translated into English by the Americans, and Lagrange's great work, the *Mécanique analytique*, which Hamilton called a scientific poem, has never been printed in English.

It is not only the work of the greatest men, but also of the less great men, that requires attention.

In any History of Mathematics men such as Sir Isaac Newton will necessarily take up a large amount of space, but, as De Morgan says, they should not take up all the space. Room should be left for the minor men, who have collected and formed the material out of which the greatest scientists have erected their wonderful edifice.

One of such minor men (if indeed this is not too low a term) was Michael Dary, a contemporary of Newton, who writes to him, and subscribes himself "your loving friend," which perhaps ought of itself to be sufficient to rescue Dary from oblivion.

Newton says "Dary is very solicitous of mathematics," and this seems to be confirmed by a letter of Dary to Newton in which he says, "Although I sent you three papers yesterday, I cannot forbear sending you this."

Dr. Wallis, than whom there could be no greater testimony, called Dary a knowing man in algebra, and John Collins, writing to Sir John Frederick, says, "'Tis well known to very many that Mr. Dary has furnished others with knowledge therein, who, publishing the same, have concealed his name; as for instance Dr. John Newton hath lately published a book of Arithmetic, another of Gauging; all that is novel in both he had from Mr. Dary"; and again, in writing to James Gregory, says: "One Mr. Dary asserts a connection between the quadrature of the hyperbola and that of a segment of a circle; if so, then a table of areas of segments, the best of which is by Sybrant Hantz, will be of greater use than is imagined." We have reason to believe that we have here the first introduction of the hyperbolic functions usually attributed to Lambert in 1768; that is, exactly one hundred years after Dary had noticed the relationship. He seems also to have considered the question of ruled surfaces, and when considering the forms of tuns, or vessels, he introduces the term cylindroid. He also appears to have introduced a method of quadratures by the aid of finite differences, "for which," he says, "I have had abundance of contemplative solace and satisfaction." It is also to be noticed that he uses the term "congruent set of intervals," and it would be interesting to know if this is not the first time the expression is used.

Dary's greatest claim to immortality, and the one which most intimately concerns us in this paper, is the solution of algebraic equations by the process of iteration.

This in modern notation may be written—the limit $\phi^n(x)$ when n increases without limit is a solution of $\phi(x) = x$.

It illustrates how important theorems are forgotten, that when some years ago the Editor of this Journal showed me a paper he had written, on the solution of the general equation, I immediately observed that the method he had used was essentially that of Dary (a man of whom the author of the paper had never heard). This was only partly the fact, because Sir Ronald Ross in his solution had gone further into the conditions

of convergence, and had given numerous graphical illustrations of the paths pursued in approximating to the roots.[1]

Now, although Dary was well acquainted with cartesian geometry, he gives no graphical illustration of his method, and there seems to be no reason to suppose he had considered the possibility that his method might fail to give a commensurable root in finite terms, or to give the roots when two or more roots are nearly equal, or that he had considered the rate of convergence of his successive substitutions.

That the method of approximation usually called Newton's method should rather be called Dary's method will be seen by those who are familiar with his works, the chief of which are :

1. *The General Doctrine of Equations numerically laid down in three chapters concerning the Invention, Reduction, Solution, of an Equation.* Printed for the author in the year 1664, London.

2. *Dary's Miscellanies.* London, 1669.

3. *Interest Epitomised both Simple and Compound.* London, 1677.

4. *The Complete Gauger in Two Parts—Theoretical and Practical.* By Michael Dary, Philomath. London, 1678.

In the first of these works, Dary deals most fully with the solution of equations and also with interest problems, and gives in tabular form the solutions of the whole twenty equations connected with a geometric progression, *i.e.* if A be the first term, U the last, R the ratio, N the number of terms, S the sum of N terms—given any three of these quantities, find the remaining two.

Now the point which in this paper we most desire to bring to the notice of our reader is, the great number of times this method of iteration (due to Dary) crops up ; Legendre, in the *Supplément à l'Essai sur la Théorie des Nombres*, second édition, Février 1816, introduces the method and considers functions which he calls " fonctions omales," which always increase or always decrease.

[1] On January 17, 1909, I received a MSS. from Sir Ronald Ross in which he gave a résumé of his very extensive researches into the Solution of Equations by Iteration, considering the methods of dealing with the rapidity of convergency or divergency and the measure of oscillation of the descending and ascending series of operations, with suggestions for the application of same to numerous well-known equations.

Fourier, in his *Theory of Heat* (English translation, p. 272), uses the method for the solution of—

$$\epsilon = \text{arctan } \epsilon/$$

and gives graphical illustrations.

Vogel, Isenkrahe, and Lemeray have given numerous papers on the subject, and the present Astronomer Royal for Ireland sent me a paper in which he had used it, and in a letter said that although he thought the method must have been previously used, he could not remember having seen it anywhere. In a paper now appearing in the *Proceedings of the London Mathematical Society* by Mr. E. H. Neville, we find the same method is used.

Perhaps a few details of the process may help to a clearer understanding of the subject, and enable us to comprehend in what way Dary's method differs from the methods of those who have rediscovered it.

The method of Dary to which the name of iteration has been given consists in the transformation of an equation to such a form that the continual repetition of some functional operation, such as the finding of a square, cube, or higher root, may lead us by successive steps to the value sought.

In *The General Doctrine of Equations* (printed in 1664), we find that the method pursued by Dary is the well-known continuous depression of an equation by substituting for the unknown a binomial $l + n$.

Dary gives a summary of the known properties of equations, and in simultaneous equations eliminates, or, as he calls it, *purges* the equations of all unknowns except the one desired to be known.

He then deals with the "clearing and trimming of an equation whereby to know how many and what kind of roots it hath." He further shows that the coefficients are symmetric functions of the roots.

For the solution he substitutes in place of the unknown a binomial, and says: "Put $l + n$ for unknown, which Potestates of $l + n$ being orderly placed do make up a canon for the equation proposed. Which canon may be considered to consist of two parts, the head and the tail. The head consists of those parcels that have not the secondary root n in them, the tail

consists of those parcels that have the secondary root in them."

In other words, the method used by him at the date 1664 consists of the usual one of finding an equation whose roots are less than those of the proposed equation by a given amount l.

On the morning of August 15, 1674, Dary however has a new idea, that of iteration; he is so pleased with it, that although he had sent Newton three papers the day before, he says: "I cannot refrain from sending you this," and adds, "Truly it pleaseth me well, but yet I do hereby submit it to your censure."

The example he gives in his letter is:

$$+ z^p = az^q \pm n$$

and the rule is this—"first guess at the root as nearly as you can, the nearer the better (not for necessity but for accommodation), and suppose that guess to be z," etc.

He then gets—

$$\begin{array}{ll} \sqrt[p]{+ az^q + n} = b & \sqrt[p]{+ ab^q + n} = c \\ \sqrt[p]{+ ac^q + n} = d & \sqrt[p]{+ ad^q + n} = e \\ \sqrt[p]{+ ae^q + n} = f & \sqrt[p]{+ af^q + n} = g, \\ \text{etc.} & \text{etc.} \end{array}$$

The examples of the quintic which he solves are given in the Bring-Jerrard form, and the evaluation of the fifth root is performed by the aid of logarithms.

Let us consider for a moment in what way the iterative methods of modern writers differ from that of Dary. The most important difference is this: Dary does not show he was aware that his method was not universally applicable; that he might for instance have such an equation, and make such an initial guess, that instead of approaching the root, he might oscillate about it. He does indeed experience some difficulty, and says of one equation, "But this soure crabb I cannot deale with by no method."

The modern mathematician, with his greater familiarity with curves and their singular points, and his wider idea of functions, looks at the problem in a much more general way. For instance: given any algebraic equation, he knows that

it can be divided into two parts separated by the sign of equality, *i.e.* it may be put in a variety of ways into the form—

$$y = f_1(x) = f_2(x)$$

If the graphs of $f_1(x)$ and $f_2(x)$ are obtained, the points of intersection of the two curves are, of course, the values required. Now it may be difficult to obtain the graphs, and even if they are obtained, as the problem is to find the numerical value of the roots, we are just as far as ever from obtaining the value *at* the point. Thus we might continually approach the value along a certain path without ever reaching it, or we might oscillate about it.

We have no room here to enter into the history of solution by approximation, but some of the difficulties in the solution by the method of iteration will be seen from the consideration of the following examples. If we take the cubic equation—

$$x^3 - 15x^2 + 75x - 125 = 0$$

which has three roots each equal to 5, as the coefficients are symmetric functions of the roots, the last term 125 being the product of those roots, we should expect a small change in the coefficients to produce correspondingly small changes in the roots, but by changing 125 to 124 we have the equation—

$$x^3 - 15x^2 + 75x - 124 = 0$$

whose roots are 4 and $\dfrac{11 \pm \sqrt{-3}}{2}$ (two imaginaries). Now this curious property of equations has been long known. James Logan, born at Lurgan, Armagh, on October 20, 1674, who accompanied William Penn, as Secretary, to America in 1699, in a letter to Sir Wm. Jones, dated from Philadelphia, May 4, 1738, gives the following equation:

$$x^4 - 24x^3 + 216x^2 - 864x + 1296 = 0$$

which has four equal roots 6, and he observes: If you change the $-24x^3$ to $-25x^3$ the root will sink from 6 to less than 3·5, and if you change it to $-23x^3$ the roots are imaginary.

Or we may consider the equation—

$$x^3 - 3x + 2{\cdot}000001 = 0.$$

One of the roots being obviously very nearly equal to 1, if we

divide by $x - 1$ and neglect the small remainder, we have the equation

$$x^2 + x - 2 = 0$$

whose roots are 1 and -2; or we may suppose one of the roots to be 1·0001, the second ·9999, and the third -2; or we may suppose two roots to be imaginary, namely $1 \pm \cdot 0001\sqrt{-1}$.

It is thus obvious that if two or more roots are nearly equal, an inaccuracy of the approximation to those roots which are employed in the depression of the primitive equation may convert real roots into imaginary, or conversely.

De Morgan in 1855 gives the following problem: "Given an equation with a parcel of equal roots, and given certain infinitesimal alterations in the coefficients, required to know what becomes of the equal roots; how many remain equal, and how many are $(a \pm b\sqrt{-1})$ed to invent a new participle."

A consideration of this question by Kronecker (1890–1891) has led him to the following theorem.

Every rational integral function $f(x)$ of the nth degree with rational numerical coefficients can, by a variation of the coefficients which is definite in character, but as small as one may please, be reduced to a product of ν linear and $(n - \nu)/2$ quadratic factors, all with rational coefficients, ν being an integer determined by the coefficients of $f(x)$.

Besides this difficulty of separating the roots of an equation when the roots are nearly equal in value, algebraists have occupied themselves with the search for rules, by means of which the limits between which the roots lie can be obtained, and many interesting properties of the equations have been found. Those who are interested in the subject should consult a paper by M. Bret in the sixth volume of Gergonne's *Annales des Mathématiques* where several methods of finding the limits of roots are given. One of the most interesting is the following:

"If we add to unity a series of fractions whose numerators are the successive negative coefficients, taken positively, and whose denominators are the sums of the positive coefficients including that of the first term, the greatest of the resulting values will be a superior limit of the roots of the equation."

Thus in the equation

$$2x^7 + 11x^6 - 10x^5 - 26x^4 + 31x^3 + 72x^2 - 230x - 348 = 0$$

the number 4, which is equal to the greatest of the quantities

$$1 + \frac{10}{13}; \ 1 + \frac{26}{13}; \ 1 + \frac{230}{116}; \ 1 + \frac{348}{116};$$

is a superior limit required, and if we change the signs of the alternate terms, we shall have $1 + \frac{11}{2}$ or 7, a superior limit of the roots of the resulting equations. Therefore the real roots of the first equation lie between 4 and -7.

We have in quite recent times many papers on the subject by Landau in 1907, Fejur in the same year, Carmichael and Mason in 1914. These two last mentioned give the interesting result that—

" *All the roots of the equation*

$$x^n + a_1x^{n-1} + a_2x^{n-2} + \ldots + a_n = 0$$

are in absolute value less than or equal to

$$\sqrt{1 + |a_1|^2 + |a_2|^2 + \ldots + |a_n|^2}$$

It was natural, that after the complete solution of the cubic and biquadratic equations, especial attention should be given to the quintic, and this does not escape Dary's notice, and he is cute enough to choose the Bring-Jerrard form for his example.

It is only since Abel's time we have been familiar with the fact that the quintic could not be completely solved without the aid of elliptic and hyper-elliptic transcendentals, and we now know that the complete solution of an equation of the fifth and higher degrees, by means of the elementary operations of addition, subtraction, multiplication, division, and a finite number of root extractions, can only be obtained when the group of the equation is soluble.

Modern writers on the subject such as Lemeray, Isenkrahe, Ross, have occupied themselves with considerations of the various forms of approach to the roots, and have also considered the rate of convergence.

Dary, who lived 200 years before the rigid ideas of convergence had taken hold, was not likely to have realised the difficulties involved, which were such as to make De Morgan

write—"I suspect all progressing series except when I know the producing function, and I trust all alternating series even when I do not know the producing function," and he speaks of divergent series as "wicked infinities in a finite disguise, pretending to be finites in an infinite disguise."

Having considered the separation of the roots into *real* and *imaginary*; positive and negative; and the determination of the limits between which the real roots lie; we have next to consider the numerical approximation to their values.

Many methods have been discovered, which vary greatly both in their theoretical perfection and practical application. Dary in his first method uses tables or "canons" he has constructed of "Potestates" arising from a binomial, but after his discovery of iteration he uses logarithms for the evaluation of the successive roots. But the method of arranging the work for the continuous reduction of an equation discovered by Ruffini in 1804 and rediscovered by Horner, 1819, is so expeditious that it seems as though further simplification is not to be expected.

It is true that Budan in 1807 also obtained a method of transformation of which the numerical work is very simple, but the number of steps required takes away from the value. Thus by the Ruffini-Horner method, the equation would be reduced to another having its roots less than the roots of the original equation by 9, in one step, while it would require nine steps by the Budan method.

One disadvantage of the Ruffini-Horner method is that in equations of advanced degrees in which several terms are absent, the method of reduction introduces significant numbers in place of the zero coefficients. A method of avoiding this has been introduced by Mr. Weddle of Newcastle-on-Tyne, the peculiarity of whose method is, that it conducts its steps by aid of transformations, through all of which the zero coefficients are transmitted.

If we ask why it is that Dary and his work have passed into oblivion, a little consideration will perhaps show us that the only person likely to be remembered by posterity is the man who is popularly supposed to have completed something.

Cardan's name lives in connection with the cubic, and Ferrari with the biquadratic, because these men completed a step in the history of mathematics.

Had the complete solution of the quintic been possible, and had Dary found it, or had he succeeded, like Abel, in proving it impossible, his name would be known to all mathematicians, but the men who fail to complete a piece of work, however near they come to the completion, and however necessary their work may be for the final result, are nearly always forgotten (except by the most diligent digger into the past), although in intellect they may be far beyond the fortunate completer.

Furthermore it is the habit of the half-educated man to attribute everything to the one name he knows: all military maxims to Napoleon, all mathematics of the seventeenth or eighteenth century to Newton; it is only those who go much deeper into history who find out that the greatest men have not done all the work.

The English mathematician who requires some information of the history of mathematics labours under a great disadvantage. There are no books to help him, as it is not an outline of his subject that he requires, but a comprehensive treatise, dealing in detail with the successive discoveries. Montucla's *Histoire des Mathématiques*, in four large quarto volumes, has no index, is very incomplete, and can only be obtained second-hand at great cost.

Cantor's work is also costly, and besides the disadvantage of being in German is very defective where British mathematicians are concerned, for unfortunately the English mathematicians imitate the cuckoo, in putting their eggs in strange nests.

Who would think of looking for valuable mathematical problems and theorems in papers bearing such titles as *The Ladies' Diary*, *The Gentleman's Diary*, *The British Diary*, *The Leeds Correspondent*, *The Northumbrian Mirror*, *The Liverpool Student*, *The Miscellanea Curiosa*, and numerous others with titles giving no index to the contents? And yet, if the history of mathematics is to be written, it is amongst such papers that we must search.

What mathematician would think of looking into a work bearing the strange device *Instruction given in the Drawing School established by the Dublin Society*? And yet this book, written by Joseph Fenn (a name almost forgotten in mathematics) gives the first example of the use in the British Isles

of the notation of Leibnitz for the differential and integral calculus, and moreover its complete discussion of the complex variable, the form—

$$(a + b\sqrt{-1})^{\alpha+\nu\sqrt{-1}}$$

and its differential is shown as clearly as it is in the work of Hartnack.

How many of our readers are familiar with the work of Brakenridge, Wolfenden, Butterworth, and Holditch? How many are acquainted with the valuable papers by Weddle, on the construction of logarithms, on the properties of curves, his hundreds of interesting problems about the triangle, and his work on equations?

Few indeed have used the extensive tables of dual numbers provided by J. Byrne, and yet these tables, although not perhaps of the value attributed to them by the author, have been found very useful in numerous calculations.

We began this article by calling attention to the loss sustained by science through a neglect of the past history of the subject, and unless we take deliberate steps to counteract this, we are likely to suffer more and more, for with the exception of Sir Thomas Muir, Sir T. L. Heath, and Dr. Gow, we have been in the habit of leaving the history of mathematics to the Germans, who by their splendid collection of works contained in Ostwald's *Klassiker der exakten Wissenschaften*, the *Bibliotheca Mathematica*, and the *Jahrbuch über die Fortschritte der Mathematik*, have made it possible for us to dispense with native historians.

But those who are familiar with German histories will realise how strong national prejudices are, and will have learnt not to expect impartiality. Moreover, there is very great likelihood that we shall have to dispense with German aid for a long period, as the continuance of the *Bibliotheca Mathematica* and *Jahrbuch* must be considered very doubtful.

However painful it may be, we must develop our own resources, and it is well that we should ask ourselves how this can be best effected.

One of our most pressing needs is a set of works dealing with the history of special branches. Complete histories of the Complex Variable, of the Theory of Groups, of the Solution of Algebraic Equations, of Finite Difference Equations,

and of Interpolation, of the various attempts at the interpretation of fractional and imaginary exponents in differentiation, and of Integral Equations, are a few of the desiderata. We have a *History of Greek Geometry* but no history of geometry since Grecian times; we have indeed some excellent works by Sir T. L. Heath and Dr. Gow on Greek mathematics, but of the period since then hardly anything. Sir Thomas Muir's famous *History of Determinants* illustrates how the lacunæ might be filled. In science it is almost essential that our history should be written backwards, the nineteenth century being wanted more than the eighteenth, and the eighteenth than the seventeenth. This, of course, creates difficulties, but they are not insuperable.

That we know little of the history of a few generations ago can be illustrated by the fact that Dr. Salmon in his *Modern Higher Algebra* refers the introduction of the term Canonical to a contemporary, Hermite, yet the term "Canonical Equation" is used by James Logan in the aforementioned letter, and used in such a way that it seems it must have been in general use; the word "congruence" calls to mind the name Gauss, yet Dary used it in the year 1664, nearly two hundred years before Gauss.

Perhaps the greatest of all aids would be the establishment of a Central Bureau, where all discoveries and completed works could be indexed on cards, and copies of the cards kept in all the large public libraries; such an attempt was started and the cards distributed by M. Gauthier-Villers, but after 10 packs of 100 cards each had been sent out, the method was discontinued for some reason, and a very inferior plan of publication in book form adopted.

Many businesses have learnt the value of the card index as a means of recording the results achieved, and as a means of protection against the wiles of the wicked, all contributors of their own experience receiving copies of the experience of their associates. Were this done in mathematics, we should probably soon find an enormous increase in the value of the output, for a pygmy on the shoulders of a giant can see farther than the giant.

If it is not considered derogatory for mathematics to take an analogy from business, we may say that carefully-managed businesses have periodic stock-takings; the set of histories

suggested above might be looked upon as such stock-taking, enabling workers to see where they stand, what gaps require filling, what rubbish should be removed. When such stock-takings are centuries apart (as in science) it will readily be understood how much waste obscures the view, and how much new material is required. But the question arises, who shall we get to fill up these gaps?

It is a *necessary* but not a *sufficient* condition that some remuneration should be paid, because even a mathematician cannot live on imaginaries, and it should be obvious that such men as are required cannot be obtained by purchase at a moment's notice. It is necessary for them to prepare themselves by a long course of study and contemplation from their youth up.

Now youth is naturally ambitious: the best of them intellectually are desirous not of great wealth, but of success and glory; they are desirous of outstripping the efforts of their contemporaries.

The consciousness of power, and the conviction of successful exertion, may exist undiminished by the neglect or the ingratitude of their own country, and the knowledge that distant posterity may repair the ingratitude of the present may sustain them. Philosophic old age may cease to be moved either by praise or blame, but great rulers and great generals know full well the power of immediate recognition of success as incentives to valour, and that man is a fool and not a statesman who hesitates (in the fields of science) to use such incitement to human action for the benefit of his country and of mankind.

THE SOLUTION OF EQUATIONS BY OPERATIVE DIVISION.[1]—PART I

By SIR RONALD ROSS, K.C.B., F.R.S., D.Sc.

CONTENTS OF PART I

I. THIRTY years ago I devised a notation which enables us to express any algebraic operation without stating the actual number upon which it operates; and I called such an expression a Verb Function, because it denotes rather the action taken in arranging quantities than the quantities themselves. The notation adds greatly to our powers of mathematical expression; gives us a precise algebra of substitution; and enables us to render quite rigid the symbolic algebra which was extensively developed by Boole for the Calculus. A lengthy paper on the subject, called "Verb Functions with Notes on the Solution of Equations by Operative Division," was published ten years ago (*Proc. Roy. Irish Academy*, vol. xxv. sec. A, No. 3, April 1905, pp. 31–76)—in which I described the notation, the elements of the "operative algebra" which it leads to, and also an application to the solution of algebraic and differential equations which was, so far as I can ascertain, a new method. Some years later I found independently another method for approximating to the roots of numerical equations by "iteration" (*Nature*, October 29, 1908); but my friend Mr. Walter Stott subsequently informed me that this second method had been originally discovered by that redoubtable "philomath" Michael Dary, a contemporary

[1] Written in connection with Mr. W. Stott's paper.

of Newton, in or before 1674, and has since then been rediscovered by Legendre, W. Heymann, and others (who did not, however, add much to the original conception); while, in my opinion, Newton, to whom Dary communicated his invention, may even have used it in devising his own famous method of approximation, which is, indeed, nothing but the quickest form of "directed iteration." However this may be, it now became obvious to me that my original method of solution by operative division gives the general algebraic expression for Dary's arithmetical solution by iteration; and I mentioned this in a third and brief paper (*Nature*, February 4, 1909). Still more recently I have found that it gives the general algebraic expression for Newton's method also—thus completing the story.

My paper on Verb Functions has, I believe, received no attention beyond a short approval in *Nature*, which, however, did not refer to operative division; and, so far as I can ascertain, not even Dary's, Legendre's, and Heymann's ideas receive so much as the honour of mention even in the most comprehensive and recent textbooks on the theory of equations —doubtless because the mathematicians who write these books are fully occupied by graver matters. But the methods are so pretty that they will be of interest to modern "philomaths" —not to mention those who, like myself, scarcely dare aspire even to this grade of mathematical knowledge and are content to call ourselves amateurs. Moreover, workers in many branches of science often require to solve equations; and a restatement and completion of the whole subject may, therefore, be not out of place in the pages of SCIENCE PROGRESS.

II. I will begin by describing the notation suggested by me for "operative algebra."

Let $\mathbf{o}$ be an operator such that $\mathbf{o}^n$ denotes the operation of raising a number or other subject to the nth algebraic power; and let square brackets be used to denote *operation upon* as distinct from *multiplication into*.[1] Thus for example:

$$[\mathbf{o}^n]\, x = x^n$$
$$[a\mathbf{o}^0 + b\mathbf{o}^1 + c\mathbf{o}^2]\, x = a + bx + cx^2$$

[1] I used β for $\mathbf{o}$ in my first paper ("Verb Functions," 1905), but the latter is more convenient. The square brackets [ ] may also be replaced by a double dot for short, as in $\epsilon^{\mathbf{o}} : x = \epsilon^x$. A single dot now expresses multiplication, as in $\epsilon^{\mathbf{o}} . x = x\epsilon^{\mathbf{o}}$.

We must now at once distinguish between an *operation* and a *function*. The former is the act of grouping numbers together in a particular manner; the latter is the result of such grouping when it is performed. The former is a verb, while the latter is a substantive; and the proposed notation gives us the power (which strangely enough we have not previously possessed) of expressing any algebraic operation by itself apart from the number or other subject to which it may be applied. Thus, since $o^o = 1$, we may now reason directly about the operation $a + bo + co^2$; or the operation $\frac{o}{1+o}$; or the operation $bo + \sin o + \log(1+o)$. The operation $1 + o$ is evidently that of adding unity to the subject; and the operation bo is that of multiplying the subject by b; and the operation $\sin o$ is that of taking the sine of the subject.

(2) An operation may be reversed, that is, may consist in *undoing* rather than in *doing*. We denote this by writing a minus sign as an index outside the square brackets. Thus $[1+o]^-$ denotes subtracting unity from the subject; and $[bo]^-$ denotes dividing the subject by b. Properly speaking, there is no such thing as a negative number, any more than there is a negative stone or a negative elephant. It is only the operation, that is, the action we perform either in grouping numbers or in measuring distances, which is negative—that is, is reversed. But it is convenient to use negative numbers in order to facilitate writing, so long as we clearly understand that we do so only by convention. Thus for example:

$$[a + bo]^{-1} = \frac{o - a}{b} \qquad \left[\frac{o}{1+o}\right]^{-1} = \frac{o}{1-o}$$

(3) An operation or reversed operation may be performed upon another operation or reversed operation. Thus for example:

$$[a + bo + co^2]\,[p + o] = a + bp + cp^2 + (b + 2cp)\,o + co^2$$
$$[a + bo + co^2]\,[p + o]^{-1} = a - bp + cp^2 + (b - 2cp)\,o + co^2$$
$$\left[\frac{a}{1-b} + o\right][bo]\left[\frac{a}{1-b} + o\right]^{-1} = a + bo$$
$$(a + bo)^2 : \frac{1}{b}(\sqrt{o} - a) = o$$

(4) The performance of an operation upon itself any number of times may be called *operative volution*, or *iteration*, and may

be denoted by an index placed outside the square brackets. For example:

$$[o]^n = o \quad [a + o]^n = na + o \quad [bo]^n = b^n o \quad [o^m]^n = o^{m^n}$$

$$[a^o]^3(1) = a^{a^a} \qquad [\log_o a]^3(\epsilon) = \log_{\log_{\log_a a}} a^a$$

$$\left[\frac{o}{1 + o}\right]^n = \frac{o}{1 + no} \qquad [a + bo]^n = a\frac{1 - b^n}{1 - b} + b^n o$$

$$[a + bo^{-1}]^n = a + \frac{b}{a +}\frac{b}{a +}\cdots\frac{b}{a + bo^{-1}} \qquad \left[\frac{o}{1 + o}\right]^{-n} = \frac{o}{1 - no}$$

(5) It is always stated in the books that if ϕ is any operation then $\phi\phi^{-1} = \phi^{-1}\phi = \phi^0 = 1$. But ϕ^0 does not equal numerical unity, and, as argued in "Verb Functions" (p. 36), cannot possibly do so. In fact we shall see, if we use the present notation, that when an operation is performed on the same operation reversed (or the converse), then the result is always, not 1, but o. For example:

$$[bo]\ [bo]^{-1} = [bo]\left[\frac{1}{b}o\right] = o\,;$$

$$[a + bo]^{-1}\,[a + bo] = \left[\frac{o - a}{b}\right][a + bo] = o.$$

That is to say:

$$[\phi]\ [\phi]^{-1} = [\phi^{-1}]\ [\phi] = [\phi]^0 = o.$$

What then is the meaning of this symbol o? Obviously it has no numerical value, because it merely denotes, so to speak, the empty space which should be occupied by the principal variable in a function. It really expresses what has been called the *identical substitution*, since $[o]\ x = x$. But when we compare the algebraic law that $aa^{-1} = a^0 = 1$, we gather that $[\phi]^0$ or o may also properly be called, not the numerical unit, but the *operative unit*. This becomes numerical unity only when affected by the algebraic power of zero; and the error referred to seems to have greatly retarded the development of operative algebra, even when it is applied to the Calculus under the name of the symbolic notation. With this correction, however, operative algebra becomes as exact as numerical algebra or quaternions.

(6) In extension of this notation, $[\phi]\,[\psi]^{-1}$ may be called an operative ratio and may also be written $\frac{\phi}{\psi}$—a double line being

used in order to distinguish it from an algebraic ratio. Thus if $\phi = \chi\psi$, $\frac{\phi}{\psi} = \chi$. As however operations in operative multiplication are not commutative as are numbers in numeral multiplication—that is, $\phi\psi$ [1] does not always $= \psi\phi$—so we may have to distinguish between $\phi\psi^{-1}$ and $\psi^{-1}\phi$; and the former may therefore be also written $\phi /\!/ \psi$ and the latter $\psi \backslash\!\backslash \phi$—but this is seldom required. The following propositions are almost self-evident:

$$\frac{\phi}{\phi} = 0 \qquad \frac{\phi+\psi}{\chi} = \frac{\phi}{\chi} + \frac{\psi}{\chi} \qquad \frac{\phi\psi}{\chi\psi} = \frac{\phi}{\chi} \qquad \frac{\phi}{\psi}\frac{\psi}{\chi} = \frac{\phi}{\chi}$$

and we must remember that $[\phi\psi]^{-1} = \psi^{-1}\phi^{-1}$. Obviously also $\phi^{-1} = \frac{0}{\phi} = \phi \backslash\!\backslash 0$.

But operative ratio may exist between numbers as well as between operations. If $y = \phi x$, we may write $\frac{y}{x} = \phi$; so that this operative ratio denotes the operation which connects the two quantities y and x. But here we tacitly assume that y and x are *variable* quantities, capable of an infinite number of values, all connected by the operation ϕ. On the other hand the ratio $\frac{a}{b}$, where a and b are single quantities, will denote an infinite number of operations capable of connecting them. In other words, the operative ratio $\frac{a}{b}$ denotes *any curve which passes through the point of which the co-ordinates are a and b.* If the same curve also passes through the points (c, d), (e, f), etc., we have the series of equations

$$\frac{a}{b} = \frac{c}{d} = \frac{e}{f} = \text{etc.} = \phi \text{ (say)};$$

and if the form of ϕ is given, we can determine its constants by the Euler-Lagrange interpolation formula or otherwise. Where the form of ϕ is not given, it can be determined if the number of equations, that is, of points passed through, is large enough.

[1] The square brackets may be neglected where the meaning is obvious without them. For the numerical product of two operations such as ϕ and ψ can be clearly expressed by $\phi \,.\, \psi$, and the numerical powers of ϕ by $(\phi)^n$.

(7) This suggests a most interesting *operative geometry* which I can recommend to fellow amateurs, and which is slightly different from Cartesian geometry because in it curves represent, not functions resulting from operations, but the *operations themselves*. Thus, with angular co-ordinates, $a + b\mathrm{o}$ is a straight line, and ϵ is an exponential curve, and so on; and o itself is the straight line given by the equation $y = x$ in Cartesian geometry. In fact the application of this operative notation to geometry, and especially the use of what may be called *the geometrical operation*, furnish beautiful proofs of many trigonometrical and Cartesian propositions—almost rivalling quaternions in this respect. I propose to write a paper upon the subject some day, but at present give only the shortest possible statement as an introduction to the proper theme of this article.

(8) We now proceed to *operative division*. If $\phi(x) = \chi(x) \times \psi(x)$ and $\phi(x)$ and $\psi(x)$ are known and we wish to find the value of $\chi(x)$, then we can often do so by the rule of *ordinary* division. In similar cases we can also generally find the form of the operation χ when $\phi = [\chi]\,[\psi]$ by a similar rule of *operative* division. Suppose for instance that we have given the equation $[c\mathrm{o}^2 + b\mathrm{o} + a]x = \mathrm{o}$ and we wish to transfer the origin to the left or negative side by putting $x = y - p$. We then have to find the form of

$$[c\mathrm{o}^2 + b\mathrm{o} + a]\,[\mathrm{o} - p]\,y\,;$$

which can be done easily enough by Taylor's theorem, or by ordinary simplification, or by the means used for Horner's method. But operative division gives perhaps the quickest and most easily remembered process. For, since $\phi\psi = \dfrac{\phi}{\psi^{-1}}$, we have

$$[c\mathrm{o}^2 + b\mathrm{o} + a]\,[\mathrm{o} - p] = \frac{c\mathrm{o}^2 + b\mathrm{o} + a}{\mathrm{o} + p}\,;$$

and we proceed as follows:

$$\begin{array}{l}
\mathrm{o} + p\,\big]\,c\mathrm{o}^2 + b\mathrm{o} + a\,\big[\,c\mathrm{o}^2 + (b - 2cp)\mathrm{o} + (a - bp + cp^2) \\
\qquad\quad \underline{c\mathrm{o}^2 + 2cp\mathrm{o} + cp^2} \\
\qquad\qquad (b - 2cp)\mathrm{o} + (a - cp^2) \\
\qquad\qquad \underline{(b - 2cp)\mathrm{o} + (b - 2cp)p} \\
\qquad\qquad\qquad\qquad a - bp + cp^2 \\
\qquad\qquad\qquad\qquad a - bp + cp^2
\end{array}$$

The quotient, therefore, agrees with the second example in sub-section (3), and the given equation now becomes:

$$cy^2 + (b - 2cp)y + (a - bp + cp^2) = 0.$$

The rule differs from that of algebraic division only in the point that each term of the quotient *operates* on the whole divisor instead of being *multiplied into* it. To find the first term of the quotient, we ask what function of o operating on the first term of the divisor, namely o, will convert it into the first term of the dividend. The answer is evidently co^2. This term of the quotient now operates on the whole divisor, and, subtracting the result, we have the second dividend. To obtain successive terms of the quotient we proceed in exactly the same way. The last term is easiest of all; because a number affected by o^0, that is unity, merely reproduces itself when it operates on anything.

Again, suppose that we wish to "centre" an equation—that is, to transform it so that the sum of the roots of the transformed equation and therefore the coefficient of the second term shall be zero. For this purpose we transfer the origin by $1/n$th of the coefficient of the second term of the original equation with its sign changed. Example $x^3 - 3x^2 - 2x + 5 = 0$.

$$o - 1]\ o^3 - 3o^2 - 2o + 5\ [o^2 - 5o + 1$$
$$\underline{o^3 - 3o^2 + 3o - 1}$$
$$-5o + 6$$
$$\underline{-5o + 5}$$
$$1$$

Thus in this case the centred equation is $y^3 - 5y + 1 = 0$.[1]

As in algebraic division the successive terms of divisor and dividend must both be in descending or both in ascending order. In the former case we have "descending division"; in the latter case "ascending division." If the first term of the divisor is simply the first power of o, the division may be called "simple." If not, the amateur will perhaps find out for himself how to carry it out, even before he reads the rest of this paper! Many more examples are given in my "Verb Functions," where operative division is also used for separating an equation into operative factors (which can sometimes be

[1] When the divisor contains a fraction, it should be dealt with as shown in 3 (5), Example N.

done) and for obtaining Cardan's solution, and where "superior division" and "synthetic operative division" are also referred to, and the use of the Rule for solving differential equations touched upon.

Of course the results, both of numerical and of operative division, are nothing but absolute identities if the remainder is not neglected. Thus, in one case,

$$\frac{0}{\phi} = \frac{c_0\phi + 0 - c_0\phi}{\phi} = c_0 0 + \frac{0 - c_0\phi}{\phi}$$
$$= c_0 0 + \frac{c_1\phi^2 + 0 - c_0\phi - c_1\phi^2}{\phi} = c_0 0 + c_1 0^2 + \frac{0 - c_0\phi - c_1\phi^2}{\phi}$$

and so on. And the algebraic ratio $\frac{1}{\phi}$ may be treated in precisely the same way.

III. It is, of course, well known that the invert of a rational integral operation of a degree higher than the fourth cannot be expressed in *finite* algebraic terms; but by means of operative division we can always find an algebraic expression in the form of a convergent infinite series for each separate real root, or sometimes for two or more roots, or for all the roots together—though I have not yet ascertained how actually to calculate more than one or two real roots from the last kind of series.

The process by which this is done is very simple.

Let $$[\phi]\, x = y.$$

Then $$\frac{0}{\phi} : [\phi]\, x = \frac{0}{\phi} : y,$$

and $$x = \frac{0}{\phi} : y.$$

We then find the form of $\frac{0}{\phi}$ by operative division, apply it to y, and equate the result to x.

Now most equations $[f]\, x = 0$ may be put in the form $[\phi]\, x = y$ in many different ways which may be called *settings*; and many different settings will yield different series. The questions are, to which root or roots does each series apply; when is it convergent; and which series gives the quickest approximation?

In this part of my paper I will deal only with the applica-

tion of *simple ascending division* for this purpose. It is suitable chiefly for equations of low degree, such as those which are mostly dealt with in text-books on the theory of equations. But I am by no means sure that it is the best method even for these; while owing to the fact that the method generally requires at least one transference of origin, it often becomes laborious for equations of high degree or transcendental equations—which, however, can frequently be readily solved by other kinds of operative division, or by the arithmetical processes which they represent. Simple operative division (when the first term of the divisor is 0) is, however, easy to remember and to apply.

(2) For a first example we will find the lesser positive root of the equation $0 = a' - b'y + c'y^n$.

Observe that it is written with the absolute term a' positive and leading. We can always reduce the coefficients of the first two terms to unity by putting $y = \frac{a'}{b'}x$ and dividing by a', which gives for solution an equation of the form

$$0 = 1 - x + cx^n.$$

When $x = 0$, this function $= 1$; that is the function cuts the axis of y at a unit's distance above the origin; and as the absolute term of the tangential, $-1 + ncx^{n-1}$ is also unity when $x = 0$, the function begins by descending towards the axis of x at an angle of $-45°$. But if c is large, the positive term cx^n soon exerts its influence and causes the curve to turn upwards before it can reach the axis of x at all; so that the two possible positive roots become unreal. Obviously, by Descartes' Rule the equation has no negative root when n is an even number; but certainly has one, and only one, when n is odd. It is easy to ascertain whether the two positive roots are real or not. For the tangential vanishes when $x = \sqrt[n-1]{\frac{1}{nc}}$. If $1 - x + cx^n$ is positive at this value of x, the roots are unreal; if it is zero, the roots are equal; and if it is negative, the roots are real. That is, the roots are unreal if

$$c > \frac{(n-1)^{n-1}}{n^n};$$

as is well known.

Now write the equation in the form

$$x - cx^n = 1$$

and find the form of $\dfrac{0}{0 - c0^n}$ by operative division. Suppose first that $n = 2$. Then

$$\begin{array}{llllll}
0 - c0^2]\,0 & [0 + c0^2 + 2c^2 0^3 + 5c^3 0^4 + \text{etc.} \\
\quad \underline{0 - c0^2} \\
\qquad c0^2 \\
\qquad \underline{c0^2 - 2c^2 0^3 + c^3 0^4} \\
\qquad\qquad 2c^2 0^3 - c^3 0^4 \\
\qquad\qquad \underline{2c^2 0^3 - 6c^3 0^4 + 6c^4 0^5 - 2c^5 0^6} \\
\qquad\qquad\qquad 5c^3 0^4 - \text{etc.}
\end{array}$$

Applying this quotient to the absolute term, unity, we have for the lesser positive root.

$$\begin{aligned} x &= 1 + c + 2c^2 + 5c^3 + 14c^4 + 42c^5 + 132c^6 + \ldots; \\ &= \frac{1}{2c}(1 - \sqrt{1 - 4c}); \end{aligned}$$

as shown by expanding the radicle by the binomial theorem. The series is divergent if $c > \frac{1}{4}$, and the root is then unreal. If $c = \frac{1}{4}$, the roots are equal, but the convergence is slow.

For the cubic $x - cx^3 = 1$ we have by the same method,

$$\begin{aligned} x &= [0 + c0^3 + 3c^2 0^5 + \ldots]1 \\ &= 1 + c + 3c^2 + 12c^3 + 55c^4 + 273c^5 + 1428c^6 + \ldots \end{aligned}$$

The series is divergent and the positive roots are unreal if $c > \frac{4}{27}$; and these roots are equal, but the convergence is slow, if $c = \frac{4}{27}$.

For the general case $x - cx^n = 1$, on carrying out the operative division, we shall easily see that the successive coefficients in the quotient are reducible to a simple binomial form; and we have

$$[0 - c0^n]^{-1} = 0 - \frac{1}{n}(-n)c0^n + \frac{1}{2n-1}(1-2n)^{(2)}\frac{c^2}{2!}0^{2n-1} - \frac{1}{3n-2}(2-3n)^{(3)}\frac{c^3}{3!}0^{3n-2} + \text{etc.}$$

where the indices in brackets denote factorials — *e.g.* $q^{(m)} = q(q-1)(q-2)\ldots(q-m+1)$. This operation applied to the absolute term gives a numerical series which is divergent if the two possible positive roots are unreal, but which always converges to the value of the lesser positive root if it is real. The convergence is slow if the two roots are equal or nearly equal; rapid if they are far apart.

If c is negative in the original equation write $0 = 1 - x - cx^n$, and the series obtained for $[0 + co^n]^{-1}$ will have alternately positive and negative signs. In this case there must be one, and only one, positive root, and this is given by the series if c is numerically small. But if c is numerically too large the series becomes divergent though the root is real—as for example in the equation $0 = 1 - x - x^2$. The series will certainly not be convergent if the slope of the curve $1 - o - co^n$ as it crosses the axis of x is algebraically less than -2; and the convergence is quickest when the slope of the curve at that point is nearly -1; facts which the ingenious amateur will doubtless be able to explain even before he reads the rest of this paper. But the slope of the curve can always be modified by suitable transformations as will now be described.

(3) We proceed to inquire how all the roots of an integral rational equation may be obtained in succession by simple ascending operative division.

In Horner's process we must first find by trial the integers between which a required root lies. The lesser of these integers is called *the first figure of the root*; and the origin is transferred to this point. The next figure of the root is obtained, again by trial, from the transformed equation, and the origin is again transferred to this second point; and so on, until the required degree of approximation is attained. In solution by simple ascending division we must often ascertain the first figure of the root by trial (or other means [1]); but a single division usually suffices for the rest of the approximation.

Write the general equation in the form

$$0 = a' - b'y + c'y^2 - d'y^3 + \ldots$$

Now for simple division it is absolutely necessary that the terms a' and $b'y$ and at least one of the other terms should exist; to ensure convergence of the quotient it is generally

[1] Which will be indicated in a later Part.

necessary or advisable that a' and b' should have different signs and that the absolute term, a', should be *as small as possible*; and it is convenient for the process of division that b' should be unity, and that as many of the other coefficients as possible should be integers.

In order that a' should be small and that a' and b' should have different signs, we must, if necessary, transfer the origin to a point less than and close to the required root (the nearest integer generally suffices). The nearer this point is to the root the smaller will be the absolute term of the transformed equation and the more certainly will its next term ($b'y$) have a different sign.

This being done, and supposing $0 = a' - b'y + c'y^2 -$ etc., to be the transformed equation (c', d', e', etc., having either sign), we have next to reduce the coefficient of the second term to unity. If we merely divide throughout by b', the new coefficients may all be fractions—which is not desirable. It is, therefore, usually better (A) to make the substitution $y = b'x$ and divide by b'^2, which will give $0 = \frac{a'}{b'^2} - x + c'x^2 - d'b'x^3 +$ etc.; in which, if the original coefficients were integers, the new ones, except the new absolute term, will be integers also. Or (B) we may make the substitution already suggested in (2) above, namely $y = \frac{a'}{b'}x$, and divide by a'; in which case the first two new coefficients will be unity, but the other coefficients will generally be fractions. These small changes do not affect the result but only facilitate the working; they are not absolutely necessary, and, for numerical work, the selection of the most convenient method will depend upon the original coefficients. By method (A), the absolute term should be considerably less than unity, to speak roughly; and by method (B) all the other coefficients should be less than unity—the coefficient of x being of course unity in both.

The final condition for convergence is, as stated at the end of the previous sub-section, that the slope of the curve at the required root must at least be algebraically greater than -2; and, for rapid convergence, is that it be about -1. Where the required root is multiple the slope of the curve will tend towards zero the nearer we approach the root, and the con-

vergence will be slow; but in this case multiplicity will be suspected, and may be detected by other means.

As an example of how transference of origin will change a divergent into a convergent invert take the equation mentioned at the end of last sub-section, namely $0 = 1 - x - x^2$. The invert of this is $x = 1 - 1 + 2 - 5 + 14 - 42 +$ etc. Now as the root evidently lies between $\frac{1}{2}$ and 1, transfer the origin to the former point and we have the equation $0 = \frac{1}{4} - 2z - z^2$; which gives, if $z = \frac{1}{8}y$, $0 = 1 - y - \frac{1}{16}y^2$. From this

$$y = 1 - \frac{1}{16} + 2\left(\frac{1}{16}\right)^2 - 5\left(\frac{1}{16}\right)^3 + \text{etc.} = 0\cdot 9441$$

and $z = 0\cdot 1180\ldots$, and $x = 0\cdot 6180\ldots = \frac{1}{2}(\sqrt{5} - 1)$.

These preliminaries being settled, let the general equation be written

$$[o - co^2 + do^3 - eo^4 + fo^5 - go^3 + \ldots]\, x = a;$$

so that $x = [o - co^2 + do^3 - eo^4 + fo^5 - go^6 + \ldots]^{-1}a$.

Proceeding to divide,

$$o - co^2 + do^3 - \ldots \,]\, o \,\, [\, o + co^2 + (2c^2 - d)o^3 + (5c^3 - 5cd + e)o^4 + \text{etc.}$$

$$\underline{o - co^2 + do^3 - \ldots}$$

$$co^2 - do^3 + eo^4 - \ldots$$

$$\underline{co^2 - 2c^2o^3 + c\,(c^2 + 2d)\,o^4 - \ldots}$$

$$(2c^2 - d)o^3 - (c^3 + 2cd - e)o^4 + \ldots$$

$$\underline{(2c^2 - d)o^3 - \quad (2c^2 - d)\,3co^4 + \ldots}$$

$$(5c^3 - 5cd + e)o^4 + \ldots$$

It is advisable to write out the successive powers of the divisor before beginning the division. We have finally,

$$\begin{aligned} x = a &+ ca^2 + (2c^2 - d)a^3 + \{5c\,(c^2 - d) + e\}a^4 \\ &+ \{14c^4 - 21c^2d + 3(2ce + d^2) - f\}a^5 \\ &+ \{42c^5 - 84c^3d + 28c(ce + d^2) - 7(cf + de) + g\}a^6 + \text{etc.} \end{aligned}$$

This series is given with weighted coefficients and directions as to how to obtain any more terms (which we may already suspect to be invariant) in "Verb Functions," pp. 52 and 53 especially. Together with the particular instances of it given in sub-section (2) above, it is already known, being obtained by

the far-from-elementary method attributed to Lagrange and developed by Murphy and also by way of Lagrange's and Burmann's differential expansions. But I can find in the books no information as to how to use it for calculating all the real roots of an equation, nor as to when it is convergent or not; nor indeed any note of its importance. It is, however, only one of the family of series given by operative division—which I believe will ultimately prove to be a process of elementary importance in algebra.

(4) These results may be written somewhat more formally as follows :

Let $0=f(x)$ be the proposed equation, and let $x_1, x_2, x_3 \ldots$ denote its real roots. Let

$$x_1=p_1+z_1 \qquad x_2=p_2+z_2 \qquad x_3=p_3+z_3 \quad \text{etc.};$$

and let $a, b, c, d, \ldots$ be now written respectively for $f(p_1)$, $-f'(p_1)$, $\frac{1}{2!}f''(p_1)$, $-\frac{1}{3!}f'''(p_1)$, etc. Then

$$0=f(p_1+z_1)=a-bz_1+cz_1^2-dz_1^3+\ldots$$

Putting $z_1=by$ and dividing by b^2,

$$0=\frac{a}{b^2}-y+cy^2-dby^3+eb^2y^4-\text{etc.}$$

Inverting this and writing s for $\frac{a}{b^2}$, we have

$$x_1=p_1+\frac{a}{b}\{1+cs+(2c^2-db)s^2+(5c^3-5cdb+eb^2)s^3+\text{etc.}\}$$

$x_2=p_2+$ similar terms in $f(p_2), f'(p_2)$, etc.
$x_3=p_3+$ similar terms in $f(p_3), f'(p_3)$, etc.

But the quantities $p_1, p_2, p_3, \ldots$ must be so near the respective roots that $s_1, s_2, s_3, \ldots$ shall be small; and the ultimate condition of convergence is that the slope of the curve $0=\frac{a}{b^2}-y+cy^2-\text{etc.}$, at its root is certainly greater than -2; as will be shown in the next part of this paper.

All these propositions may have been given entirely in operative notation. They are merely instances of the general theorem that

$$[\psi\phi\psi^{-}]^n=\psi\phi^n\psi^{-}.$$

(5) **Examples.**

Numerical equations may be solved directly from the various

recorded series in the three previous sub-sections, or, if these are not at hand, by the easily remembered process of division itself. The following well-known ones will suffice:

A. $$0 = 5 + 2x - x^3.$$

Newton's famous equation. In this form it is not suitable for obtaining the positive root by simple division. But as the root evidently lies between 2 and 3, transfer the origin by dividing by $0 - 2$ and we have

$$0 = 1 - 10y - 6y^2 - y^3.$$

To reduce the coefficient of the second term to unity we may either divide throughout by 10 (which is a convenient divisor); or may put $y = \frac{1}{10}z$; or may put $y = 10z$ and divide by 100 (sub-section (3)). By the last,

$$0 = \cdot 01 - z - 6z^2 - 10z^3.$$

Then, either by direct division of 0 by $0 + 60^2 + 100^3$, or from the series,

$$z = \cdot 01 - 6(\cdot 0001) + (72 - 10)(\cdot 01)^3 - 5(216 - 60)(\cdot 01)^4 \ldots$$
$$= \cdot 01 - \cdot 0006 + \cdot 000062 - \cdot 0000078 + \ldots;$$

and $x = 2 + 10z = 2\cdot 1,\ 2\cdot 094,\ 2\cdot 09462,\ 2\cdot 094542 \ldots,$

the last convergent being right to five figures, though we have considered only four terms of the invert. The root is $2\cdot 09455148 \ldots$

To investigate the two possible negative roots of the original equation, put $x = -\frac{5}{2}y$ and divide by 5. We get

$$0 = 1 - y + \frac{25}{8}y^3;$$

but as $\frac{25}{8} > \frac{4}{27}$, the roots are not real—sub-section (2).

B. $$0 = 5 - 2x - 3x^2 + x^3.$$

To find the least root of this (negative), divide by $0 + 2$, which gives $0 = 11 - 22y + 9y^2 - y^3$. The root is $x_1 = -1\cdot 3300587 \ldots$ To obtain the second (lesser positive) root, divide this last equation by $0 - 3$, or, which is the same thing, the original equation by $0 - 1$; whence we obtain the transformation $0 = 1 - 5y + y^3$ (see section 2 (8)), which gives the root $x_2 = 1\cdot 2016376 \ldots$ For the third root, divide

the last equation by $\mathbf{o}-2$; which gives the transformation $o = 1 - 7y + 6y^2 + y^3$ and the root $x_3 = 3{\cdot}12842\ldots$

C. $$o = 2 + 10x^4 - x^5.$$

To transfer the origin by dividing by $\mathbf{o} - 10$ would give a long transformed equation; but by putting $x = \frac{1}{y}$, we have $o = 1 - 10y - 2y^5$; which quickly gives the only real root $x = 10{\cdot}00019998\ldots$

D. $$o = 7 - 7x + x^3.$$

Dividing by 7, we see by (2) that, as $\frac{1}{7}x^3$ is only a little less than $\frac{4}{27}x^3$, there must be two real positive roots close together and that the series for the lesser one will be very slowly convergent. In fact we have the series $x = 1 + {\cdot}1428 + {\cdot}0612 + {\cdot}0350 + {\cdot}0229 +$ etc., and are still far from the root. Proceed at first, therefore, by Horner's process, using only operative division, as an exercise, for the successive transfers of origin. For the first step divide $\mathbf{o}^3 - 7\mathbf{o} + 7$ by $\mathbf{o} - 1$, giving $\mathbf{o}^3 + 3\mathbf{o}^2 - 4\mathbf{o} + 1$; and operate with this quotient on $\frac{\mathbf{o}}{10}$ and then multiply throughout by 1000: and proceed as shown below:

$$[10000][\mathbf{o}^3 - 7\mathbf{o} + 7][\mathbf{o} + 1]\frac{\mathbf{o}}{10} = \mathbf{o}^3 + 30\mathbf{o}^2 - 400\mathbf{o} + 1000$$

$$[10000][\mathbf{o}^3 + 30\mathbf{o}^2 - 400\mathbf{o} + 1000][\mathbf{o} + 3]\frac{\mathbf{o}}{10} = \mathbf{o}^3 + 390\mathbf{o}^2 - 19300\mathbf{o} + 97000$$

$$[10000][\mathbf{o}^3 + 390\mathbf{o}^2 - 19300\mathbf{o} + 97000][\mathbf{o} + 5]\frac{\mathbf{o}}{10} = \mathbf{o}^3 + 4050\mathbf{o}^2 - 1532500\mathbf{o} + 10375000$$

$$[10000][\mathbf{o}^3 + 4050\mathbf{o}^2 - 1532500\mathbf{o} + 10375000][\mathbf{o} + 6]\frac{\mathbf{o}}{10} = \mathbf{o}^3 + 40680\mathbf{o}^2 - 148389200\mathbf{o} + 1325416000$$

Here of course operation *upon* a factor such as $\mathbf{o} + 5$ is equivalent to division *by* that factor reversed, that is by $\mathbf{o} - 5$, which $= [\mathbf{o} + 5]^{-1}$. At any step we may arrest Horner's process and return to operative division. We then take the selected quotient, as for instance after division by $\mathbf{o} - 5$, and

(before operating on $\frac{\mathrm{o}}{10}$ and multiplying by 1000) invert it. In this case we invert $\mathrm{o}^3 + 405\mathrm{o}^2 - 15325\mathrm{o} + 10375$. Dividing numerically by 15325, we have, by the series,

Remainder of root $= \cdot 67700 + \cdot 01211 + \cdot 00046 + \cdot 00002 + \text{etc.}, = \cdot 68959$.

Thus ultimately:

$$x = 1 \cdot 3568959 \ldots$$

But the two last figures are too low, as the terms of the invert are all positive. Observe that as the tangential of this function (after division by 15325) is about $-0{\cdot}982$ at its root, $0{\cdot}6896$, the convergence of its invert is quick; while, as the tangential of the original function $1 - \mathrm{o} + \frac{1}{7}\mathrm{o}^3$ was $-0{\cdot}210$ at its root $1{\cdot}356896$, the convergence of its invert was slow—see (2) above.

We gather generally from these examples that simple ascending operative division may give convenient and quick approximations in many cases; but that in other cases, especially where there are equal or nearly equal roots, as in the last example, the approximation may be very slow. In the latter cases, however, it may be quickened by transference of the origin to a point nearer the root. As already mentioned, it is by no means pretended that this form of division is always the best.

E. Show that the coefficients of o in the operative quotient of $\frac{\phi}{\mathrm{o} - h}$ are also given by the successive remainders when ϕ is divided algebraically by $\mathrm{o} - h$ once, twice, thrice, etc.

F. Prove that if

$$[\mathrm{o} + c\mathrm{o}^2 + d\mathrm{o}^3 + \ldots]^{-1} = \mathrm{o} + C\mathrm{o}^2 + D\mathrm{o}^3 + \ldots$$

by simple ascending operative division, then also

$$[\mathrm{o} + C\mathrm{o}^2 + D\mathrm{o}^3 + \ldots]^{-1} = \mathrm{o} + c\mathrm{o}^2 + d\mathrm{o}^3 + \ldots;$$

and $$[\mathrm{o} + c\mathrm{o}^2 + d\mathrm{o}^3 + \ldots][\mathrm{o} + C\mathrm{o}^2 + D\mathrm{o}^3 + \ldots] = \mathrm{o}.$$

G. Prove by simple ascending operative division that $\left[\frac{\mathrm{o}}{1 + \mathrm{o}}\right]^{-1} = \frac{\mathrm{o}}{1 - \mathrm{o}}$.

Divide o operatively by the algebraic quotient of $\frac{\mathrm{o}}{1 + \mathrm{o}}$.

H. Given the series for ϵ^x, find by operative division the series for $\log(1+y)$; and *vice versa*.

Put $\epsilon^x - 1 = y$, so that $x = \log(1+y)$. Then also by operative division,

$$\left[o + \frac{o^2}{2!} + \frac{o^3}{3!} + \ldots\right]^{-1} = o - \frac{o^2}{2} + \frac{o^3}{3} + \ldots;$$

and *vice versa*.

I. Treat similarly the series for $\sin x$ and $\sin^{-1}x$, and other trigonometrical functions.

J. $$\phi(x) \,.\, \epsilon^x = \psi(x),$$

where $\phi(x)$ and $\psi(x)$ are rational integral functions or can be reduced to such. Expand ϵ^x, collect coefficients of the same powers of x, and invert. Show how to render, if possible, the series convergent by suitable transformations.

K. $$o = f(x);$$

if $f(x)$ can be expanded in an ascending series. Expand $f(p+z)$, invert by the method of (4) above, and select p so as to render, if possible, the series convergent.

L. Given the binomial theorem for integral exponents, prove it by means of operative division for fractional exponents.

Let $(1+x)^n = y$, where n is a positive integer; so that $x = y^{\frac{1}{n}} - 1 = \{1+(y-1)\}^{\frac{1}{n}} - 1$. Then

$$1 + nx + \frac{n(n-1)}{2!}x^2 + \ldots = y;$$

$$x + \frac{n-1}{2}x^2 + \frac{n-1}{2}\,\frac{n-2}{3}x^3 + \ldots = \frac{y-1}{n}.$$

Inverting as usual,

$$x = \frac{y-1}{n} - \frac{n-1}{2}\left(\frac{y-1}{n}\right)^2 + \left\{2\left(\frac{n-1}{2}\right)^2 - \frac{n-1}{2}\,\frac{n-2}{3}\right\}\left(\frac{y-1}{n}\right)^3 - \text{etc.}$$

$$= \frac{1}{n}(y-1) + \frac{1}{n}\left(\frac{1}{n}-1\right)\frac{(y-1)^2}{2!} + \frac{1}{n}\left(\frac{1}{n}-1\right)\left(\frac{1}{n}-2\right)\frac{(y-1)^3}{3!} + \text{etc.}$$

$$= \{1+(y-1)\}^{\frac{1}{n}} - 1, \text{ by the binomial theorem.}$$

M. If $\phi = o - co^2 + do^3 - \text{etc.}$, compare the coefficients of the operative quotient of $\frac{o}{\overline{\phi}}$ with those of the algebraic quotient of $\frac{o}{\phi}$, and show that they are composed of precisely the

same combinations of c, d, e, etc., with the same signs, but that these combinations are generally affected by different numbers in the two cases ("Verb Functions," p. 53).

N. In order to facilitate division of ϕ by $o - h$ when h is a decimal fraction, first shift the decimal point suitably both in the dividend ϕ and in the divisor $o - h$, and then shift it backwards in the quotient. For instance (Example D), in order to divide $o^3 - 7o + 7$ by $o - 1{\cdot}35$, carry out the division of $o^3 - 700000 + 7000000$ by $o - 135$, giving the quotient $o^3 - 405o^2 - 15325o + 10375$. Then shifting back the decimal point, we have $o^3 - 4{\cdot}05o^2 - 1{\cdot}5325o + {\cdot}010375$ as the actual result.

Show also that to divide operatively by $o - 1{\cdot}35$ is equivalent to dividing operatively by $o - 1$, $o - 0{\cdot}3$, and $o - 0{\cdot}05$ successively in any order we please.

(6) In subsequent sections I hope to show by the geometrical interpretation of the series that it is the algebraic expression of the arithmetical processes of Dary and Newton (and of a general theorem of my own), and to give tests for convergence; to prove that the series is nothing but the first term of the multinomial theorem of operative volution or iteration; and to examine descending division, "critical points," and the selection of the appropriate "setting" of the original equation for each root in turn. The general series, which contains all the roots together and is the true algebraic invert, will remain to be considered.

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THE INFLUENCE OF RESEARCH ON THE DEVELOPMENT OF THE COAL-TAR DYE INDUSTRY.—Part I

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It is frequently said that the coal-tar dye industry began in 1856 with the discovery of mauve by the late Sir W. H. Perkin.

Nevertheless the real foundations of the industry were laid some thirty years previously in the discovery of benzene by Michael Faraday at the Royal Institution in 1825.

In those days gas-lighting was in its infancy, and gas produced by the distillation of oil was sent round to subscribers compressed in iron cylinders.

From time to time the complaint appears to have been made to the managers of the "Portable Oil Gas Company" that the cylinders, instead of containing pure compressed gas, also contained a quantity of oil, which of course represented a loss from the consumers' point of view, and Michael Faraday was invited to investigate the question.

He examined the oil and found that it consisted largely of a light, mobile, fluid hydrocarbon to which he gave the name of "bicarburet of hydrogen."

This substance was identical with what is now known as benzene, and was thus isolated for the first time by Faraday (the original specimens are now in the Science Museum, South Kensington). Faraday had, of course, little inkling in those days of the importance that would presently attach to the substance as the starting-point of a new industry.

Between 1825 and 1856 a great deal of valuable pioneer work was done upon the investigation of coal-tar, but before considering the matter in detail it is worth noting the position of chemistry, and more particularly of organic chemistry, in those days.

Organic chemistry was little known or appreciated in this country, and the keen spirits who wished to study it had perforce to go to Liebig at Giessen, or to Wöhler at Göttingen.

Prof. Justus von Liebig was already exerting a great influence on the development of organic chemistry both by his fine research work and still more by his lectures both in Germany and in this country, where they were received with great enthusiasm; this was, however, of rather a fleeting character so far as the British were concerned.

In 1840 Liebig published his treatise on *The Chemistry of Agriculture and Physiology*, in which for the first time it was shown how important was an exact knowledge of chemistry for these two subjects, which up to that time had been almost entirely empirical and governed by rule of thumb. It may be remembered that in 1828 Wöhler had succeeded for the first time in preparing from purely inorganic materials a substance—urea—which was previously believed to be capable of production solely by living agencies.

The doctrine of a special "Vital Force" slowly disappeared before the advance of chemical research, and it was gradually realised that many substances previously of purely animal or vegetable origin might in the not very distant future be produced by the chemist in his laboratory. In 1842 Liebig toured through England and delivered a series of lectures upon the importance of chemistry from a national point of view. The immediate result was to make chemistry a popular science and to enforce the need for laboratory work.

As a result a scheme was formulated for the establishment of a National College of Chemistry, where chemistry should be taught and studied for its own sake, and not simply as a subsidiary subject for the training of pharmacists, mining engineers, and others, such as was related, for instance, of the Turk in Playfair's laboratory who wished merely to learn enough chemistry to enable him to expound the doctrines of Paracelsus!

Largely through the efforts of the Prince Consort and of Sir James Clark, the Queen's physician, a fund was opened to provide such an institution, and in 1845 the Royal College of Chemistry arose, which was at first housed in George Street, Hanover Square, and in 1846 moved to new buildings in Oxford Street.

At the request of the trustees, Liebig recommended one of his pupils, A. W. Hofmann, as a suitable candidate for the position of Professor of Chemistry, and in 1845 he came to England to undertake the task of organising the study of chemistry at the College.

If Hofmann had possessed foreknowledge of the importance to which the coal-tar industry was presently to attain, he could scarcely have undertaken research work more directly calculated to lay its foundations.

Whilst at Giessen Hofmann had undertaken, at Liebig's suggestion, an investigation of the composition of coal-tar, and upon his arrival in London he continued the work and put one of his students, Charles Mansfield, to examine the hydrocarbons in tar, which resulted in the isolation of benzene and various other bodies in quantities which showed coal-tar to be a convenient source for such compounds.

At the same time a second student, William Henry Perkin, was set to investigate another tar hydrocarbon, "paranaphthalene" (or, as it is now termed, anthracene), which, though not affording great results at the time, gave Perkin most valuable experience, which he turned to good account a decade later in the synthesis of alizarine.

At the same time Hofmann himself was investigating an oily substance, obtained by the distillation of indigo, to which the name aniline had been given (from the Portuguese "anil" for indigo), and showed that this body could also be obtained from benzene.

Such then was the condition of chemistry shortly before 1856; no one, least of all Perkin or Hofmann, had any idea of the enormous potentialities lying dormant in the black, viscid, evil-smelling tar they were patiently examining, but were content to investigate the substance in a scientific manner in the endeavour to find its composition and to see what fresh facts it could be made to yield up.

The First Period: 1856–1867. Mauve, Magenta, etc.

At that time, in spite of much hard work at the College, Perkin was so enthusiastic in his chemical studies that he arranged a small laboratory in his home in the east of London,

where he could work during the evenings and vacations when the College was closed.

Whilst still researching, under Hofmann, Perkin was struck with the idea of attempting to synthesise quinine from allyl-toluidine by oxidation, since, with the vague views on organic chemistry held in those days, the reaction expressed thus:

$$2(C_{10}H_{13}N) + 3O = C_{20}H_{24}N_2O_2 + H_2O$$

seemed perfectly feasible.

On attempting the oxidation, however, by means of potassium bichromate a dark-coloured precipitate was produced, and in order to examine the reaction under simpler conditions Perkin tried the oxidation of aniline itself with the same oxidising agent and again obtained a dark precipitate insoluble in water, but which dissolved in alcohol to a violet solution which dyed silk a magnificent purple colour. He was at once struck with the vast possibilities opened up by the discovery should the use of the dye prove successful on a technical scale, and sent specimens of the new colouring matter, which he named "aniline purple," to Messrs. Pullars of Perth, and on June 12, 1856, he received a reply: "If your discovery does not make the goods too expensive it is decidedly the most valuable that has come out for a long time. This colour is one which has been very much wanted in all classes of goods, and could not be obtained fast on silks, and only at very great expense on cotton yarns . . . on silk the colour has always been fugitive: it is done with cudbear or archil and then blued to shade."

Perkin finally decided to patent it, and on August 26, 1856, Patent No. 1,984 was issued to him.

Although at this time only eighteen years old, Perkin made up his mind to leave Hofmann and the Royal College of Chemistry, in order to undertake the manufacture of the new dye on a large scale, in spite of Hofmann's endeavours to dissuade him. With the assistance and co-operation of his father and brother, a small factory was started at Greenford Green near Harrow.

It must be remembered that in those days aniline was a fairly new substance, rarely seen outside of one or two research laboratories, and Perkin's first task was to find out the right conditions for obtaining it from benzene.

At first nitrobenzene was prepared by heating benzene and strong nitric acid in glass boltheads, according to a patent originally taken out by Mansfield ; later the method of nitrating by means of a mixture of nitric and sulphuric acid in iron vessels was worked out and has continued with few modifications up to the present day.

The reduction of the nitrobenzene so obtained was carried out by means of iron filings and acetic acid in large iron stills with removable tops, and seems to have been a somewhat exciting process if we are to credit the fact that at first it was customary for a workman to stand by with a water-hose ready to play on the retort if the reaction showed signs of becoming too turbulent !

Later the cheaper hydrochloric acid was used in place of acetic acid. The aniline so produced was then dissolved in dilute sulphuric acid, oxidised with potassium bichromate, the insoluble product filtered off and purified, and was then ready to be sold as " Tyrian purple," or later " Mauvein " or " Mauve," the latter name being given to it by French dyers.

There can be no doubt that in the working out of the technical methods for the preparation of aniline Perkin accomplished a work of even greater importance than the discovery of mauve itself, a fact frequently overlooked, for mauve was destined to have a brilliant but short career, being soon eclipsed by various other new dyes ; but aniline was, and still is, the ultimate foundation of by far the greater number of dyes now in use.

The difficulties of the young discoverer, however, in no way ended with the production of the dye-stuff.

It was still necessary to convince the dyers of the value of the new colours, and we are told that the English dyers did not take kindly to it, and it was, in fact, due to the initiative of the French printers, who quickly saw the possibilities of the new dye, that a demand began to arise for it ; indeed, until French-dyed prints done with mauve began to come into England, the English printers refused to interest themselves in it at all.

The yield of mauve was of necessity small, as is shown by the following table (given by Sir W. H. Perkin in a lecture before the Royal Society of Arts on December 7, 1868) :

	lb.	oz.
Coal	100	0
Coal-tar	10	12
Coal-tar-naphtha	0	8½
Benzol	0	2¾
Aniline	0	2¼
Mauve	0	0¼

but in spite of the small yield, the high price obtainable for the dye fully recompensed for this, as we are informed that the price of mauve in those early days was equal, weight for weight, to that of platinum.

The real " boom " in the popularity of mauve appears to have occurred after it had been on the market for two years, namely in 1859, and *Punch* for that year contains a reference to the craze for the new colour under the heading of " The Mauve Measles " :

" Lovely woman is just now afflicted with a malady which apparently is spreading to so serious an extent that it is high time to consider by what means it may be checked.

" There are many who regard it as of purely English growth, and from the effect which it produces on the mind contend that it must be treated as a mild form of insanity.

" Other learned men, however, including Dr. Punch, are disposed rather to view it as a kind of epidemic, and to ascribe its origin entirely to the French. . . . Dr. Punch is of opinion that it is not so much a mania as a species of measles !

" The main reason which inclines Dr. Punch to this opinion is that one of the first symptoms by which the malady declares itself consists in the eruption of a measly rash of ribbons about the head and neck of the person who has caught it. The eruption, which is of a *mauve* colour, soon spreads, until in many cases the sufferer becomes completely covered with it. . . . Married ladies have been cured by amputation of their pin money, but this is a strong course, and except in extreme cases Dr. Punch would not advise it.

" Dr. Punch therefore prescribes a milder form of treatment and recommends that when the symptoms of the *mauve* measles first show themselves a gentle dose of reasoning should be at once exhibited, with a view of ascertaining if the mind be much affected. . . ."

So successful, indeed, did mauve become that numerous attempts were made to prepare the dye by other methods, none of which, however, were as successful as the original process,

with the possible exception of the method patented by Dale & Caro for the use of copper chloride as an oxidising agent.

In the course of these attempts to circumvent Perkin's patent, various shades of reds and violets were produced, all of which were claimed to be identical with mauve, though there is no doubt they consisted chiefly of other colouring matters, but finally in 1859 M. Verguin of Lyons produced a brilliant new red dye by treating aniline with tin tetrachloride, which he named "fuchsin" (or "magenta," as it came to be known in England), which in some ways seems to have been of even greater interest to dyers than mauve itself, very probably because the latter dye had already done the pioneer work of waking up the printers and dyers to new possibilities in their thousand-year-old industry.

The success of fuchsin was so great that an attempt was made to "corner" its manufacture in France by forming a company known as "La Fuchsine," which had sole rights for its manufacture; as is almost invariably the case, however, when the attempt is made to establish a monopoly in a new industry by preventing competition, the scheme came to grief as newer and better methods were worked out elsewhere for its production, and for some considerable time the failure produced a feeling of distrust amongst investors.

The young industry was not to be held back by any such artificial restrictions as these, and soon other processes were worked out in Germany and in England, in the latter case by Medlock, who used arsenic acid as the oxidising agent, which gave good results but had the disadvantage that the frequent presence of small quantities of arsenic in the crude dye-stuff gave rise to the legend, which persisted for many years, that the aniline dyes were themselves poisonous. The industry was now settling down in earnest and was rapidly attracting to itself the more thoughtful and scientific among chemists and business men.

Heinrich Caro, one of the founders of modern dye chemistry, has well expressed the general state of feeling at the time in these words:

"The beauty, the fastness, and the brilliant success of the first aniline colours acted like sparks on tinder.

"A new world was disclosed full of magic promise, and all

joined eagerly in the search, the manufacturer and the professor, the business man and the adventurer ; for the one a new gold-mine, for the other new opportunities for fruitful investigation."

Prof. Hofmann, who had strongly advised Perkin against the latter's project of manufacturing mauve, was himself caught in the whirlpool of excitement resulting from his pupil's discovery, and very soon prepared a new violet dye—" Hofmann's violet "—by treating magenta with ethyl chloride, the violet dye so resulting becoming a serious competitor with Perkin's mauve, the master thus competing against his own erstwhile pupil !

From thenceforward a continuous and ever-growing stream of new dye-stuffs made its way into the world, such as " aniline blue " or " Bleu de Lyons," discovered by Girard and Delaire in 1861, and the soluble sulphonic acid " Nicholson's blue," prepared from it by Nicholson by the action of concentrated sulphuric acid, Britannia violet, iodine green, and many others. Even at this time in the early days of the industry, two men, Perkin and Hofmann themselves, foresaw clearly that the only way in which the young industry could grow healthily and normally would be by the close co-operation of chemical theory and technical practice.

In particular Hofmann devoted himself to the elucidation of the constitution of the dye-stuffs and his laboratories at the Royal College of Chemistry gradually became the chief centre for the British dye industry—that is to say, for the aniline colour industry of the world.

Perkin also was emphatic in his recognition of the necessity for scientific chemical research, as evinced by his remarks at the end of the lecture cited above :

" The coal-tar industry is entirely the fruit of theoretical chemistry, not studied for the purpose of producing commercial products, but simply for its own sake."

And at the close of another lecture he again emphasises the same fact : " This industry is the fruit of scientific researches in organic chemistry conducted mostly from the scientific point of view ; and while this industry has made such great progress it has, in its turn, acted as the handmaid to chemical science, by placing at the disposal of chemists products which otherwise could not have been obtained."

The wonderfully rapid progress of the industry in its early years may be gauged by the fact that at the International Exhibition of 1862, only six years from the beginning of the industry, the annual production of coal-tar dyes was already valued at £400,000 sterling, and the number of products obtained from coal-tar, given by Mansfield as 13 in 1848, had increased to 40 in 1862.

At the same Exhibition Messrs. Simpson, Maule & Nicholson exhibited a "crown" of magenta crystallised on a wire framework, calculated to be worth £100, whilst the dye remaining in the crystallising vat was valued at £8,000, the market price being about £3 per ounce. Both by nature and by good fortune England seemed destined to be the great coal-tar dye producing country of the world; Great Britain produced more tar than any other country, the first synthetic dye was discovered here, and Professor Hofmann himself at the Royal College of Chemistry was the centre of attraction of the industry; one thing only was lacking, namely a sufficient scientific education of the general public, such as was already obtaining in Germany, and a sympathetic understanding and appreciation of the methods and objects of research. To this last is to be traced directly and indirectly the ultimate collapse of the British dye industry.

In order to appreciate fully the revolution which was inaugurated by the first aniline dyes, it is worth while noting the position of the dyeing industry in those days.

Practically the only dyes obtainable in any quantity were madder, indigo, logwood, fustic, Orleans, cochineal, saflor, catechou, and orseille, together with a few compounds such as Prussian blue, and coloured compounds of iron and chromium and arsenic, also picric acid and murexide to a small extent.

Nearly each dye needed separate and different treatment, so that mixed dyeings were very difficult or impossible, and with the exception of indigo and madder the colours were mostly dull, not very fast, and very expensive.

The appearance of aniline dyes altered everything; dyeing became simpler and cheaper despite the high cost of the dyes, owing to their greater purity and intensity; mixed dyeings were readily obtainable; and the uses and production of all manner of textile goods increased enormously.

Whilst in France the failure of "La Fuchsine" set back

the industry considerably, in Great Britain the manufacture of synthetic dyes was in a state of vigorous and sustained development, and it is therefore hardly surprising that many German chemists came over to this country either to study under Professor Hofmann, or to take up a technical position.

In particular two names must be mentioned: Heinrich Caro, who for some years was engaged at the dye-works of Dale, Roberts & Co., of Manchester, and Peter Griess, who was chemist at the brewery of Messrs. Allsopp & Co., and devoted his spare time to research work which was later almost to revolutionise the dye industry.

During the period 1860 to 1865, therefore, Great Britain was the centre *par excellence* of the dye industry, with Hofmann working chiefly at the purely scientific side of the matter in London, and Caro, Griess, Schunck, and many others at work on the technical side in Manchester. In 1862 Nicholson, also one of Hofmann's pupils, prepared the "soluble blue" named after him; Caro, in the attempt to produce mauve by direct oxidation on the fibre, obtained instead aniline black, and shortly after he prepared induline and nigrosine for the first time, which are still used in some quantity for the manufacture of printing inks.

In 1863 Roberts, Dale & Co., no doubt with the advice of Caro, whom they had persuaded to become a partner in 1860, obtained the services of Dr. Martius, and his first-fruit of discovery in Manchester was the brown dye known as Bismarck- or Manchester-brown, by the action of nitrous acid on metaphenylenediamine, and in the following year Martius yellow or Manchester yellow made its début, having been made simultaneously by Caro and Martius, so that already well over a dozen new dyes were being prepared on a large scale by this date; and at the International Exhibition of 1862 three English firms—Perkin & Sons; Roberts, Dale & Co.; and Simpson, Maule & Nicholson—were exhibiting, besides the French firm of Renard Frères and the German firm of Frank. Hofmann was studying the constitution of rosaniline and "Bleu de Lyons" meanwhile in London, the latter of which he showed to be a phenylated rosaniline, and arising out of this discovery he succeeded in preparing the so-called "Hofmann's violet" already mentioned, leading soon after to the various shades of methyl violet which still form an important item in the dyer's

list; the research is moreover of considerable importance as being the first case where the scientific investigation of a dye has led to the elucidation of its structure and so by logical steps to the preparation of new colours related to it.

Caro was also devoting a portion of his energies towards the solution of the same somewhat academic problem, a fact worth noting as showing "the close co-operation between science and technology which also characterised Caro's later life, and is the chief cause of the progress of chemistry, particularly of coal-tar products, in Germany" (Professor Bernthsen).

In Germany also activity was beginning to be apparent from 1860 and onwards, and various works were started for manufacturing coal-tar dyes: in particular the company "Chemische Fabrik Dyckershoff, Clemm & Co.," the nucleus of the great "Badische Anilin und Soda Fabrik" of to-day, was founded in Mannheim in 1861; Meister, Lucius & Co. began in 1863 with five workmen and one chemist; Kalle & Co., of Biebrich, being also founded in the same year.

In 1863 twenty patents were taken out in Great Britain for synthetic dye-stuffs by British firms (which is incidentally the maximum number taken out by British firms in any year up to date, the next highest number being fifteen patents in 1901!), and by 1865 the British dye industry had reached the zenith of its existence.

Unfortunately, however, the necessity for continuous and careful research work was as little appreciated in England then as it is to-day, and the want of understanding as to the value of scientific research shown both by the subscribers to the Royal College of Chemistry and by the general public, as also the open dissatisfaction expressed by some of the former, seems to have made Hofmann feel that England could no longer afford him a congenial atmosphere in which to carry on his important investigations on coal-tar derivatives, and in 1865 he decided to resign his position as Professor at the College and return to Germany, to the great detriment of the British chemical industry.

"Whatever may be the explanation, it cannot be denied that the higher chemical industries have rapidly retrograded in this country almost from the moment of Hofmann's departure" (Prof. H. Armstrong).

The atmosphere of careful chemical research which characterised the first few years of the industry in England seemed to disappear with the departure of Hofmann, with the exception of Perkin's Greenford Green works, and the scientific method gave way little by little to empiricism and rule of thumb.

However it may be, the fact remains that with the loss of Hofmann the dye industry in Great Britain seemed to lose its nucleus, and by degrees the other German dye chemists left the country and returned to Germany, where they felt that their abilities in research would be recognised and utilised.

In 1866 Caro returned to Germany, and a couple of years later joined the little group of chemists and manufacturers at Mannheim which in 1865 had taken the name of the "Badische Anilin und Soda Fabrik," and was persuaded to become Technical Director.

In 1865 also Professor Kekulé announced his famous "ring theory" of the structure of benzene, which in Caro's words came "like the breaking dawn to lighten the hitherto dark paths of industry, and to assist its progress upon clear lines towards a definite objective. Technical chemistry felt that it must keep abreast with the advance of the science to which it owed its origin and whose leaders were also its own."

From this date onwards it may be said that the manufacture of coal-tar dyes ceased altogether to be a business conducted by rule-of-thumb methods, and became simply an application of scientific chemistry on a large scale.

From 1865 to 1868 there was a period of diminished activity as regards the preparation of new dyes either in England or abroad, attention being chiefly devoted to the development of existing dyes, and in fact the years 1856–1868 form a very definite initial period in the coal-tar industry.

The Second Period: 1868–1884. Synthesis of Alizarine; Beginnings of Azo Dyes

A new era of activity was inaugurated in 1868 by the discovery of Graebe & Liebermann in Berlin that alizarine, the colouring matter of madder, was directly related to the hydrocarbon anthracene, which under the name of "paranaphthalene" had been shown to be present in coal-tar as far back as 1832; and it will also be recalled that Perkin had begun

an investigation on anthracene at the Royal College of Chemistry without, however, achieving any definite results.

Alizarine itself (from alizari, the Levant name for madder) had been obtained from madder-roots by Colin and Robiquet in 1827 by sublimation, and was also separated later by Dr. Schunck by extraction. Madder is, next to indigo, one of the oldest known dyes and is chiefly used for dyeing the fast scarlet shade known as " Turkey red."

The madder industry was one of very large dimensions, some idea of which may be gathered from the fact that in France (the chief producer of the natural dye) 20,466 hectares were planted with madder in 1862; the dye was chiefly exported in two forms, either as crude madder-root, or as the partially purified dye-stuff known as garancine. Great Britain was the chief importer of the dye, the average annual imports from various sources for the period 1859–1868 being 305,850 cwt. of madder, worth 45*s* to 50*s*. per cwt., and 45,560 cwt. of garancine at 150*s*. per cwt., the total annual imports averaging about £1,000,000. In 1868 the world's production of madder was estimated at about 70,000 metric tons and worth £3,000,000 to £3,500,000.

Having regard to the magnitude of the industry, it is hardly surprising that great efforts were made to synthesise the dye from coal-tar. For a long time, owing to a wrong formula having been given to alizarine, it was regarded as a derivative of naphthalene, and Perkin himself undertook research with a view to obtaining the dye from this source, but, of course, without success; alizarine was believed to have the formula $C_{10}H_6O_3$, so that it was assumed to be related to the body known as " chloro-oxynaphthalic acid," $C_{10}H_5ClO_3$ and Martius and Griess, by replacing the chlorine atom by hydrogen, obtained a substance $C_{10}H_6O_3$ which was not identical with alizarine and was assumed to be an isomer.

Graebe and Liebermann, however, showed that by distilling alizarine with zinc-dust the hydrocarbon anthracene was obtained, and the formulæ were therefore given:

$C_{14}H_{10}$	$C_{14}H_8O_2$	$C_{14}H_6O_2(OH)_2$
Anthracene.	Anthraquinone.	Alizarine.

They further showed that the dye-stuff could be obtained conversely by oxidising anthracene to anthraquinone, treating the

latter with bromine, so producing dibromanthraquinone and fusing the latter with alkali to produce alizarine.

$C_{14}H_{10}$	$\rightarrow$	$C_{14}H_8O_2$	$\rightarrow$	$C_{14}H_6O_2Br_2$	$\rightarrow$	$C_{14}H_6O_2(OH)_2$
Anthracene.		Anthraquinone.		Dibromanthraquinone.		Alizarine.

The process was patented in 1868, but owing to the prohibitive cost of bromine was of no commercial value.

The process was nevertheless of the greatest importance as showing for the first time the possibility of synthesing products on a large scale in the chemical factory which had been hitherto solely of natural origin.

To obviate the difficulty with regard to the cost of the bromine, the idea occurred simultaneously to Caro in Germany and to Perkin in England to utilise the cheaper sulphuric acid instead, and to fuse the resultant sulphonic acid with alkali.

But the process was not so simple as it looked, and Caro is said almost to have given up hope of ever making the sulphonic acid until one day, being called from the laboratory for some time and omitting to remove the flame from under the porcelain basin in which the anthraquinone and sulphuric acid were being heated, on his return he found the laboratory filled with fumes of acid, and on examining the basin a distinct pink colour was to be seen on its edges, suggesting to Caro that perhaps the desired reaction had taken place, and that the resultant product had formed the coloured aluminium lake under the influence of the alkali and the alumina of the porcelain.

A little investigation showed that this was so, and further experiments proved that it was necessary to use fuming sulphuric acid for the sulphonation.

Perkin had independently arrived at the same conclusion, and the two patents were issued from the British Patent Office, within twenty-four hours of one another, that of Caro, Graebe, and Liebermann bearing the date June 25, 1869, that of Perkin being June 26, 1869. Perkin at once began the production of synthetic alizarine at the Greenford Green works, partly by the sulphuric acid process, and partly by another method which he had patented, starting from dichlor-anthracene, which avoided the use of fuming sulphuric acid, which in those days had to be imported from Germany and was a relatively expen-

sive item; and in fact it is not too much to say that during the first few years of manufacturing synthetic alizarine it was Perkin's originality in working out the second process that permitted the new branch of the industry to be carried on profitably until methods were found for producing the fuming acid cheaply in this country. In Germany the Badische firm, under the guidance of Caro, proceeded very cautiously for the first few years, devoting most of their energy to a detailed examination of the chemical mechanism of the reaction before setting out to produce the colouring matter on a large scale.

In addition, owing to the vague state of the German Patent Laws in those days, the German patent lapsed through some technical flaw, so that any other firm could take up its manufacture, an invitation that was quickly accepted by several firms, notably the firm of Meister, Lucius & Brüning, so that the Badische Company had from the outset to compete against home as well as foreign competition.

In connection with the Hoechst Company an interesting incident was related regarding the lack of chemical knowledge on the part of English tar-distillers in those days:

In order to obtain the necessary supplies of crude anthracene, the Hoechst Company sent a representative, Herr De Ridder, to England to arrange with tar-distillers about the matter, but apparently no one knew in the least what anthracene was, nor would any of the distillers allow Herr De Ridder to enter their works in order to show them what he wanted, until at last he obtained permission to go over the works of a distiller, John Blott, of Poplar; almost immediately on entering the courtyard De Ridder saw a huge pile of rubbish waiting to be carted away, which he recognised at once as crude anthracene; needless to say, his offer of £20 per ton was accepted at once and in addition he was promised the entire output for the next twelve months at the same price, although, of course, after that date the price increased very considerably.

In spite of the activity abroad, however, little competition was felt in England until 1873 in the alizarine industry, and the Greenford Green works continually increased their annual output as shown by these figures (for convenience the figures for the output of the Badische works, given by Graebe and Liebermann, are also shown):

Year.	B.A.S.F. (tons).	Perkin & Sons (tons).
1869	—	1
1870	—	40
1871	125–150	220
1872	400–500	300
1873	900–1,000	435

Although at first the dyers were somewhat suspicious of the new product, they soon began to use it in conjunction with garancine, and finally used it alone as its obvious advantages over the natural product, both as regards quality and price, became known.

The inevitable result was the gradual collapse of the madder industry, as can be seen from the following table:

Average Annual Imports of Madder and Garancine into the United Kingdom

Year.	Madder (cwt.).	Garancine (cwt.).	Value per cwt. Madder.	Value per cwt. Garancine.
1859–68	305,850	45,560	45*s.*	150*s.*
1875	100,280	24,860	—	—
1876	59,137	15,396	—	—
1877	38,711	8,875	—	—
1878	32,990	2,790	18*s.*	65*s.*

By 1879 the price of Turkey roots had dropped to 11*s.* per cwt., whilst the price of 100 per cent. alizarine dropped continuously:

In	1869 1 kilo	cost	Marks	270
,,	1873	,,	,,	120
,,	1878	,,	,,	23
,,	1902	,,	,,	6·3

In 1874 Perkin decided to retire from the manufacture of dye-stuffs in order to devote himself again to purely scientific work, for which his business left him little leisure.

The works were sold to Messrs. Brooke, Simpson & Spiller, who in their turn later disposed of the factory to Messrs. Burt, Bolton & Haywood, the latter transferring the business from Greenford Green to Victoria Docks, where they ultimately became the present British Alizarine Company.

However much the loss of Sir W. H. Perkin to the British industry may be deplored, there is no doubt that when he retired from active participation he left the industry in a flourishing state and well able to support foreign competition.

One difficulty had, however, shown itself already to him

which is worthy of consideration here, namely that if the British industry was to continue to progress and to hold its own with the German firms there must be a sufficient supply of first-class chemists trained in the methods of organic research.

Unfortunately, however, organic chemistry was not even recognised then as a subject for study at the universities, and all attempts to obtain the sort of men required proved fruitless.

It was not, in fact, until 1874 that the first chair of organic chemistry was installed in Great Britain at Owens College, Manchester. This state of affairs has been considerably changed since then, but even to-day we are assured by Prof. W. H. Perkin, jun.—a son of the discoverer of mauve, and himself one of the most famous of living organic chemists—that organic chemistry does not flourish in our universities to anything like the extent it does at almost every German university and technical school.

Although, then, at the time Sir W. H. Perkin sold his works in Greenford Green the British alizarine industry was well established, the fact that, whilst the German firms were devoting large sums to the investigation of everything even remotely connected with the production of synthetic alizarine, in this country continuous and systematic research work was not undertaken to any extent, soon lost us the lead that had for a second time been given to the British industry by its founder, so that the German firms continuously gained ground on the British concern, and in 1909 Germany was exporting close on 10,000 tons of alizarine and related dyes, covering by far the greater portion of the world's demand.

Although the technical production of alizarine, on account of its magnitude, and the fact that it showed for the first time that the chemist could beat nature at her own game, overshadowed almost everything else for the first decade of its existence, nevertheless considerable advances were being made simultaneously by chemists in other branches of the synthetic dye industry.

In 1871 Professor Adolph von Baeyer produced two new dyes, gallein and coerulein, which soon attained considerable importance on account of their fastness, and a year or two later in 1874 a new dye, eosin, was prepared simultaneously by Caro and Emil Fischer by the action of bromine on fluorescëin (which had been discovered by Baeyer, but was unsuited as a dye).

The process was not patented but was kept a secret, as the German Patent Laws at that time, so far from protecting such chemical discoveries, seemed merely arranged to throw them open to the whole world, as in the case of alizarine; but none the less the composition of the new dye was quickly found out by Dr. Martius (who with P. Mendelssohn-Bartholdy had founded the " Gesellschaft für Anilinfabrikation " in Berlin), and numerous other firms besides the Badische firm began its manufacture.

In 1874 Caro also made another important substance for the first time, nitroso-dimethyl-aniline, which forms the basis for many valuable colours such as toluylene blue, meldola blue, gallocyanine, and so on. The divergence in the methods adopted by the German firms and the British firms also becomes evident about this time. The English firms appeared to be content to go on making use of the original alizarine process, without any attempt being made to carry on researches to investigate the possibility of producing new dyes from alizarine or anthracene.

In Germany, however, unceasing efforts were devoted to research work leading to the manufacture of alizarine orange in 1875 by Caro and the Badische Company on a technical scale, and opening the way for a whole series of new dyes derived from alizarine, such as alizarine blue, discovered by Prudhomme in 1877, alizarine blue, S., discovered by Brunck in 1878, and many others.

In 1876 Caro prepared methylene blue, a very valuable dye for cotton and prints, and also of interest as forming the subject of the first German dye patent under the new Patent Laws which came into force on July 1, 1877.

This same year is a notable one in the history of the synthetic dye industry, since in that year Griess published his researches on diazo-compounds and so inaugurated a period of enormous activity in the synthesis of azo-dyes.

When it is remembered that with the components known to-day several millions of azo-dyes can theoretically be made from them, and that actually many thousands of such bodies have been made to determine whether they are suitable for use as dyes, it will be realised what a momentous event the publication of these researches meant.

Actually the azo-dyes form by far the largest class of dyestuffs known to-day.

One or two azo-dyes such as Bismarck brown & chrysoidin had been prepared before this date, but their constitution was for the most part unknown, and it was not until Griess's investigations cleared matters up that systematic progress began to be made in this direction, showing once again the close and inevitable co-operation between science and technology necessary for the sound development of the industry.

It was at first supposed that azo-dyes could only be obtained in yellows or oranges, but Caro's discovery of "fast red" in 1879 effectually disposed of these imaginary limitations, and from that date onwards every year saw the production of new azo-dyes of all shades and colours; one dye in particular deserves notice, namely Biebrich scarlet, discovered by Nietzki in 1879 as being the first representative of the sub-class known as disazo dyes, which have since grown to be of very great importance.

To Caro we are also indebted for the process of sulphonating various dyes, such as magenta and so on, thus obtaining the class of acid dyes, such as "acid magenta," which are in many respects faster than the original dyes, and have an extensive use in wool-dyeing.

In 1878 malachite green, which holds an almost unique position in the dyeing world on account of its clear green tone, was prepared on a technical scale, having been discovered by Otto Fischer in 1877 under the name of "Phthalgrün," and it is characteristic of the scientific way in which the German dye industry was managed that when Fischer first announced his discovery of "Phthalgrün" from benzaldehyde and dimethylaniline it was assumed to be of little more than academic interest, since benzaldehyde was a fairly expensive reagent obtainable only in relatively small quantities. Nevertheless by May 1878 the Badische Company, under the skilful and energetic guidance of Caro, had succeeded in overcoming most of the difficulties and began to produce the dye on a commercial scale. Professor Bernthsen, therefore (who mentioned the matter in a memorial address to the German Chemical Society on Heinrich Caro), may be pardoned for his proud boast, "Wenn die deutsche Technik erst einmal Bedarf für eine Produkt hat, so weiss sie auch alle Hindernisse für seine technische Darstellung zu überwinden."

ON INSTRUMENTAL AIDS FOR DEAFNESS

By PROF. F. WOMACK, B.Sc., M.B.

THE question has often been propounded—why is it apparently not possible to remedy defects of hearing, especially such as are concurrent with advance of years, with the same satisfactoriness that defects of vision can be remedied by the use of spectacles ? In answering this question, we shall be better able to understand the difficulty to be surmounted if we consider the difference of the two scientific problems.

Most of the difficulties or defects of vision are due, not to lack of sensitiveness of the retina, but to limited range of focussing power ; and all that is necessary is to so deflect the path of the light by refraction as to cause the rays to diverge from some point within the restricted range of focussing. Had the defect been due to lack of sensitiveness of the retina, the problem would be as difficult of solution as that relating to hearing ; in fact, the former would be virtually unsolvable, since the brightness of every image of finite magnitude is diminished by every optical contrivance employing reflection or refraction. The problem in the case of hearing is, how to increase the energy density of the sound waves reaching the observer—how to concentrate the vibrational energy of the wave motion, that the number of ergs per second per square centimetre may be sufficiently increased to reach the minimum necessary for audition. The energy density necessary for hearing by a normal person is surprisingly small. Dr. P. E. Shaw measured the amplitude of movement of the diaphragm of a telephone when this was only just sufficient to produce an audible sound, and found that this minimum displacement was of the order $0{\cdot}7 \times 10^{-7}$ (seven hundred-millionths of a centimetre, or three hundred-millionths of an inch). The air immediately adjacent to the telephone diaphragm would however not move backwards and forwards even to this extent, but as estimated by Lord Rayleigh would be approximately five times less, or $0{\cdot}15 \times 10^{-7}$ cm.

Lord Rayleigh has also investigated this same problem by two entirely different methods; one depending on the blowing of a whistle by a stream of air working at known power, the observer going so far away that the whistle is only just audible, and the second depending on the estimate of the rate of output of sound energy by a tuning-fork at the instant at which it ceases to be audible though still vibrating. The former method gave $0{\cdot}8 \times 10^{-7}$ cm., and the latter $1{\cdot}2 \times 10^{-7}$ cm. as the minimum amplitude of vibration of the air for audibility. The figures of Rayleigh are several times larger than those of Shaw, but taking the higher mean figure, say 10^{-7} of Rayleigh, this corresponds to an energy density of only 0·000043 ergs per sec. per sq. cm., or $1{\cdot}3 \times 10^{-9}$ ergs per c.cm. If we might neglect the existence of bone conduction of sound through the skull, and if we take the opening of the external meatus of the ear as about 1 sq. cm., then provided vibrational energy reach the observer at the rate of 0·000043 ergs per sec., or say 6×10^{-15} of a horse power, the sound will be audible to a person of normal acuity of hearing. For sounds used in ordinary conversation this figure would have to be increased some ten times.

It is, however, a question not merely of collecting enough vibrational energy and conveying this to the ear of the person with defective hearing, but it is just as essential that the *quality* or *timbre* of the sound shall not be so far altered as to render the sounds unintelligible. Now in ordinary speech the consonants merely serve as momentary interruptions between vowel sounds, the interruption being produced at lips, teeth, or back of hard palate, the timbre of the sound is that given by the succession of vowel sounds.

Much research has been expended in the study of the conditions for vowel-production, pioneer work being done by Willis and by Wheatstone. It appears from the present state of our knowledge that, to produce a vowel sound of given character, it is necessary that the sound shall contain from six to eight separate partial tones, each of definite pitch, irrespective of the fundamental tone to which the vibration of the vocal chords give rise. These partial tones are widely separated in pitch, and do not form a harmonic series. If, then, in any piece of apparatus designed to act by the principle of resonance it is possible for these partial tones *and no others* to be reinforced, there is reason to hope that such a piece of apparatus may

leave the timbre of the vowel sound not materially affected, and that it may therefore fulfil the second desideratum. This conclusion is arrived at by the study of the forms of manometric flames, and by sections of the depth of the indentations in the plate of a gramophone, and is borne out by the observation that the vowel sound emitted by a gramophone is entirely altered if the disc is run at a different speed.

It is, however, only right to say that there is an alternative theory of vowel quality, according to which a given vowel requires the existence of partials of a prescribed *order* to be present, and not partials of fixed and prescribed pitch. To what extent this latter theory may be true (and according to Auerbach both characteristics must be fulfilled) it is apparently impossible that any resonator of fixed shape can satisfy the conditions demanded by this second theory.

In short, both quantity and quality of the sound heard must be taken into account in considering the efficacy of any instrument for hearing.

For purposes of convenience the existing types of mechanical aids to hearing may be attemptedly classified, so far as there is any principle at all underlying their construction, under the following headings :—

A. Simple transmission tubes.

B. Cone-shaped or tapering reflectors. When short these are called by various names, such as trumpets, cornets, auricles, etc.

C. "Trumpets," *i.e.* a conical tube like the brass trumpet used on the Continent by the station master in starting a train, but with the narrow end usually doubled three times on itself.

D. "Resonators," so called—bell-shaped, with a bent tapering tube attached at one point in the side.

E. Instruments in which the idea of "bone conduction" is aimed at.

F. Table instruments, in which no definite principle is aimed at, such as resonance, reflection, or conduction through a solid.

G. Telephonic instruments.

Class A is familiar to every one in the form of the speaking-tube used from one room to another in a house. Provided that certain conditions are fulfilled, and especially that sharp angular bends are avoided, such simple instruments are of

service for the purpose of ordinary conversation between two persons. The physical principle is merely that of continued reflection of the sound waves from the inner wall of the tube, with consequent prevention of spreading out of the wave front and diminution of the energy density with increase of distance. The law of reflection is immaterial, and the efficiency is but little affected by the bore of the tube, which may be attributed to conduction of sound also through the material of the tube (as in a wooden stethoscope) playing some secondary part in the process.

Before considering the next classes, B, C, D, it may be as well to refer to the underlying acoustic principles, viz. reflection of sound waves and resonance.

All reflection of wave motion by a surface to an observer is fundamentally *diffuse* reflection ; that is, however large or whatever the shape of the reflecting surface may be, every part contributes something to the energy reaching the observer. When we are dealing, however, with short waves, such as those of light, the outlying portions of the surface reflect waves that are mutually destructive, and the effective portion of the surface is so small that the energy is reflected strictly in accordance with the familiar law that the angles of incidence and reflection are equal. But with long waves, such as we have to deal with in sound, the effective part of the surface is not small, and all of the surface with which we have to deal contributes something to the sound ultimately heard. This is well illustrated in the case of reflection of sound by the wall of a house or the face of a cliff, or by the "sounding-board" suspended over a pulpit, or by the difficulty of determining whether a vehicle is coming from right or left along a street when the observer is in a side-street. The degree to which the problem is affected by the length of the waves, or, what is essentially the same thing, the pitch of the note, may be well illustrated by listening to the shrill ticking of a watch held in the hand, and noticing that if a postcard or book is held a few inches from the ear the sound of the ticking is no longer heard. But if the lower pitched ticking of a pendulum clock is employed the introduction of the book hardly reduces the loudness.

In class B, which may be suggested by fig. 1, what little efficiency these instruments possess, and for the most part this is very little indeed, depends solely upon reflection ; the source

of sound, the speaker, being far away from the wide end of the cone. The inefficiency is due to the fact that the area of the reflecting surface is too small a fraction of the "effective" area appropriate to the long waves employed in speech. It is

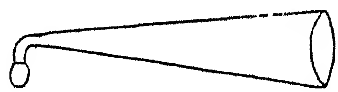


FIG. 1.

as if one were to endeavour to reflect waves on the sea by holding a small flat surface like a spade for them to impinge against.

If one realises that the long waves of sound are diffusely reflected, and not reflected in accordance with the law of equality of the angles of incidence and reflection, one will understand the uselessness of all instruments that pretend to

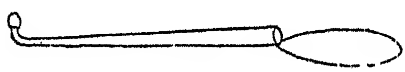


FIG. 2.

"focus" the sound waves to a point and then convey them by a tube to the observers' ear. See fig. 2.

In Class C, usually known as trumpets, and illustrated in general design by fig. 3, reflection, ineffective as it must necessarily be because of the smallness of the whole reflecting surface, is reduced to nil; and what little efficiency they possess can

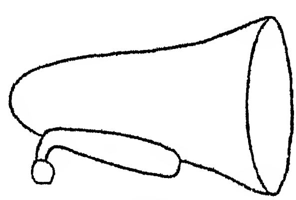


FIG. 3.

only be due to resonance. Now if one were to reverse the action of the instrument, and, unscrewing the nipple end, use the instrument with a suitable mouthpiece as a producer of sound, *e.g.* as a bugle, it would be realised that there is a very limited series of tones that it will emit; and similarly there is a very limited series of tones to which it will respond. The

pitch of the lowest tone to which it resounds depends on its total length, and to some extent on the diameter of its trumpeted end, but it will be generally found that the fundamental tone of the instrument is considerably higher than that employed in speech. Apart from the natural raising and lowering of pitch during speech, a man's voice employs notes of about $e' = 160$ vibrations per second as a mean, and the resonator for this would require to be about 42 inches long, though a shorter instrument would suffice for listening to a woman's voice. Moreover, as shown by Helmholtz and others, for the production of a vowel quality of tone, several partial tones are necessary, and the lowest of these is for most vowels much below the fundamental tone of the instruments used in deafness. Let one take, for example, one of the large shells of the type formerly used, on account of its pearl-like colours, for decoration, and

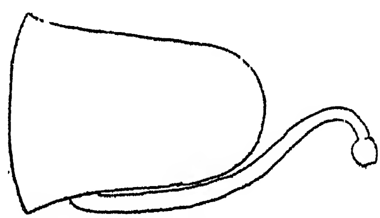


FIG. 4.

hold it to the ear. It will respond to a very limited series of tones, the lowest of which will probably be about $b' = 490$ or $c'' = 523$, and the next an octave higher. These may be easily found by striking the notes of a pianoforte while holding the shell to the ear.

The fundamental defect therefore of Class C of instruments is that they reinforce only a few notes, and these of too high a pitch, and consequently the quality of the sound is materially altered; vowels—on which the character of speech almost solely depends—being entirely altered in timbre.

Class D, see fig. 4, usually called resonators, are generally of the shape of a small bell, with one narrow, bent, and tapering tube leading from near the mouth of the bell to the external meatus of the ear. The inefficiency of these is not reduced by the addition of a more or less ornamental grille blocking up the mouth of the bell, although it is sometimes claimed that the grille has a "useful and necessary acoustical effect."

The writer of this article has recently examined nearly eighty instruments of different types, testing them not merely subjectively, but by two physical methods of extreme delicacy, the instrument being connected to an extremely sensitive manometric flame, and also to a drum membrane pendulum detector. Nearly all the instruments of classes B, C, and D were found to be virtually useless; it was only in class F that some slight improvement was obtained.

Class F constructionally does not differ from D. See fig. 5. There is usually an air cavity A, short and wide, and resounding especially to one note of fixed but generally high pitch, connected by a flexible tube B to a monaural or binaural ear-piece at the further end. The metal portion A is intended to rest on a table. This class of instruments depends principally on solid conduction, and requires that the table on which the

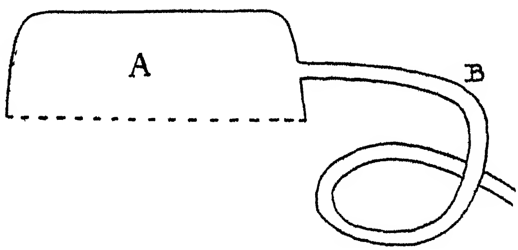


FIG. 5.

receiver is placed should be of wood, not covered by a cloth. So far as resonance of the air chamber of the receiver plays any part in the mechanism, the criticism urged against the preceding classes applies, viz. that the resonant air-chamber is too small, and that its fundamental tone lies above the tones usually employed in speech.

On account of the complex shape the fundamental tone of the air-chamber cannot be computed, though it may easily be found experimentally; but that the dimension is in general too small may be judged from this, that to resound to the note $g' = 192$, a spherical König receiver would have to be 15 cm. diameter, or nearly 2 litres capacity.

With regard to class G, we have here a class of instrument dependent on the principle of the microphone, in which the energy of the incident waves may, by supplementing it by the energy of an electric battery, be increased any desired number

of times, and the sound correspondingly intensified. The instrument is, in fact, a portable telephone-receiver and transmitter. The possibility of doing this completely satisfactorily depends on the facts (1) that the diaphragm of the telephone receiver being of fairly thin sheet-iron, and of considerable diameter, has an intrinsically low-pitched fundamental, (2) that the partial tones it possesses are almost unlimited in number, and do not form a " series " 1, 2, 3, 4, etc., or 1, 3, 5, 7, etc., with wide intervals of pitch as in ordinarily vibrating bodies ; but that the higher members of the group have tones lying so close together that the limited number of partial tones in the voice sounds are pretty certain to have their counterparts in those of the telephone diaphragm. There is a certain alteration of the timbre, in part due to the representation of these partial tones not being complete, but probably principally because the changes of resistance of the microphone receiver are not proportionate to the movement of its diaphragm, and as in the telephone receiver the consonants are imperfectly produced. Much improvement in this latter respect is doubtless possible by the employment of some of those principles used in connection with wireless telephony, and in fact much more rapid improvement in various forms of apparatus would be feasible if the persons who devise them had a modicum of knowledge of physics, instead of being guided by pure empiricism.

In brief, of the mechanical aids so far devised, classes B, C, D, and E are of little efficiency, A is serviceable with the limitation that it can only be used for conversation between two persons, while class F is the most efficient.

ESSAY-REVIEWS

HUMAN PALÆONTOLOGY, by A. G. Thacker, A.R.C.Sc.: on

(1) **Prehistoric Times**, by the late Rt. Hon. Lord Avebury. Seventh Edition. [Pp. 633, with 284 illustrations.] (London: Williams & Norgate, 1913, price 10*s*. 6*d*. net.)

(2) **Ancient Hunters**, and their Modern Representatives, by W. J. Sollas, F.R.S. Second Edition. [Pp. xxiv+591, with 2 plates and 314 text-figures.] (London: Macmillan & Co, 1915, price 15*s*. net.]

The last fifteen years have seen no palæontological discoveries comparable in importance to the startling advance in biological thought due to the rediscovery of Mendel's work and the publication of De Vries' researches. Yet palæontology has made much progress, and in no branch of the science have investigators been more active than in that which deals with human fossils. Lord Avebury's work was republished at an unfortunate time. The Piltdown discovery, one of the half-dozen most important finds of fossil human remains, was not made known to the world until December 1912, and the veteran scholar had no time to incorporate more than a brief reference to the subject. Thus the meaning of the Sussex skull, and certain other very new problems, are not discussed in *Prehistoric Times*, although in so far as most of the recent information upon the older questions is given, the publishers claim that the book is up-to-date is justified. There are, however, a few survivals of Nineteenth-Century mistakes. The old confusion between the auroch and the bison is perpetuated, the Latin names of various mammals (which it has been necessary to change owing to the law of priority) have not been revised, and there is a surprising error with regard to the Javan ape-man, who is described as being either a large gibbon or a "very small man." The femur is of course our only index to the size of the Javan creature, and judging from that he was fully up to the average human height. Moreover, Lord Avebury was not able to correct the proofs himself, and unfortunately they were carelessly revised; but in spite of these blemishes, it is hardly necessary to say that

FIG. 1.—Fossil leaf of *Rhododendron ponticum* from the Hötting breccia.

(After v. Wettstein from Sollas. Reproduced by kind permission.)

FIG. 2.—A flowering branch of the existing *Rhododendron ponticum* from the Caucasus.

(From Sollas. Reproduced by kind permission.)

the work of the great pioneer to whom the very words palæolithic and neolithic are due demands a place in every library.

It would be difficult to imagine a book more completely modern than the new edition of Prof. Sollas's *Ancient Hunters*. Here the student may find the results of the most recent research and a full discussion of the questions which are still a matter of controversy. Investigation has proceeded in several different directions. In the first place, our knowledge of the kind of environment in which the early men of Europe lived has been enlarged. It is not too much to say that the existence of four distinct glacial epochs in the Pleistocene, with of course three interglacial epochs, is now proved, and Prof. Sollas lends no support to the perverse scepticism with which the reality of interglacial periods is still regarded in some quarters. The interglacial flora is even more convincing than the fauna, although monkeys and hippopotami are significant enough. A charming piece of evidence is that of the Pontic Alpine Rose, *Rhododendron ponticum*. At Hötting, near Innsbruck, there is a famous breccia that lies between two boulder clays, which Prof. Sollas believes represent the third and fourth glacial epochs respectively. In this Hötting breccia fossil remains of *R. ponticum* have been found, and as the plant lives at the present day in the Caucasus and other regions with a considerably warmer climate than that of Innsbruck, the fossils give us a clear indication of interglacial conditions. Sollas, following Prof. Penck of Berlin, concludes that the snow-line around Innsbruck in the days of the Hötting breccia stood no less than one thousand feet above its existing level. Of course, the evidence of this rhododendron, illustrations of which are reproduced here, does not stand alone, but is corroborated by many other proofs of an interglacial flora.

Another much-debated problem is the antiquity of worked flints, and here Sollas is a safe and an expert guide. Pliocene and even Miocene eoliths, as well as the Pliocene East Anglian "rostro-carinates" of Reid Moir, were accepted by Lord Avebury, but I do not think many people will have much confidence in any of these alleged artifacts after reading Sollas's criticism of the specimens. The Piltdown discovery is not discussed in *Ancient Hunters* as fully as one would have hoped, but the author agrees with Smith Woodward in regarding the skull as Pleistocene, not Pliocene.

There are other advances in prehistory which might be discussed at almost any length: the minute characteristics of the different types of Paleolithic implements—Prof. Sollas's description of the implements of the Aurignacian Age is especially exhaustive—the exact division of the Paleolithic into subordinate ages, and the vexed question of the correlation of those subordinate ages with the glacial and interglacial epochs: but more interesting still is the light thrown on the nature of the early men themselves. What has palæontology to say about the descent of man? This question is even more pertinent to-day than it was fifteen years ago, for the progress of biology has of course thrown much doubt upon the whole hypothesis of gradual evolution as advocated by Darwin. There are now some reasons for thinking that evolution takes place by saltation, or "mutation," not by the imperceptible merging of one species into another species. Palæontology ought to have much to say in this debate, and it is strange to find that Lord Avebury, writing as recently as 1912, should discuss the supposed linking-up of species without so much as a reference to Mendel, to De Vries, or to Bateson. Even Sollas, thinking no doubt that the subject would lead him too far from his main thesis, does no more than hint that the problem exists.

It has often been said that the weakest side of the case for organic evolution is in geology. I do not think that there has ever been very much force in that criticism; on the contrary, geological discovery has probably given evolution more support than the earlier evolutionists would have dared to hope. But when the criticism is directed not against organic evolution in general, but against the particular hypothesis of gradual evolution, it certainly has point. There are cases in which fossil species do appear to merge, but the phenomenon is not to be seen on the great scale that one would have expected if the Darwinian hypothesis were true. The cases of apparent merging present a difficulty to the Mendelians, but the latter are able to explain the instances of variable species among living animals, and it must be remembered that mutations are not necessarily large "sports." Of course many of the gaps between species are too large to have been filled by a single mutation, but in general the constancy of species in time and the very wide-spread phenomenon of gaps are more readily explicable on the

Mendelo-mutationist theory than by Darwinism. When the origin of a species, perhaps known from many hundreds of individual fossils, cannot be traced, the classic Darwinian explanation is that it was evolved somewhere else, or that the particular stratigraphical zone, or fraction of a zone, in which it was evolved has been washed away. And, of course, it is true that negative evidence is most inconclusive in geology; but negative evidence in this instance is cumulative, and since it is not a few species of animals that must have been evolved "somewhere else," but thousands of species, one begins to suspect that the theory requires serious modification. No palæontologist would be so rash as to dogmatise on such a difficult subject, but the study of fossil species from this point of view might well be pursued further than it has been hitherto.

And now if we confine our attention to the narrow department of human palæontology, we find the same phenomena; namely, constancy of species in time, and gaps between species. The facts relating to fossil men are regrettably few, but they are interesting, and the discoveries fall at once into two groups. *Homo sapiens*, real man, is known fossil; that is one class of evidence. The other class of evidence is the occurrence of fossils of creatures who, although members of the human tribe, are not placed in the species *Homo sapiens*. This multiple character of the evidence is masked by the continued use of the word Paleolithic, and still more by the mischievous expression "Paleolithic man." The term Paleolithic was appropriate enough forty years ago, when we knew only of a vague "Older Stone Age" more ancient than the Neolithic Period, but it merely serves now to cover up the fact that the pre-Neolithic Hominidæ are known to be extremely heterogeneous. Hence I proposed in Science Progress for October, 1913, that the Late Paleolithic Period, that in which *Homo sapiens* is found, should be renamed the Deutolithic Period, and that the Early Paleolithic Period, the strata of which contain remains of the extinct species of the Hominidæ, should be styled the Protolithic. These names Protolithic and Deutolithic are formed on analogy with the terms Protozoic and Deutozoic, which signify of course the two great divisions of the Paleozoic era.

The Deutolithic strata contain, therefore, relics of our own species only, but the remains of several different races have been

found. One type, the so-called Grimaldi Race, is nearly related to the Bushmen of South Africa, and another, the Chancelade Race, is certainly identical with, and probably directly ancestral to, the modern Eskimo. Yet another type is the famous Cro-Magnon Race, a splendid people of great stature, who have no very close kindred among living nations. Now the oldest division of the Deutolithic is called the Aurignacian, and both the Cro-Magnon and the Grimaldi Races are found in the Aurignacian strata. It is worth noting that the chief distinction between the Grimaldi people and the modern Bushmen is that the former had considerably larger brains. The Bushmen no doubt give us, however, a fairly correct idea of the kind of life led by the Grimaldi Race, and Sollas describes those interesting South African savages, now almost extinct (it is stated by W. H. Tooke that "the extermination of the Bushmen was for a long time regarded by the Cape Government as a matter of State policy"), in considerable detail, whilst full descriptions are likewise given of the Tasmanians, Australians, and Eskimos. Sollas is not afraid to date the Aurignacian in years. He believes that this age began shortly after the beginning of the recession of the ice of the Fourth Glacial Epoch, and he thinks that recession began 17,000 years ago, although he is careful to point out that one factor in this computation is merely a guess. This would place the beginning of the Aurignacian at *circa* 13,000 B.C. There is considerable cogency in the arguments for Sollas's dating of the Fourth Ice Age, but whether it is correct to assign the Aurignacian to this point in the Pleistocene time-table is, I think, more doubtful. Prof. Penck and the late Prof. James Geikie placed the Aurignacian at the end of the Third Interglacial Epoch, and there is much to be said for this latter view. For instance, Penck's scheme accounts satisfactorily for the fact that the fauna of the Magdalenian (the third division of the Deutolithic) indicates a decidedly colder climate than that of the Aurignacian, whereas Sollas is obliged to explain this phenomenon by a slight temporary re-advance of the retreating ice. But be this as it may, and whatever be the date of the Aurignacian in years, there is this to be noted about Aurignacian man: he is fully human. He is not intermediate between *Homo sapiens* and any other species. Not only so, but he is already divided up at this his first appearance into several distinct races.

Of the four extinct species, three—the Piltdown, Heidelberg,

and Javan species—are known only from small fragments of the skeleton of one individual of each type. The skeleton of the fourth species, the Neandertalers, is almost completely known from the remains of numerous individuals. These Neandertalers are most puzzling beings. They had receding foreheads with monstrous brow-ridges, and they walked with a permanent crouch, and yet they had big brains and were skilled workers in stone. The anatomy of Australians shows certain slightly neandertaloid features, and Sollas appears to think that this fact may have some significance. But the resemblances are very slight, and may well be purely fortuitous.

Thus as we trace *Homo sapiens* backwards we do not find him gradually merging into a different animal. On the contrary, we find some glorified Bushmen and another race which is one of the finest that the world has ever known. The Darwinians may or may not be right in thinking that man as we know him arose by imperceptibly slow progress from some sub-human creature; the indirect proofs of this theory may or may not be forcible; but of direct palæontological evidence of such an origin there is none.

THE FATHER OF MODERN SCIENCE, by H. G. Plimmer, F.R.S.: on **Roger Bacon.** Essays contributed by various writers on the occasion of the commemoration of the seventh centenary of his birth. Collected and edited by A. G. Little. [Pp. viii + 426.] (Oxford: at the Clarendon Press, 1914, price 16*s*. net.)

On the 10th of June 1914, the seventh centenary of Roger Bacon's birth was celebrated at Oxford. This commemoration seems to have been only partially successful as a tribute to the neglected memory of one of Oxford's very greatest sons; for, although a statue of Bacon was unveiled in the University Museum and the volume named at the head of this paper was published, the fund intended to be used for the publication of Bacon's works was not sufficient for the purpose, and more than half of the works of this great prophet of Science remain to-day unpublished.

These commemoration essays form probably the last international book that will be printed for some years, for its pages are written in English, French, and German by such scholars as Ludwig Baur, François Picavet, Cardinal Gasquet, and Sir John Sandys. The essays are of course unequal and there seems to be but little enthusiasm of the contagious kind, except

in Picavet's charming essay on *La place de Roger Bacon parmi les philosophes du XIII^e siècle.* The book attempts to give an account of Roger Bacon's manifold activities by people who are able to judge and to criticise the various departments of his work. As far as it goes, it is well done, but it would take several such volumes to touch the whole gamut of Bacon's work.

Carlyle, who was rarely generous to science and would know nothing of it, said that in this thirteenth century Roger Bacon and Albertus Magnus were "cheering appearances"; he quaintly and truly suggests that they were not blind to Nature's greatness, but had no poetic reverence of her, so that they ventured fearlessly into her recesses and extorted from her many a secret. There is no doubt that Roger Bacon's unfolding of physical science was the first of that long series of victories which will make man more and more Nature's king, more and more "master of things." Bacon is the prophet and one of the patriarchs of modern science: he is of the race of Galileo and of Newton, and Science owes a much larger debt to this poor brother of the Order of St. Francis than most of its votaries dream of. It seems not a little ironical that a Franciscan monk should have given the most powerful impulse to natural science which it has ever received.

"Friar Roger Bacon of the English nation and the county of Dorset," as John Rous called him, is said to have been born in 1214, probably at Ilchester, near the county of Dorset. This was an era of the happiest augury for such a spirit as his. The Magna Charta, which was the foundation of our national freedom, was signed before he was a year old, and his work laid the foundation of our intellectual freedom. The breath of such a time could not fail to penetrate even college and cloister walls. The main events of his life are few. He studied at Oxford, lived there, and later, before 1245, went to Paris, where he saw Alexander of Hales (the irrefragable doctor) and heard William of Auvergne and John de Garlandia. He joined the Franciscan Order, but where, or in what year, is not known. In 1266 Pope Clement IV., who had become interested in Bacon's earlier work whilst Archbishop of Narbonne, requested him to send copies of his works to Rome. Bacon felt that his great opportunity had come, and eventually sent to the Pope the work which appeared later as the *Opus Majus.* He appears to have returned to

Oxford in 1268, and to have continued writing. In 1277 Jerome of Ascoli, the Minister-General of the Friars-Minor, "condemned and reprobated the teaching of Friar Roger Bacon of England." As a result of this Bacon was sent to prison in Paris, where he remained incarcerated until 1292. In this year it is believed that he returned to England, and shortly after he died, it is said in the same year, and "no man knoweth of his sepulchre unto this day." This scant abstract of his life-events is in accordance with Mr. A. G. Little's very careful account in the introductory essay. Other authors give more, according to their fancy, but this appears to be all there is positive evidence for.

Diderot, who was a good judge, described Roger Bacon as "one of the most surprising geniuses that nature had ever produced, and one of the most unfortunate of men." There is no doubt of his genius; he was unfortunate in that his work and his ideas were far in advance of his age; his works were damned and destroyed by Rome, and real, earnest research into Nature was held up for centuries, and then had to be fought for step by step; he left no disciple, and seven centuries have not been sufficient time wherein to publish his works.

To give any account of his writings in such a notice as this would be impossible. Mr. A. G. Little, the editor of the commemoration volume, quotes Leland's saying that "it is easier to collect the leaves of the Sibyl than the titles of the works written by Roger Bacon," and his admirable bibliography of Bacon's works extends over forty-four pages. The principal and larger works are the *Opus Majus*, *Opus Minus*, and *Opus Tertium*, the two latter being of the nature of an introduction and an appendix to the main work. The ground these works cover is enormous. He begins with an exposition of the four general causes of human ignorance, and goes on to theology, the study of languages, mathematics, optics, experimental science, moral philosophy, and speculative and practical alchemy: in fact, almost every branch of knowledge is dealt with in an entirely original manner. His other works comprise treatises on the calendar, on astronomy, comets, alchemy, astrology, mirrors, metals, minerals, geography, Greek and Hebrew grammar, physics, medicine, old age and methods of its retardation, and gunpowder. Well may Whewell say, speaking of the *Opus Majus*, that Bacon's works were "at once the *Encyclopædia* and

the *Novum Organum* of that age." They formed not only a summary of existing knowledge, but they pointed out and defined the only right method of acquiring natural knowledge, by observation and experiment.

The first mention of these works in literature is by Pico della Mirandola, the friend of Lorenzo dei Medici; and a list of the works is given in Bale's *Illustrium Majoris Britanniæ Scriptorum* in 1548. Curiously, Dante does not mention Bacon, although it is most probable that he was not ignorant of his works. Dr. Liddon suggested many years ago that Dante might have had the same idea of Britain that the Jews had of Galilee, so that he could not conceive the possibility of an English Franciscan being comparable to one of Latin blood. In English literature Bacon appears as a necromancer from Gavin Douglas, through Robert Green, to Byron. He was, however, noticed by a few workers during the centuries. The second President of the Royal Society (Sir Joseph Williamson) copied out some of his treatises with his own hand; Goethe, when preparing material for the *Farbenlehre*, studied what was available of Bacon's works and wrote very appreciatively of him; Humboldt also knew something of him, although he wrongly attributes to *Francis* Bacon, instead of to *Roger*, the discovery that light has an appreciable velocity.

The three principal works—the *Opus Majus*, the *Opus Minus*, and the *Opus Tertium*—contain a mass of discoveries, of demonstrations and of propositions which he had made in the various sciences. He had visions of telescopes, of microscopes, of steam power for land and sea, and of flying machines; he knew a great deal about lenses and mirrors, and concerning the refraction of light; he argued that the world was round; he discussed the nature of fire, the volume of the sun and moon, the centre of gravity of bodies, and sidereal light; he suggested the reform of the calendar (not realised until the sixteenth century); he suspected the compass in studying the magnet, and also the diving-bell in treating of density of bodies, and of the elasticity of air. The essays in the commemoration volume by Prof. D. E Smith, by Prof. Wiedemann, by Dr. Vogl, by Dr. Würschmidt, and by Mr. Pattison Muir give an admirable account of his work in these directions. He declaimed energetically against routine, and against the acceptance of opinions without examination and without proof.

In these writings lie the first ideas, the germs and the principles of most of the discoveries of our age, so that Bacon may justly be considered as the father of modern science and as the begetter of experimental science. Some of his ideas bore very practical fruit in the time near to him: for instance, Columbus says, in a letter to Ferdinand and Isabella, that among the things which determined him to start on his great voyage was that portion of the *Opus Majus* which was incorporated without acknowledgment by Petrus Alliacus in his *Imago Mundi*, in which Bacon asserts the possibility of going westward from Spain to India. Humboldt says that this had more to do with the discovery of America than the advice and teaching of the astronomer Toscanelli. Bacon first divined, what to us is now only a commonplace, the rigorous necessity of controlling by positive experiment the affirmations of speculation and of reasoning. He did not wish to clip the wings of the human spirit to the space of a laboratory or of a dissecting-room, but he urged that inquiry should begin with the simplest objects of science and should extend gradually to the more complex, every observation being controlled by experiment. In chemistry his views rested on the work of the Arabian pioneers, Geber and Avicenna, and he did not, perhaps, do more actual work than Albertus Magnus; but he saw what the possibilities of chemistry were to a much greater degree than any of his contemporaries. He knew and judged the superstition of his time, but he kept himself above it by his knowledge and judgment. Goethe says that "the writings of Luther contain much more superstition than those of Roger Bacon." But he, to whom so much of the magic of nature had unfolded itself, knew that many natural phenomena (especially optical) could be made to appear to the ignorant as supernatural. He knew, too, that minute quantities of certain substances can destroy the red colour of copper, or the yellow colour of gold, and also the extraordinary effect of a small quantity of mercury on tin, or of lead on gold, so that it is not to be wondered at that he was looked upon as a magician or necromancer. These examples of natural magic are wonderful even to-day. His idea of the Aristotelian elements is characteristic; he imagines that each of the elements is convertible into the nature of another element; he says further, "barley is a horse by possibility, that is occult nature, and wheat is a possible man, and man is possible wheat." His idea is that all things

might arise from *hyle* (this is the Greek ὕλη), the true matter, but his clear and terse exposition of these ideas shows that he has no relationship to the mystics.

One of the papers in this volume excites one's apprehension: it is the essay on "Roger Bacon and Gunpowder," by Col. Hime, who explains certain passages in the *Epistola de Secretis Operibus Artis et Naturæ*, which are otherwise incomprehensible, by means of cryptogramic interpretation. Col. Hime certainly extracts sense out of passages otherwise meaningless, but on account of the ridiculous association of cryptograms with Roger Bacon's great namesake, one has got an uncomfortable feeling of doubt which makes one wish, probably unjustly, that this essay had not been included in the book. The relation of Bacon to the invention of gunpowder does not seem quite clear, even if we are not prejudiced against cryptograms. According to a very exhaustive review of the subject by MM. Reinaud and Favé,[1] gunpowder was known to the Arabians in the thirteenth century, so it seems doubtful whether it was an independent discovery of Bacon's, as Col. Hime believes. There was in all his work the tendency to try to solve the unknown through the known, and to conquer the far off through the near. He saw far beyond his day, and the things which were then being thought of and worked at, and he often speaks of things as if they were already attained. For instance, the telescope is treated of so wonderfully that it is almost impossible to believe that he did not possess one. But the mind of man is ever in advance of technique, and he trusted to the future more than to the current views of his contemporaries, of whom he was often somewhat contemptuous. He strove to reform scientific study and to insist that knowledge should be controlled by experimental research; and he demanded freedom of inquiry at a time when inquiry, like everything else, was ruled by authority. He fought the principal causes of the error and of the lethargy of his time: namely, the pride of a pretended knowledge which he thought was the principal cause of human ignorance, the rule of authority unfounded on knowledge, the adhesion to conventional views, the influence of the opinion of the ignorant multitude, and the darkness of the minds of those representing knowledge. His writings, although confused often by repetitions and re-arrange-

[1] *Du feu grégeois, des feux de guerre et des origines de la poudre à canon*, Paris, 1845.

ments, testify to the strength, radiance, and clearness of his mind, and these qualities made him feel the possibility of insight into both things material and things spiritual. He knew that the testimony of the senses was limited, and also that Nature hid many things from the senses. His love of mathematics made him feel that this was the principal key to all the hidden things of science, from the lower levels where the formula will solve the problem to the higher planes where there are only symbols. In everything his great need of certainty enabled him to make definite steps forward. What the ancients had experienced and thought, what he himself had found and illuminated, he brings before us, not very methodically perhaps, yet in a very striking and naïve manner, so that in reading him one feels definitely that "a spirit communicated is a perpetual possession." His maxims are simple but very fruitful in practice, and everything hangs together and has sequence; and since the known lay clearly before him, the unknown did not seem strange, for he saw ahead, which was then very hard to do, and saw those things which only have become clear after centuries of continuous observation of Nature, and ever finer technique. It would be well to remember this sentence of his in these days of minute specialism in science: "All the sciences are connected and foster one another with mutual aid. They are like parts of the same whole, every one of which accomplishes its own work, not for itself alone but for the others also."

That the complete work of such a man, who, by his reliance on independence of inquiry and his recognition of the immense importance of experimental science, is still a living power, should after seven hundred years be still inaccessible is a disgrace to the Science of which he is the father. That this disgrace may speedily be wiped out is "a consummation devoutly to be wished," not only on account of the advantage to science, but also that the memory of this poor brother of St. Francis may be duly honoured, after so long a period of neglect, by the publication of those works, written with such difficulty, which should stand as an undying example to us of piercing vision and heroic strength.

RECENT ADVANCES IN SCIENCE[1]

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A. Cambridge.

PROF. EMIL LAMPE of Berlin, the editor of the well-known *Jahrbuch über die Fortschritte der Mathematik*, recently wrote to the writer of this report that all the German mathematical periodicals are still published in spite of the war. The great trouble is that there is a shortage of compositors, so that, for example, the last part of the *Jahrbuch* for 1912, which would normally have appeared in May of this year, is probably delayed until August. However, we cannot, as a rule, get any idea over here as to what is being done by Germany in the world of mathematics. The task of settling accounts after the war will no doubt be an arduous one here as elsewhere.

Strictly speaking, the death of a mathematician is not a progress in mathematics. Yet deaths of great men remind us of some progress they took part in; and so we must here record the death of Morgan Crofton (1826—1915), perhaps best known for his article "Probability" in the ninth edition of the *Encyclopædia Britannica.*

Logic and the Principles of Mathematics.—In Prof. J. Brough's study (*Proc. Aristot. Soc.* 1914, **14**, 152) of some points in the logical doctrines of the authors in the first volume of the *Encyclopædia of the Philosophical Sciences*, mentioned in *Science Progress* for July 1915, p. 115, there is some mention of Couturat's article on mathematical logic, but the points touched on do not seem to be of much interest to mathematical logicians. The first important contributions to these branches of mathematics which must be noticed are three papers by Dr. Norbert Wiener (*Proc. Camb. Phil. Soc.* 1914, **17**, 387, 441, and 1915, **18**, 14). Wiener noticed that certain points in the logic of relations as given by Whitehead and Russell in their *Principia Mathematica* are simplified if we practically revert to Schröder's treatment, given in the third volume (1895) of his *Algebra der Logik*, of a relation as a class of couples.

[1] To be continued every quarter.

In all Wiener's papers, the symbolism of Whitehead and Russell is used. In his second paper, Wiener shows that we can regard the series of the instants of time as a construction from the non-serial relation of complete temporal succession between events in time. In a note to this paper, and in the third paper, he is occupied with methods for constructing series from non-serial relations quite in general. The method last given bears much the same relation to the various series of senstion-intensities that the method of the paper just referred to bears to the series of instants that constitutes one sort of extension, time.

Another short paper by Wiener in these *Proceedings* for 1915 (**18**, 56) contains a solution of the question to find the shortest line dividing an area in a given ratio. The proof is based entirely on first principles, and does not use any higher branch of technical mathematics. Another work on the principles of geometry is Dr. M. J. M. Hill's study of the fifth Book of Euclid's *Elements* from the point of view of its relation to the principle known as " the axiom of Archimedes," which is, however, assumed in the fourth definition of Euclid's fifth book (*Trans. Camb. Phil. Soc.* 1915, **22**, 87). The results of Hill's previous (1897, 1902) investigations on the subject of Euclid's fifth book are given in his *Theory of Proportion*, which will be reviewed in the January number of this Quarterly. We may also notice that the well-known logical method of treating geometry on the basis of axioms about indefinables such as " point " and " is collinear with " is exemplified in Prof. H. P. Manning's *Geometry of Four Dimensions* (New York : The Macmillan Co.; London : Macmillan & Co., Ltd., 1914, 8*s*. 6*d*. net).

Very closely connected with the principles of mathematics is the enormously important theory of aggregates, and a careful translation, with a long historical introduction and notes by the present writer, of Georg Cantor's famous memoirs of 1895 and 1897 on transfinite numbers has just been published (London : Open Court Co., 1915, 3*s*. 6*d*. net).

Arithmetic and Theory of Numbers.—The invention of logarithms simplified multiplication and division by reducing them to addition and subtraction respectively ; but involution and evolution were only replaced by the still possibly laborious multiplication and division. E. Chappell, in a paper read to the Royal Society in March of this year, describes a

table of logarithms of numbers recently compiled, by the use of which involution and evolution are reduced to addition and subtraction. These tables are likely to be very useful, for fractional indices, both positive and negative, are continually occurring in most branches of modern experimental science.

Perhaps the most striking thing in D. N. Lehmer's *List of Prime Numbers from* 1 *to* 10, 006, 721 (Washington D.C.: Carnegie Institution of Washington, 1914, 5 dollars) is the proof it gives of the accuracy of Riemann's famous formula for the number of primes which are less than a given number. The error is zero up to 9,050,000, and for 10,000,000 it is only + 87. The errors fluctuate in sign, and on this account alone Riemann's formula is superior to those of Legendre and Chebichev.

In the theory of numbers, H. C. Pocklington (*Proc. Camb. Phil. Soc.* 1915, **18**, 29), is occupied with a method for determining whether a large number is prime or composite.

Algebra.—Major P. A. MacMahon, in two papers (*Trans. Camb. Phil. Soc.* **22**, 55, 101) of 1914 and 1915, (1) defined and investigated permutation-indices of a new kind; (2) exhibited in their categories the invariants, algebraic and operational, of the Halphenian homographic substitution and the transformation of linear differential operators in general.

Prof. W. Burnside (*Proc. Lond. Math. Soc.* 1915, **14**, 251) completely solves what Cayley has called the problem of cyclotomic quinquisection—the determination of the system of relations expressing any rational function of the roots of a certain Abelian equation with a cyclical group as a linear function of the roots. To the theory of groups also belong W. E. H. Berwick's researches on the condition that a quintic equation should be soluble by radicals (*ibid.* 301).

Analysis and Theory of Functions.—Tetsuzô Kojima (*Science Reports of Tôhoku Imperial University*, **3**) gives a method for obtaining a better approximation to the graphical solution of the ordinary linear differential equation of the first order between two variables than that given by Czuber, and further applies graphical methods of a similar character to the approximate solution of certain other first-order equations, including the general equation of the first order and second degree.

Some years ago, Prof. H. F. Baker gave a general method for the solution of linear differential equations; and, in a paper read to the Royal Society on June 17, 1915, he showed

the application of the method to certain equations of astronomical interest.

It is well known that Abel gave a certain integral equation as a generalisation of the problem of the isochrone, stating that he had solved it, but the solution does not appear in his published works. The Rev. P. Browne (*Proc. Roy. Ir. Acad.* 1915) finds a solution of Abel's equation which involves integration along an infinite straight line in the plane of the complex variable; and extends the method to the solution of certain other functional equations.

In 1902, Prof. E. T. Whittaker discovered a simple integral representation of the general solution of Laplace's equation—a form in which most of the well-known particular solutions can be readily expressed. But the ellipsoidal harmonics, which are products of Lamé's functions, resisted all attempts to express them in this form. However, Whittaker (*Proc. Lond. Math. Soc.* 1915, **14**, 260) has now succeeded in doing this by his discovery of a certain integral equation of which the solutions are solutions of Lamé's differential equation.

Eric H. Neville gives (*ibid.* 308) a new method of solving simultaneous numerical functional equations, and illustrates it by the solution of the amusing puzzle as to how a circle may be completely covered by five smaller equal circular discs.

We will now pass to the theory of series and the closely connected theory of analytic functions. J. Kampé de Fériet (*Compt. Rend.* 1915, May 3) has given a generalisation of the series of Lagrange and Laplace. Prof. J. Pierpont's *Functions of a Complex Variable* is reviewed elsewhere in this number.

G. H. Hardy (*Proc. Lond. Math. Soc.* 1915, **14**, 269), acting on a suggestion made by Dr. H. Bohr and Prof. E. Landau, proves that the mean value of the modulus of an analytic function is, like the maximum of this modulus on a circle of radius r, a steadily increasing function of r. Hardy proves this and much more, and the importance of such researches is well known through some of the work of Poincaré, Hadamard, Borel, Blumenthal, and many others.

Geometry.—The history in Japanese mathematics of the Pythagorean equation connecting the sides of a right-angled triangle is discussed by Kiochiji Yanagihara (*Tôhoku Math. Journ.* **6**). Various methods of obtaining integral solutions of this equation were given in the eighteenth and nineteenth centuries, and the

theorem of Pythagoras was in constant use in Japanese mathematics, but many of the demonstrations had no logical force owing to the lack of systematic establishment of geometry. On this point reference may be made to Smith and Mikami's *History of Japanese Mathematics*, pp. 10, 13, 180.

A. J. Rahilly (*Proc. Roy. Ir. Acad.* 1915) very simply shows that relations between geometrical determinants of the type considered in a theorem of Frobenius exist between many sets of geometrical entities; in fact, a large part of the metrical geometry of the point, line, circle, plane, and sphere may be reduced to such determinant-equations.

Lastly, in 1915 Gaston Darboux has presented to the Paris Academy of Sciences papers on the representation on a plane of the surface of the fourth order with double conic; and a new edition of Salmon's *Geometry of Three Dimensions* will be reviewed in the January number of this Quarterly.

Notes on Applied Mathematics.—Prof. Louis T. More, in his book on *The Limitations of Science* (New York: Henry Holt & Co., 1915) has practically taken the same attitude towards modern physical science, with its theories of electrons and relativity, that Stallo did towards the modern physics of his day. It requires a degree of ignorance which is not so frequently met with among men of science nowadays as it was formerly to maintain that neither theory of knowledge or metaphysics enters into physical theory or even method in general. Dr. H. Wildon Carr discusses (*Proc. Aristot. Soc.* 1914, **14**, 407) the relations to metaphysical theory of the new kinematics resulting from the principle of relativity. It appeared to H. S. Shelton (*ibid.* 100), in working out a thesis that the co-ordination of the facts and theories of science is a branch of philosophy and that this co-ordination should not be a shadow but a solid reality, that the principles of the interrelation between mathematics, considered as a pure science, and its application to physical problems required investigation. For a lack of a clear understanding of the methodology of mathematics, the whole of the work of Lord Kelvin and Tait on the secular cooling of the earth and allied subjects was absolutely invalid, not merely on account of discovery in radioactivity, but inherently and theoretically invalid. This is to prove the claim as to the solid reality of the philosophy of co-ordination above referred to—what Shelton calls "objective philosophy"—and the

work of Cowell, Rutherford, Stoney, Joly, Sollas and others, on the subject is also criticised.

In the application of mathematical methods to physical phenomena, it is obviously very important that one should be able sometimes to follow out what goes on, by geometrical conceptions, without blind calculation. Sometimes such calculations are necessary and advantageous from the point of view of generality and economy of effort, as we see in Lagrange's *Mécanique*. But vector methods, for example, give us far greater insight, and insight is often prized, perhaps rightly, even above generality. In the theory of the gyroscope, Prof. Horace Lamb (*Proc. Roy. Soc. Edinb.* 1915, **35**) obtains the *intrinsic* equations and shows that, besides their uses as a basis of calculation, they enable us to foresee the general character of the motion in cases where the actual calculation would be difficult.

The analogy between paths of particles and forms of strings, on the one hand, and the paths of rays of light on the other, is familiar to all who have studied the applications of the principle of least action; and Prof. Levi Civita (*Atti dei Lincei*, 1915, **24**), gives a general dynamical investigation of certain reciprocal theorems in optics due to Helmholtz and Straubel, which is based on this analogy.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

The Age of the Earth.—It has been generally assumed that the conclusion reached by Lord Kelvin from his discussion of the age of the Earth—viz. that the temperature of the Earth's crust cannot have been near its present value for much more than twenty or thirty million years—has been invalidated by the discovery of radioactive elements, and that in this way the much greater latitude required by the theory of tidal friction and by geologists might be accounted for. In a letter to *Nature* (vol. 95, p. 204, 1915, April 22) Dr. F. A. Lindemann argues against such a conclusion. Lord Kelvin's estimate was based upon three independent considerations—the temperature gradient inside the Earth's crust, the amount of tidal friction, and the total amount of energy radiated by the Sun. The first of these arguments has been invalidated by the discovery of radioactivity. Since, however, the tempera-

ture of the Earth is conditioned by the amount of heat received from the Sun, the time during which the Earth can have existed in its present state cannot be greater than the time since which the temperature of the Sun has been about its present value. Sir Ernest Rutherford has, however, shown that even if the Sun were made entirely of uranium, only about five million years would be added to its duration as a heat-giver, so that here radioactivity can have but little effect. Dr. Lindemann shows also that if the Sun were supposed formed by the collision of two extinct stars, the duration of its heat-supply would be unaffected by radioactivity. Therefore neither radioactivity nor any other known cause will account for a longer period than Kelvin supposed.

To this argument C. E. Stromeyer replied (*Nature*, vol. 95, p. 259, May 6) that there is a net loss in the Earth's heat over and above any possible interchange of heat with the Sun, and further that the permissible age of the Sun might be increased by the heat-producing power of a meteoric bombardment. Lindemann, however, pointed out (*Nature*, vol. 95, p. 372, June 3) that the amount of energy lost in consequence of the Earth's temperature gradient is 6,000 times less than the total amount radiated, and so may be neglected, and, further, that the heat due to meteoric bombardment to date had been taken into account in the assumed mass of the Sun.

A further objection was raised (*Observatory*, vol. 38, p. 261, 1915, June), viz. that, assuming stars to be formed by collision, a short life of twenty million years for the Sun would require the production of about fifty novæ per year to maintain a stellar system of one thousand million stars. Lindemann replied (*Observatory*, vol. 38, p. 299, 1915, July) that although this number might be reduced owing to the fact that the real age of the Earth may be considerably lengthened if the time is included during which its temperature was much lower than it is at present, yet the number of novæ required still remains larger than seems admissible. He suggests that this difficulty may be avoided by admitting the existence of many dark stars, which seems reasonable if the life of a single star is comparatively short. The strongest evidence against a short age of the Sun and the Earth, in his opinion, is provided by Prof. Strutt's estimation of the age of various radioactive minerals from the amount of helium which they contain. He concludes

that if the age of the Earth is admitted to be large, then it must be assumed that some catastrophic event must have occurred in the sidereal system at no very distant epoch.

Stellar Dynamics.—The well-known phenomenon of star-streaming indicates that the stars, instead of moving in all directions at random, have a predominant tendency to move in two opposite directions. Prof. Turner suggested that these preferential directions are directed along a line passing through the centre of the stellar system, and that the stars are, in reality, describing elongated orbits about the centre, the two streams being the aggregate of the stars moving respectively towards and away from the centre. The objection to this explanation was that it apparently necessitated a great concentration of stars near the centre—greater than appeared admissible. Prof. Eddington (*M.N. R.A.S.* vol. 75, p. 366, 1915, March) has discussed the dynamics of a globular stellar system which, for the sake of simplicity, was supposed to possess spherical symmetry. Starting from Schwarzschild's ellipsoidal law of velocities, which is in good agreement with observation, he has succeeded in deducing a density law which does not give too great a concentration at the centre, and also gives a density which diminishes continuously from the centre outwards. Although the system discussed is an ideal one, this article is a valuable contribution towards our knowledge of stellar dynamics, and it is to be hoped that the author will succeed in extending his results to a system more nearly approaching our sidereal system in its construction.

The Nature of Cepheid Variation.—The term Cepheid is given to a class of short-period variables in which the rise from minimum to maximum brightness occupies less than half the period of variation. The brightness varies continuously, and so the variability cannot be due to eclipses. Their spectroscopic study has led to the belief that they are, nevertheless, binary systems, whose period of orbital revolution is equal to the period of their light variation. The maximum brightness is found to occur near the point of maximum velocity of approach. No satisfactory theory of the dependence of the brightness on orbital motion has been advanced. R. H. Curtiss (*Astroph. Journ.* 20, p. 186, and also *Pub. Astron. Obs. Univ. Mich.* vol. i. 1915, p. 104) has attempted an explanation by supposing the existence of a resisting medium around the

stars, which brushes back the atmosphere from the front surface of the secondary. Harlow Shapley (*Astroph. Journ.* 40, p. 448), in a thorough discussion of the problem of Cepheid variation, advances strong arguments against the possibility of these stars being binary systems. The Cepheids are all stars of very great absolute luminosity and of large dimensions, and the small dimensions of their orbits, obtained on the binary theory, seem very improbable, being often of smaller dimensions than the stars themselves. Shapley favours the hypothesis that the variations in brightness are due to regular periodic pulsations in the atmosphere of a single star. This would explain the connection between maximum brightness and maximum radial velocity. The periods of free vibrations and the variations of the radial velocity obtained on this hypothesis are of about the right order of magnitude, but the necessary displacements in the atmosphere appear somewhat large. The Cepheids are all solar-type stars, and if this hypothesis is true, we may regard our Sun as a long-period Cepheid variable, the period being that of the sun-spot cycle, but the range of variation being very small.

C. D. Perrine (*Astroph. Journ.* 41, p. 307), has also advanced a tentative explanation, assuming the stars to be binary systems. He shows that the orbits are of small eccentricity and small dimensions and that the mass of the secondary is small, and suggests that the variation in the light is caused mainly by variations in the light of the secondary, due to the disturbance in its orbit near periastron.

Orbits of Eclipsing Binaries.—An important volume on this subject has recently appeared (*A Study of the Orbits of Eclipsing Binaries*, by Harlow Shapley, *Contrib. from Princeton Univ. Obs.* No. 3). Within the last few years new methods for the computation of the orbit of an eclipsing binary from the observations of its brightness have been introduced and developed by Prof. H. N. Russell and Dr. Shapley. In this publication a brief summary of the theory underlying these methods is given, together with some applications of them to stars typical of the various classes which arise, and also the results of the treatment of the observations of ninety eclipsing variables. In this treatment, practically the whole of the observational material available—to which Dr. Shapley himself has largely contributed—has been discussed. In

many cases orbits have been calculated both on the supposition of uniform brightness of the discs and also on those of a darkening towards the limb, such as is found in the case of the sun. The evidence in favour of such a darkening does not appear very convincing. In the final chapter is given a general discussion of the results obtained.

The Measurement of the Heat from the Stars.—In *Pub. Ast. Soc. Par.* vol. 26, p. 169, 1914, a preliminary account was given of some experiments on the measurement of radiation from stars made with the Crossley reflector of the Lick Observatory, by W. W. Coblenz, of the U.S.A. Bureau of Standards. Further particulars of the methods employed, and of the various problems which the perfection of the method may be expected to assist in solving, are given in *Pop. Sci. Monthly*, vol. 86, p. 432, 1915. The method adopted was to focus the rays from the star on a specially constructed very delicate thermo-element, placed in a glass cell with a fluorite window, at a very low pressure. The deflection produced in a delicate galvanometer connected with the cell gave a measure of the total energy received. It was found that red stars gave a much larger deflection than blue ones of the same visual magnitude. Some idea as to the distribution of energy in the spectrum was obtained by measuring the deflection when the radiation was passed through a cell of water. The results obtained and the information which they provide as to the temperatures of different types of stars have been independently discussed by Keivin Burns and P. W. Merrill (*Pub. Ast. Soc. Pac.* 27, p. 110 and p. 120, 1915). It is found that the results enable the temperatures of the later-type stars (types G—M) to be assigned with considerable accuracy, but that the temperatures of stars of types B and A are more uncertain, owing to the fact that they are found not to radiate as black bodies. The evidence, however, tends to indicate that type A stars are of a higher temperature than type B. From type A to type M the average temperature steadily decreases from about 8,000° to about 2,500°. The temperature of the Sun (type G) is generally supposed to about 6,000°, in fair agreement with the preceding result. These first experiments were admittedly experimental, and we may hope that, with the further perfecting of the work, very valuable results will be obtained.

PHYSICS. By JAMES RICE, M.A., University, Liverpool.

Ionisation.—In the *Phil. Mag.* for April and May, Prof. J. A. Pollock publishes the results of some work on the nature of the large ions in the air, which were discovered by Langevin in 1905 and have a mobility approximately equal to 1/3000; and also on the nature of a new type of ion in the air, with a mobility intermediate between that of the Langevin ion and the small gas ion of mobility 1·5. He summarises a good deal of information concerning the Langevin ion, which he considers to be a collection of water molecules surrounding a dust particle, the whole being electrified by the attachment of a small ion, and so affording an interesting example of the adsorption of water vapour at a rigid surface. The application of a thermodynamic argument leads to the conclusion that the adsorbed moisture is in the liquid state with a latent heat and density little different from those of water. This ion, it would appear, has a mobility which at constant atmospheric pressure is a function of the relative humidity only, and a diameter varying from 3 to 4 micromillimetres according to atmospheric conditions. Regarding the second type of ion, it would seem that it cannot exist in the air if the vapour pressure is above 17 mms., and its mobility (on the average about 1/50) depends on the vapour pressure rather than on the relative humidity. It is probable that it consists of a rigid nucleus enveloped in a dense atmosphere of water-vapour, the mass of the ion becoming greater as the vapour pressure increases until the absorbed fluid assumes the liquid state and the aggregation develops into the large ion of Langevin, which alone exists above the critical pressure mentioned above.

Dr. N. Campbell (*Phil. Mag.* June) gives a preliminary account of some experiments still in progress, on the ionisation produced by the positive particles liberated from heated sodium and aluminium phosphates, the ionisation consisting of the liberation of electrons from the surface of a copper plate on which the positive particles impinge. Curves are given showing the connection between the P.D. through which the particles fall and the number of electrons liberated by each positive particle. This number reaches a maximum at a P.D. between 30,000 and 40,000 volts, the maximum being in the neighbourhood of 3. The suggestion is made that the fall in the number beyond this P.D. is due to the fact that

the particles are beginning to penetrate the surface-layer of the metal. This is a property which distinguishes α-rays from canal-rays, but the author states that, even at the highest potential reached (about 54,000 volts), there was no indication that the positive particles were beginning to acquire the great ionising power of the α-rays.

Radioactivity and X-Rays.—The *Phil. Mag.* for May contains the second part of a paper by Messrs. A. Holmes and R. W. Lawson on the end product of thorium in certain radioactive minerals (the first part was published in the *Phil. Mag.* December 1914). The authors come to the conclusion that thorium lead (thorium E) is an unstable product with a half-period approximately 10^6 years. It is highly probable that it disintegrates with the loss of a β-particle and so produces an isotope of bismuth. It seems unlikely that this thorium bismuth (thorium F) is stable, and further work will be needed to determine its half-period and whether the product of its disintegration is a thallium isotope or a polonium isotope.

R. W. Varder (*Phil. Mag.* May) gives the results of experiments on the absorption of homogeneous -rays by aluminium and compares them with a formula deduced from theoretical consideration by Dr. N. Bohr.

H. Richardson (*Proc. Roy. Soc.* A. vol. xci., A. 630 June) discusses the results of some work carried out by him on the absorption in lead of the γ-rays emitted by radium B and radium C. In addition to the penetrating type of radiation whose absorption coefficient μ is 0·5 (cm.$^{-1}$) in lead, radium C emits also the soft types for which $\mu = 46$, $\mu = 6{\cdot}0$, and $\mu = 1{\cdot}5$, and which are practically absorbed in 1·5 cm. of lead. Radium B emits not only radiations closely similar to three soft types referred to (a fact already noted by Rutherford and Andrade), but also a radiation for which $\mu = 40$ in aluminium. The absorption of the radiations in different elements has been examined, to find evidence, if possible, of the anomalous absorption investigated by Barkla for elements of lower atomic weight, in virtue of which the absorption coefficient of the radiation characteristic of an element is abnormally high for an absorber whose atomic number is slightly less than that of emitting element. Working with uranium, lead, mercury, gold, and barium as absorbers, the author finds no evidence of an anomalous rise in the absorption coefficients of the

radium B and radium C radiations, as the atomic number of the absorber approaches that of the emitting radium product.

In the *Proc. Roy. Soc.* (A. vol. xci., A. 629, May), H. Moore continues some work on the corpuscular radiation liberated in vapours by homogeneous X-radiation; the previous experiments have been described in the *Phil. Mag.* January 1914, and the two papers show that for carbon, oxygen, nitrogen, sulphur, and chlorine, the number of corpuscles liberated from an atom by a beam of X-rays is the same whether the atom is in combination or not, and this atomic corpuscular radiation is approximately proportional to the fourth power of the weight of the atom; by combining his work with some earlier results of Barkla, Simons, and Philpot, the author shows that the X-radiation absorbed by an atom of a given element is also proportional to the fourth power of the atomic weight, and suggests that these results, combined with others obtained recently by Bragg and Pierce for elements in the solid state, form a body of evidence for the universal validity of this rule. In a further paper in the *Proc. Phys. Soc. London* for June, the same author applies these results to the discovery of a method for calculating the absorption coefficients of different substances for homogeneous X-radiation.

Thermionics.—The *Phil. Mag.* for June contains a paper by K. K. Smith which is in continuation of some earlier work of Prof. Richardson (*Phil. Mag.* xxvi. 345 (1913)]. It exhibits the results of an exhaustive series of experiments on the negative thermionic currents from tungsten, and opposes the criticism which has recently been levelled in certain quarters at the usual explanation of such thermionic currents. The author concludes that the formula $i = aT^n e^{-\frac{b}{T}}$ has an enormous range of validity, but points out that so far the data obtained are unable to decide whether the value $n = \frac{1}{2}$ or $n = 2$ gives the best agreement with the results.

Another paper, by Prof. Horton in the *Proc. Roy. Soc.* (A. 91, A. 629, May), also supports the orthodox view concerning thermionic currents, by showing that the emission of electrons from a glowing Nernst filament or glowing lime is independent of the nature of the gas in the discharge tube, and that the apparent increase in the number of electrons emitted at certain gas pressures is really an increase in the

thermionic current due to ionisation of the gas molecules by collisions.

Optics.—In the *Proc. Roy. Soc.* (A. 91, A. 630, June) the Hon. R. J. Strutt describes experiments carried out on the resonance radiation of sodium vapour. It appears that the centres emitting the resonance radiation of sodium vapour excited by the D lines are not persistent enough to be carried along with the vapour when it is distilled away from the place of excitation. This is in sharp contrast, not only with the behaviour of sodium vapour excited electrically, but also with the behaviour of mercury vapour whether excited optically or electrically. The resonance radiation is invisible through even a very dilute layer of sodium vapour placed in front of it; moreover it is changed in intensity when the vapour is placed in a magnetic field, a result which is quite in accordance with the Zeeman effect.

The same number contains an account of some experiments by T. R. Merton on the origin of the "4686" series. A considerable controversy has been raging for some time around the type of atom responsible for this series. The earlier view ascribed it to hydrogen. Lately, under the influence of the Bohr theory of the atom, the view that the series is produced by helium has been gaining ground, and some spectroscopic work by Mr. Evans (referred to in the last number of SCIENCE PROGRESS) supports it. Mr. Merton employs a vacuum tube containing helium and hydrogen at low pressure giving the helium lines, hydrogen lines, and 4686, and determines the highest order of interference of the spectrum lines at which the fringes produced by the method of Fabry and Perot remain visible. This method enables the observer to calculate the ratio of the mass of a known atom emitting a radiation to the mass of an atom emitting another radiation, when he knows the limiting orders at which these two radiations show interference fringes. The author, while pointing out possible sources of error, arrives at the conclusion that the 4686 line is due to systems of subatomic mass, having a mass about one-tenth of that of hydrogen.

Heat.—T. Carlton Sutton (*Phil. Mag.* April) discusses the validity of two formulæ which have been used in recent work for determining the latent heat of vaporisation of liquids. By applying the Clausius-Clapeyron equation to the Biot

formula connecting vapour pressure and temperature, viz. $\log p = A + B\alpha^t + C\beta^t$ (where A, B, C, α, β are constants), one obtains an equation for the latent heat which fits the facts very well for all the non-associated liquids which have been examined. But if a similar process is applied to a slightly modified van der Waals' formula (the modification being consistent with the notion of a molecule expanding uniformly with rise of temperature, a notion recently put forward in other quarters), then the facts for non-associated liquids are equally well summarised. Neither formula, however, seems to agree with the facts concerning associated liquids. The same number of the *Phil. Mag.* contains a paper by Dr. A. Ferguson explaining a new empirical formula connecting the boiling-points and the molecular weights of the normal paraffins with considerable accuracy over a wide range, and also, for the same series, empirical formulæ showing the relation between critical temperature and chemical constitution.

When fusion of a pure metal takes place, there is, as is well known, a sudden decrease in its electrical conductivity in most cases. According to the well-known generalisation of the Wiedemann-Franz law, there ought to be a corresponding change in the value of the thermal conductivity. The June number of *Proc. Phys. Soc. London* contains an investigation by Prof. Porter and F. Simeon of this point. For mercury and sodium they find that the change in the thermal conductivity on fusion is of the same order as that of the electrical.

Sound.—In the same journal there is a paper by Prof. Morton and Miss Darragh on the origin of Combination Tones, and a discussion of the theories bearing on it, one propounded by Koenig and extended by Voigt, the other proposed by Helmholtz and generalised by Everett.

The magazines referred to above also contain descriptions of apparatus for various purposes. A Duplex Harmonograph for compounding four harmonic motions is described in the April *Phil. Mag.* An instrument to measure air velocities, by measuring the resistance of a hot wire lying in the air-current (the Kelvin Double-Bridge being employed) also appears in the same number. In the April *Proc. Roy. Soc.* Prof. Callendar, W. A. Bone, and H. J. Yates show how the bolometer may be adapted to the measurement of the efficiencies of radiating surfaces such as gas-fires, electric radiators, incandescent

surface combustion diaphragms, and the like. In the *Proc. Phys. Soc. London* (June) Prof. Fleming describes an instrument which will project on a screen or photograph on a plate such curves as hysteresis curves, characteristic curves of wireless detectors or rectifiers, or resonance curves, in process of delineation.

There are also a number of papers bearing on the structure of the atom, and on formulæ, meant to summarise spectroscopic series, which are extensions of the Rydberg-Ritz type of formula.

Two important mathematical-physical papers in the *Proc. Roy. Soc.* should be mentioned : one, by Prof. Hicks, in the April number, deals with the orbits of a charged particle in motion round a nucleus which is both electrified and magnetised; the other, by Lord Rayleigh, in the May number, is concerned with the application of the third order of approximation to deep water waves, progressive or stationary.

INORGANIC CHEMISTRY. By C. Scott Garrett, B.Sc., University Liverpool.

Elements.—The long-drawn-out controversy which has raged around the conditions under which active nitrogen is formed has at length been settled. The main point under dispute was the presence of a trace of oxygen in the nitrogen, as an essential condition for the formation of the *active* modification. After some collaboration and a mutual exchange of apparatus by the opposing sides, it was found that neither view in itself was entirely correct. In the last paper on the subject (*Proc. Roy. Soc.* A. **91**, 303, 1915) Strutt, the original discoverer of the new modification, reviews the whole controversy. A little active nitrogen can be produced by the passage of the discharge in absolutely pure nitrogen, but the addition of a small quantity, not only of oxygen, but of almost any other foreign gas, enormously increases the yield of the active modification. Thus hydrogen sulphide, water vapour, carbon dioxide, carbon monoxide, acetylene, ethylene, methane, oxygen, mercury vapour, chlorine and hydrogen are effective to a degree roughly diminishing from left to right. With methane 1 part in 30,000 parts of nitrogen, was found to have an influence, but the maximum effect is obtained when the ratio is about 1 to 1,000. The inert gases, argon and helium, are, not altogether unexpectedly, without effect. As an explanation of the pheno-

menon Strutt suggests that the character of the impact of the foreign gas with the nitrogen molecules is altered by their known capacity of combining with the free electrons in the discharge space. Argon, helium, and nitrogen itself lack this property, so that, consequently, they are unlikely to function as activators. Mercury vapour, although it promotes activation of the nitrogen, does not, however, assume a negative charge in the canal rays. This anomalous behaviour of mercury vapour may possibly be explained when it is remembered that the positive charges borne by the mercury atoms in the canal rays are considerably larger than is normally the case. In connection with these results it is of some interest to find that recently in several papers Baly has put forward a theory of reactivity, based on the opening up of the condensed electromagnetic force fields of atoms and molecules, by the presence of small amounts of chemically opposite activators. In our present state of knowledge of the exact significance of such intra-atomic and inter-molecular forces it is not impossible that both views have an entirely compatible basis.

Compounds.—The extended investigations of Stock and Priess on compounds of boron, more particularly the hydrides, have now reached the concluding stages. The chemical behaviour and physical constants of the trichloride and tribromide class these compounds as distinctively non-metallic. The four well-defined hydrides, B_2H_6, B_4H_{10}, B_6H_{12}, and $B_{10}H_{14}$, which so far have been isolated, are in the latest paper (*Ber.* **47**, 3115, 1914) studied in respect to their behaviour with chlorine and bromine. Replacement of hydrogen by halogen is the general rule, such compounds as $B_{10}H_{12}Br_2$ and B_2H_5Br being obtained. With excess of halogen B_2H_6 first gives highly halogenated derivatives like $B_2H_3X_3$. which later split up into the mono-haloid B_2H_5X or the original hydride B_2H_6 and BX_3. In no case could evidence of BH_3 or its halogen derivatives be obtained, and such a compound would appear to be as incapable of separate existence as the methyl group CH_3. The conclusions which the authors draw from their extensive study of these boron derivatives are highly interesting, and mark an advance on previous conceptions. Boron would seem to be an exception to the Bodländer-Abegg rule that the sum of the positive and negative valencies of an element is eight; its highest negative valency (hydrogen) must be placed

at four, whilst its greatest positive valency (chlorine, oxygen) determined with any certainty is never greater than three.

Further advances in the domain of preparative work come from Franklin (*J. Amer. Chem. Soc.* **37**, 852, 1915). In this fruitful field of reaction (liquid ammonia) a potassium ammonoargenate, AgNHK NH_3, has been produced by the combination of silver amide with potassamide. A crystalline compound separates out, in which the terminal NH_3 molecule may be looked upon as "ammonia of crystallisation," analogously to "water of crystallisation."

By a study of equilibrium relationships of aqueous potassium and sodium sulphates at various temperatures Okada (*Mem. Coll. Sci. Kyoto*, **1**, 95, 1914) draws the conclusion that a double salt $K_3Na(SO_4)_2$ has a stable existence. If correct such a salt would clearly indicate the superior electro-positivity of the potassium atom when matched against the sodium atom.

Another sulphate, that of cadmium, which is found in the solid state to have empirically the abnormal formula $CdSO_4$ $8/3H_2O$, has been the subject of investigation by Blomberg (*Zeitsch. anorg. Chem.* **91**, 248, 1915). In concentrated solutions, according to Noyes and Drucker, ionisation would take place according to the following equation:

$$3CdSO_4 \rightleftarrows [Cd_2SO_4]^{\cdot\cdot} + [Cd(SO_4)_2]''$$

If now, Blomberg points out, the two complex ions combine together when crystallisation takes place, it is possible for a hydrate to be formed which will not contain a number of water molecules exactly divisible by three.

Analytical.—Perhaps the most important progress we have to report under this heading is the compilation and publication by the Institute of Chemistry of a "List of Reagents for Analytical Purposes." This important publication gives a list of the finer chemicals obtainable from British manufactures along with the standards of purity to which each reagent attains and the tests, namely, those adopted by the Eighth International Congress of Applied Chemistry, which have been applied. Some such work as this has been badly needed, not only by analytical chemists, but by all workers in the wider field of chemistry, and it is sincerely to be hoped that the Institute of Chemistry will continue their good work by enlarging and revising the List from time to time.

An analytical reaction of some importance has been discovered by Guareschi (*Atti. R. Accad. Sci. Torino*, **50**, 354, 1915) in the action of ammonium bromide on metallic iodides. All such compounds, even the very stable AgI, are decomposed when heated in a long narrow hard-glass tube with ammonium bromide, a reaction taking place according to the following equation :

$$2MI + 2NH_4Br \rightarrow 2MBr + 2NH_3 + H_2 + I_2.$$

The method has proved capable of detecting as little as 0.0001 grm. potassium iodide in presence of a large amount of alkali or alkaline earth chlorides or bromides. As it is possible to obtain insoluble iodides of most metals, the method is pregnant with capabilities.

A couple of interesting papers have recently appeared on the relative merits of the commoner desiccating agents, and, as the results are of general utility to chemists, we may conveniently include them here.

Berkeley and Hartley (*Phil. Mag.* **29**, 609, 1915) claim great efficiency for a surface type of sulphuric acid drier. Sulphuric acid would seem to be as capable of drying air as phosphorus pentoxide for amounts of moisture of the order of the ordinary atmospheric vapour pressure. Pure anhydrous copper sulphate, strangely enough, is very effective if the air only contains traces of moisture—amounts less than the vapour-pressure of the hydrate $CuSO_4 . H_2O$.

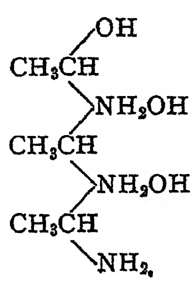
In the ordinary desiccating jar under reduced pressure, Marsden and Elliot (*J. Ind. Eng. Chem.* **7**, 320, 1915) find aluminium oxide more efficient than either sulphuric acid or calcium chloride, especially if the amount of moisture is small. If the amount is large, however, 95 per cent. sulphuric acid is as good as aluminium oxide, whilst calcium chloride is inferior to both.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School.

A method for distinguishing Tautomeric, Isomeric, and Polymeric from Polymorphic substances has recently been published by Sidgwick (*Trans. Chem. Soc.* 1915, **107**, 672). Many examples are known of substances which are capable of existing in two mutually interconvertible modifications ; this condition

of affairs may be due to a difference in molecular structure between the two forms (tautomerism, dynamic isomerism, polymerism) or to a different arrangement of identical molecules within the crystals (physical dimorphism, polymorphism). If the two forms are dynamic isomers they will retain their identity in solution for a time at least, whereas if they are physical polymorphs the differences between them will only persist in the solid state, and will disappear in solution. To determine to which class a given pair of substances belong the author adds some of the variety B to a saturated solution of the more soluble variety A. If the forms are polymorphic and consequently both give the same molecules in solution, the concentration of the solution will remain unchanged, since B cannot dissolve owing to the solution being saturated, or the solution might even become slightly weaker, owing to some of A crystallising out on to B. If, however, the two forms are tautomeric the solution will become stronger on adding B since the latter will dissolve in the solution irrespective of the presence of A, provided of course that the tautomeric change is not too rapid. The method employed for observing the changes in concentration is to determine the freezing-point of the solution of one modification, and then to add some of the second modification, and to redetermine the freezing-point. If there is any appreciable lowering, the second substance is a tautomer or isomer, and if not it is a polymorphic variety.

Since the discovery of aldehyde ammonia by Liebig in 1835, it has been the generally accepted view that this substance was a simple additive compound formed from one molecule of each of the two reacting substances. According to Ossian Aschan, however (*Berichte*, 1915, **48**, 874), this view is incorrect, and the constitution of this substance should be represented by the formula:



Instead of preparing it by the old method of passing dry ammonia into the aldehyde dissolved in ether, the author finds that it may be obtained more rapidly by mixing the aldehyde with a strong aqueous solution of ammonia when, on cooling, the substance separates out in a pure condition. Aldehyde ammonia, when freshly prepared in this way, is a trimolecular compound $\left(CH_3CH\left\langle\begin{matrix}OH\\NH_2\end{matrix}\right.\right)_3$, but, on keeping, it gradually changes to the bimolecular compound, according to the equation

$$2(C_2H_7NO)_3 = 3(C_2H_7NO)_2.$$

The ordinary methods employed for benzoylating polyhydric alcohols, result in the replacement of all the hydroxyl groups. Emil Fischer (*Berichte*, 1915, **48**, 266) has now succeeded in partially benzoylating dulcitol and mannitol, and has prepared mono-and di-benzoyl dulcitol $C_6H_{13}O_6, COC_6H_5$ and $C_6H_{12}O_6(COC_6H_5)_2$, and tetrabenzoyl mannitol $C_6H_{10}O_6(COC_6H_5)_4$. The method adopted consists in making the acetone compound of the alcohol by shaking the latter with acetone containing 1 per cent. of hydrogen chloride, as described by the same author some years ago (*Berichte*, 1895, **28**, 1168). Treated in this way mannitol gives mannitol triacetone.

$$C_6H_{14}O_6 + 3CH_3COCH_3 = C_{15}H_{26}O_6 + 3H_2O.$$

From this substance the partially benzoylated alcohol tetrabenzoyl mannitol can be obtained by treatment with benzoyl chloride and quinoline followed by the removal of the acetone complex by means of a 5 per cent. alcoholic solution of glacial acetic acid.

The influence of under-mastication (bolting) and over-mastication (Fletcherising) upon the utilisation of ingested protein has recently been investigated by Foster and Hawk (*J. Amer. Chem. Soc.* 1915, **37**, 1347). According to Chittenden thorough mastication of food is a material aid to the digestion, while Metchnikoff considers that it is harmful to chew food very long, and only to swallow it after keeping it in the mouth for a considerable time. A disorder known in America as "Bradyphagism" caused by the habit of eating too slowly is apparently cured by eating more quickly. According to the authors the importance of chewing of protein foods has been overestimated, for, as they point out, all high protein

feeding animals bolt their food. In the present experiments two normal men were fed on a fairly high nitrogen diet the principal protein of which was cooked beef in the form of 15 mm. cubes. A preliminary period of normal feeding was followed by a period of bolting, and then by one of slow mastication, and finally there followed a second period of normal feeding. The results, so far as protein utilisation is concerned do not support the claims of Fletcher, the champion of excessive mastication of food, nor do they demonstrate the harmfulness of food bolting. The protein utilisation was actually highest during slow mastication, or "Fletcherism," but it averaged only about 0·17 per cent. higher than during normal mastication.

A great many suggestions have been put forward recently for dealing with the fly danger in the war area. In a recent communication Roubaud (*Comptes rend.* 1915, **160**, 692) recommends the use of powdered ferric sulphate. This substance, if dusted on to dead bodies, acts as a very efficient disinfectant, and destroys the larvæ of flies, and, according to the author, is more efficient than ferrous sulphate, bleaching powder, quicklime or formalin. Putrid meat treated with the powdered salt loses its smell almost at once, and the larvæ of flies are killed by a 20 per cent. solution in 3 hours. As a protection against flies, latrines should be sprayed with a mixture of 2·5 kilos of ferric sulphate, 500 cc. of heavy coal-tar oil, and 10 litres of water for every 2 cubic metres of latrine. This mixture being destructive to vegetable life, must not be applied to manure used for agricultural purposes. Manure-heaps may, however, be sprayed with a 5 per cent. solution of sodium cresylate, using 15 litres per cubic metre, and later with a 10 per cent. solution of ferric sulphate.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Stratigraphy and Regional Geology.—Prof. H. E. Gregory has studied the formation and distribution of fluviatile and marine gravels as a contribution to stratigraphy from the viewpoint of physiography (*Amer. Jour. Sci.* 1915, 4, **39**, 487). He makes an attempt to analyse the physiographic features resulting from ocean and river-work in so far as they are related to the deposition of gravel, hoping thereby to recognise constant features characterising conglomerates of various origin. The

lack of quantitative data has rendered this study indecisive, but Prof. Gregory shows that by far the greater amount of gravel is yielded by the continental agencies, rivers and glaciers, whilst marine action yields relatively little.

An account of the geology of the island of South Georgia is given by D. Ferguson, Prof. J. W. Gregory, and the writer (*Trans. Roy. Soc. Edin.* 1915, L, 797), based on observations and collections made by the first-named author. The island consists of an extensive sedimentary series, mainly arenaceous in character, but with a considerable development of argillaceous types, and bedded trachytic tuffs, which have undergone sporadic scapolitisation. The evidence of the few crushed and fragmental fossils as to the age of the rocks is ambiguous, and appears to show that a single well-knit formation, the Cumberland Bay Series, is probably Lower Palæozoic in its basal, and Mesozoic in its upper beds. The facts are not sufficient for the authors to pronounce definitely on the tectonic relations of the island; and the question whether it belongs to a great loop, the so-called "Southern Antilles" of Suess, connecting the Andes with Graham Land, or whether it is a remnant of Prof. Schwarz's old southern continent of Flabellites Land, is left open pending the collection of new facts.

The Canadian Geological Survey publishes a memoir (No. 67) by D. D. Cairnes, on the Yukon-Alaska International Boundary between the Porcupine and Yukon Rivers. The boundary traverses a mountainous region which is built mainly of sediments forming one of the most complete Palæozoic sections yet known in the North American Cordillera. Another Cordilleran area is described by C. W. Drysdale in a memoir (No. 56) on the geology of the Franklin Mining Camp, British Columbia. Here the great Mesozoic batholiths appear, also a later series of alkaline rocks (*postea*, under Petrology), intruded respectively into Palæozoic and Cainozoic sediments.

Petrology-Igneous Rocks.—J. P. Iddings and E. W. Morley (*Jour. Geol.* 1915, **23**, 231) describe the leucitic lavas of Mount Mouriah in Java and the shonkinites and nepheline-syenites of the Pic de Maros in Celebes. The first-named group includes vicoite, leucite-tephrite, leucitophyre, and shoshonite lavas. The Celebes locality exhibits several varieties of shonkinite, including one rich in biotite and augite which is called *marosite*. Nepheline-syenites, with syenite-porphyry, bostonite, trachyte,

and phonolite, form the summit of the Pic de Maros. A comparison of the chemical composition of the rocks from these two districts shows that they are chemically similar. Highly potassic rocks are now known to exist within a region nearly a thousand miles in length, covering areas in Java, Celebes, and Borneo.

H. S. Washington has commenced a series of papers descriptive of the igneous rocks of Sardinia (*Amer. Jour. Sci.* 1915 (4), **39**, 513). The volcano of Monte Ferru is shown to consist of a core of trachyte, passing locally to phonolite, and covered by a thick mantle of basalt which extends far beyond the boundaries of the trachyte mass. The different varieties of these rocks are described and analysed with the commendable detail characteristic of American petrography. Small flows of analcite-basalt occur in this locality, but these are described more fully in another publication (*Jour. Geol.* 1914, **22**, 742). These rocks were formerly described as leucite-basalt, but the presence of analcite is fully demonstrated by chemical analysis. This raises the question whether " leucite-basalts " described from certain soda-rich petrographical provinces (*e.g.* Bohemia) are not really analcite-basalts.

In the same journal J. D. Mackenzie (*Amer. Jour. Sci.* 1915 (4), **39**, 571), defends his interpretation of the analcite occurring in certain volcanic rocks of Alberta as of primary origin. It has been suggested that this analcite, which occurs in lavas of trachytic type as phenocrysts ranging up to one inch in diameter, is derived from leucite by the action of soda-bearing solutions. This reaction, which certainly takes place under laboratory conditions, involves a 10 per cent. expansion of volume, of which there is no trace in the rocks in question.

The intrusive rock of Marston Jabet, Nuneaton, is described by A. Brammall (*Geol. Mag.* 1915, (6), **2**, 152), as a camptonite. Its lamprophyric character has been recognised by several authors, but the rock still awaits a complete chemical analysis to make its systematic position certain. A supplement to this paper (*ibid.* 224) describes the suspected occurrence of embryonic chiastolite as a result of the contact-metamorphism of shales by the Marston Jabet intrusion. Chiastolite is regarded as a distinct mineral species, and not as a variety of andalusite. This conclusion, however, is combated by R. H. Rastall (*ibid.* 336).

The above-mentioned Memoir No. 56 of the Geological Survey of Canada on the Geology of the Franklin Mining Camp, British Columbia, contains an interesting chapter on the Tertiary alkaline igneous rocks of that area. These include augite- and melanite-syenite, with a melanocratic facies described as shonkinite-pyroxenite, and various related dykes and volcanic rocks. They belong to a petrographic province which includes the district of Rossland and other isolated areas in British Columbia, and exhibits a series of rocks very similar to those of the Central Montana petrographical province over the United States border.

Petrology-Sediments.—The box-stones of East Anglia have been studied from a petrological point of view by P. G. H. Boswell (*Geol. Mag.* 1915 (6), **2**, 250). They are phosphatic sandstones with a rich suite of "heavy minerals," of which garnet, andalusite, staurolite, epidote, muscovite, and kyanite, are the most abundant. These indicate derivation from an area of metamorphic rocks adjacent to a crystalline massif. The angularity, large size, and absence of grading in size of the grains, suggests that they have not travelled far. The nearest area answering the above description is in the Ardennes.

The microscopic investigation of coal is reaching a high degree of refinement in the hands of certain American investigators, and E. C. Jeffrey (*Jour. Geol.* **23**, 218) is able to reach definite conclusions as to its origin from the structures revealed by the aid of the microscope. From the examination of numerous coals from all parts of the earth, and of all geological ages, he has arrived at the conclusion that coal is not due to accumulation by growth *in situ*, but to "the agelong gradual accumulation of vegetable matter in open water. In other words coal is not a compost heap, but a sedimentary deposit." The work is illustrated by a beautiful series of photo-micrographs of coal structures.

Economic Geology.—R. B. Dowling (Mem. 59, 1915, *Geol. Surv. of Canada*) describes the coal-fields and coal resources of Canada. The great Dominion contains by far the largest coal reserve in the Empire, but most of it is brown coal and lignite, and some is not easily available for commerce. In another portion of the Empire, J. J. Garrard (*Trans. Geol. Soc. South Africa*, **17**, 75) describes the geology of the Swaziland coal-field, which embraces 700 square miles of outcropping measures,

and some 1,200 square miles of available coal. The Swaziland coal is partly semi-bituminous, and partly excellent anthracite.

In the same publication A. E. V. Zealley describes the numerous chromite deposits of Selukwe, Rhodesia. These are found in talcose schist and silicified serpentine. In spite of the alteration of the enclosing rocks and the evidence of secondary deposition of at least some of the chromite, Mr. Zealley is inclined to ascribe the origin of the ore-bodies to magmatic differentiation of chromite from an original ultrabasic magma.

The relation of ore-deposits to igneous rocks receives consideration in two papers in *Economic Geology*, Feb.—Mar., 1915. From the petrological side L. V. Pirsson discusses the origin of the copper, lead, and zinc ores, found in regions of practically level sedimentary rocks, such as the State of Missouri. Geologists have been reluctant to accept an igneous origin for these ores on account of their remoteness from any superficial signs of igneous activity. Prof. Pirsson holds that they may have been deposited from the volatile constituents of igneous magmas, which, instead of explosively making their way to the surface as a volcano, have worked their way quietly upward through the strata, and have been largely absorbed by meteoric waters.

B. S. Butler discusses the relations of ore deposits to different types of intrusive bodies in Utah. The intrusions consist of laccolites and stocks, of which the latter are subdivided into those which have been truncated by the present plane of denudation near their apex, and those truncated at greater depth. The ore deposits associated with the apically-truncated stocks are of much greater value than those associated with the laccolites and deeply-truncated stocks. This fact is explained by the process of differentiation having concentrated the lighter mobile and volatile constituents of the magmas, which carry the metals and sulphur in solution, near the apices of stocks, thus forming valuable metalliferous deposits. In the deeply-truncated stocks these deposits have been eroded away; whilst in the laccolites the amount of magmatic material was too small, and the differentiation too incomplete, to furnish large ore deposits.

The Geological Survey of the United Kingdom has published a Handbook to the Collection of Kaolin, China-clay, and China-stone in the Museum of Practical Geology, by J. A.

Howe. In spite of its unpromising title this is an excellent and exhaustive memoir on the distribution, origin, and economics of kaolin, and is enriched by a valuable appendix on microscopical methods of investigating this substance by Allan B. Dick.

BOTANY. By F. CAVERS, D.Sc., A.R.C.Sc.

Plant Physiology.—Considerable progress has recently been made in the investigation of the oxidising ferments or oxidases of plants, and it appears probable that the relation of these to reducing ferments (reductases) and of both to respiration will prove to be essentially the same as in animals. In a series of papers Atkins (*Sci. Proc. Roy. Soc. Dublin*, **15**) has reported the occurrence of oxidases and of inhibiting bodies in a large number of plants, but two groups of tissues are apparently free from oxidases—tissues markedly acid in reaction and tissues containing large amounts of reducing substances. Reed (*Bot. Gaz.* **59**) points out that if oxidases play the essential rôle in respiration which has been attributed to them they must be present in all living cells, and he has succeeded in demonstrating their presence in a considerable number of algæ, as well as in acid tissues, by using more delicate methods of detection and by testing acid tissues in such a way that the acid juice and the ferment were kept apart. Ewart (*Brit. Assoc. Rep.*) points out that such terms as peroxidase, catalase, tyrosinase, applied to oxidase enzymes, are objectionable since they indicate only one of the many reactions of these bodies; also that the distinction usually drawn between the oxidase and peroxidase classes of ferments is unjustifiable, their supposed fractional precipitation with alcohol being merely the result of attenuation. He finds that organic oxidases are proteins, with or without metals in basic or acid combination, and that organic oxidases, like inorganic ones, vary only in degree of strength—the strong will cause direct oxidation from the oxygen dissolved in aqueous solution, the weak will only transfer oxygen from labile oxygen compounds such as hydrogen peroxide or will use dissolved oxygen only in presence of sensitisers like sodium chloride. He suggests that the sodium chloride always present in plant-ash may not be a useless constituent, but may exert a stimulatory or controlling action on metabolism in connexion with special oxidation or

with respiration in general. With reference to these papers on plant oxidases, it may be remarked that some of the botanical workers fail to keep themselves informed of the results which are being obtained by workers on the corresponding substances in animal tissues—an unfortunate circumstance in the case of problems which are obviously to a large extent common ground in biology. An interesting contribution to a closely related problem—the connection between light and respiration—has been made by Spoehr (*Bot. Gat.* **59**). It has long been known that respiration (both in plants and animals), as indicated by the liberation of carbon dioxide, is increased by exposure to light, other conditions being equal, and more recently it has been found that respiratory activity is higher during the daytime than during the night in the case of plants kept at constant temperature and in darkness—that is, when the only variable external condition to which the plants are exposed is the air of daytime on one hand, and that of the night on the other. Spoehr made careful determinations of the day-night ratio with normal and deionised atmospheric air, and his results indicate that, as might be expected, there is a definite relation between respiratory activity and the ionisation of the air due to sunlight. In discussing this very remarkable indirect relation between light and respiration, Spoehr points out that various lines of work seem to lead to the conclusion that the accelerating influence of light and heat on biological oxidative processes may be explained by the necessity for the dissociation or liberation of free valencies in the oxygen molecule—that is, it is in all probability the partially dissociated oxygen that combines with the oxidisable substance in such processes. This opens up a very interesting field for further work, and suggests the probability, for instance, that the respiratory activity of plants in arid regions may (other conditions remaining equal) be higher and show greater day and night variations than that of those in lowlying and moist situations, since atmospheric ionisation is decreased greatly by water vapour and high relative humidity. Shive (*Amer. Journ. Bot.* **2**) points out that since the general problem of the mineral requirements of plants largely remains still to be studied, and since nutrient solutions will have to be used in experiments bearing upon this problem, owing to the harmful action of distilled water on plants, it is important that the standard solution used as a basis of

comparison be as simple as possible and also that it produce good growth of the plants. He has made preliminary trials with a three-salt solution (monopotassium phosphate, calcium nitrate and magnesium sulphate, with a trace of ferric phosphate), which has given better results than the four-salt solutions employed by Knop and by Tottenham.

Ecology.—Much of the physiological work published nowadays is so intimately related to the ecology of plants as to be almost or quite inseparable from that branch of botany, which is making rapid strides and becoming something vastly different from the perfunctory recording of plant communities which passed for plant-ecology only a few years ago. Livingston (*Plant World*, **18**), has contributed a comprehensive and useful paper on the methods and results of measurement of the evaporating power of the air—strictly speaking a branch of meteorology, but one which has been chiefly developed by plant-ecologists, and which promises results of the utmost importance in the study of plant distribution. The same author, with collaborators, has dealt (*Carnegie Inst. Publ.* **204**), in a similar manner with the water-relation between plant and soil, and with the water-supplying power of the soil as indicated by osmometers ; this double memoir and the *Plant World* paper are of the greatest value to ecologists. Shreve (*Carnegie Inst. Publ.* **199**) gives an elaborate account, with very fine collotype plates, of the vegetation of the mountain rain-forests of Jamaica, including, among many other interesting matters, some experimental data which appear to show that hydathodes (water-glands), drip-tips, and other supposedly adaptational features of tropical rain-forest plants fail entirely to function in the manner attributed to them by writers who have given teleological explanations unsupported by experimental evidence. Miss Delf (*Journ. Ecol.* **3**), in an extremely useful critical discussion of the structural and physiological ideas expressed by the term " xerophily " (adaptation of plants to inadequate water-supply or excessive water-loss), deals in a very similar manner with the widely prevalent but largely false views which have gained vogue through hasty generalisation and facile interpretation of function in terms of structure ; her paper is extraordinarily useful and brings together a large amount of recent work in plant physiology and. ecology focussing on the problem dealt with. That British botanists are doing

their share in the young but progressive science of plant ecology is evidenced by the publication in the same journal (June 1915), of one of the best papers we have yet seen dealing with the ecology of a single plant species—that by Jefferies on the purple heath-grass (*Molinia cærulea*), which goes far to solve, as the result of patient field and laboratory work, the puzzling problem presented by the distribution of this plant; while Marsh contributes an elaborate account of the successions of maritime plant communities as developed at Holme (Norfolk) and worked out by members of the Cambridge Botany School. Fritsch and Salisbury (*New Phytol.* **14**) give a further account of the vegetation of Hindhead Common, resulting from similar co-operative work by members of the Botanical Department of East London College, and dealing mainly with the interesting succession phases observed in the regeneration of heath areas after fires have more or less completely destroyed the previously existing vegetation.

Flowerless Plants.—Lady Browne (*Ann. Bot.* **29**) has contributed a second paper on the anatomy of the cone and fertile stem of *Equisetum*; one of the plates accompanying this elaborate memoir must surely be the largest line-block ever published in a botanical paper, having been prepared from a drawing of a model, standing about six feet high, constructed from extensive series of sections, and showing the vascular system of the horsetail cone with remarkable clearness. The fossil genus *Bothrodendron* is dealt with in the same journal by Miss Lindsey, who brings forward further evidence in favour of the view that the peculiar stem-markings found here and in the allied *Lepidodendron* are the scars of fallen branches which were cut off by an absciss-layer like that formed at the bases of leaves which drop in autumn. The primitive fern family Ophioglossaceæ is by way of becoming one of the most exhaustively known plant groups, for on the heels of Lang's elaborate account of the anatomy of all three genera (referred to in *Science Progress*, **10**, p. 135), we have a paper by Petry (*Bot. Gaz.* **59**), on branching of the rhizome in *Ophioglossum* and *Botrychium*; the fact that in the former genus branching is by forking, while in the latter (as also in the third genus, *Helminthostachys*) axillary buds are regularly present, is taken as emphasising the close relationship of the two latter genera, and the differences between them on one hand and *Ophioglossum*

on the other. The special interest of Petry's results lies in his very reasonable suggestion that *Botrychium* and *Helminthostachys* have been derived from an ancestor which branched freely, and in the strengthening of the evidence pointing to a close relationship of this isolated living fern family to the most primitive extinct ferns.

Flowering Plants.—From the extensive literature dealing with the flowering plants we select, as being of evolutionary rather than merely systematic or geographical interest, the following contributions. Two interesting papers, both of the nature of preliminary notes, deal with the morphology of the remarkable genus *Gnetum*. Pearson (*Journ. Linn. Soc. Bot.* **43**) describes various new features observed in the flower which lead him to believe that the primitive form was hermaphrodite; that the flower-envelope has arisen by specialisation of a barren leaf structure; that the antherophore is of leaf origin and has been formed by fusion of two filaments; and that the primary endosperm of *Gnetum* is entirely homologous with that in the allied Welwitschia and cannot be regarded as a true prothallus, but is a new structure not found in other plants. To this "new morphological entity, neither sporophytic nor gametophytic," Pearson gives the name "trophophyte" and suggests that the endosperm of Angiosperms may be a highly specialised form of this trophophyte. Thompson (*Amer. Journ. Bot.* **2**) has also studied the embryology of several species of *Gnetum*, and he too finds evidence pointing to a much closer relationship between Gnetales and Angiosperms than has hitherto been obtained. He states that the pollen-grains may germinate well up in the so-called "style," the pollen-tubes growing down to the nucellus as in Angiosperms; that no male prothallial cells are produced; that only free nuclei (never cells) are formed in the embryo-sac before the tubes enter (their entry being also immediately preceded by the organisation of one or more eggs); that before fertilisation the female gametophyte becomes divided into numerous multinucleate compartments in each of which all the nuclei then fuse; and that the endosperm is formed by divisions of the fusion nuclei in the lowest compartments. The results of these two important papers will certainly figure largely in future discussions regarding the origin of the Angiosperms. A long paper by Miss Sargant and Mrs. Arber (*Ann. Bot.* **29**), deals with the morphology of the

embryo and seedling of grasses, which appears likely to open up new lines of work on the evolution of the Monocotyledons from a lily-like type derived in turn from some ancestral buttercup-like type; these authors find that, although the complicated anatomy of the grass seedling renders comparison with the seedlings of other Monocotyledons difficult, all the variations found can be derived from a hypothetical type the vascular skeleton of which is that of a hypogeal Monocotyledon showing remarkable similarity to that of the Zingiberaceæ. The paper is illustrated by a large number of remarkably clear figures which, together with the skilful manner of presenting the facts, makes it easy to follow the somewhat intricate details described. In two papers published almost simultaneously Willis (*Ann. Bot.* **29**, *Journ. Linn. Soc. Bot.* **43**) deals with various structural and biological features of the Podostemaceæ, a remarkable family of tropical aquatic plants in which the functions of stem and leaves are assumed by the roots; the author proposes the separation of certain genera as a new family (Tristichaceæ).

ZOOLOGY. By C. H. O'Donoghue, D.Sc., University College, London.

Protozoa.—The life histories of a number of protozoan forms both free-living and parasitic are dealt with in various papers. Heron-Allen records a long series of investigations into the reproductive processes and the Economics of some of the Foraminifera that have occupied some years (*Phil. Trans. Roy. Soc. B.* June 1915). Together with Earland he also publishes the second part of a large monograph on the Foraminifera from the Kerimba Archipelago in Portuguese East Africa in which thirty-two new species are treated (*Trans. Zool. Soc.* April, 1915). Pixell-Goodrich has reinvestigated the life history of sporozoa living in Spatangoids, named *Lithocystis schneideri*, and has identified five separate species (*Quart. Jour. Micro. Sci.* vol. lxi., May 1915), and also shown that *Minchinia chitonis*, a parasite of the mollusc chiton, is undoubtedly a Haplosporidian, and not a coccidian as was previously thought (*Proc. Zool. Soc.* May 1915). Mackinnon has added a third part to her studies on parasitic protozoa concerning *Embadomonas* obtained from trichopterous larvæ and a Trichomastigine multiplication cyst (*Quart. Jour. Micro. Sci.* vol. lxi. May 1915). The parasites of mice form the subjects of two papers.

Stevenson adds substantially to our knowledge of *Klossiella muris* (*Quart. Jour. Micro. Sci.* vol. lxi. May 1915) and Fantham and Porter record the natural occurrence of Herpetomonads (Leptomonads) in these animals (*Parasitology*, vol. viii. No. 1, 1915). Although *leucocytozoon anatis* is found in all the ducks suffering from a new disease, Wickwain has not been able to establish definitely that they are the actual cause of it, but is continuing the inquiry (*Parasitology*, vol. viii. No. 1, 1915).

Invertebrata.—While searching for examples of *Calliobdella lophii*, Sharpe had sent to him a worm from the Orkney Isles which proved to be *Ganemedes cratere* n.g. et s. (*Parasitology*, vol. v. No. 1, 1915). Nicoll has described a number of parasitic Trematodes belonging to new species and new genera that he obtained in looking through a collection of North Queensland fishes (*Parasitology*, vol. viii. No. 1, 1915). A new species of tapeworm from the Parakeet *Brotogerys tirica* is noted by Meggitt (*ibid.*) and avian sestodes also form the basis of a contribution by Beddard (*Proc. Zool. Soc.* May 1915).

The Asterids are divided by some authorities into two groups, the Phanerozonia and the Cryptozonia and Gemmil gives a detailed account of the early development and larval forms of the starfish *Porania pulvillus*, with a discussion of its bearing on this division (*Quart. Jour. Micro. Sci.* vol. lxi. pt. 1, May 1915). He further discusses the occurrence and the cause of a double hydrocœle that is to be found in certain of the larvæ of *Asterias rubens* and their changes during development and metamorphosis (*ibid.*).

The Arthropoda again come in for a number of papers. Meek has an account of the Mitotic spindles in the spermatocytes of *Forficula auricularia* which continues and to some extent modifies his previous measurements on the mitotic spindle, and the deductions to be drawn from them (*Quart. Jour. Micro. Sci.* vol. lxi. pt. 1, May 1915). Robinson has examined the tick *Amblyomma herbracium*, and the variability in its size (*Parasitology*, vol. viii. No. 1, 1915). Two interesting papers by Harrison deal with the Mallophaga. In the first those obtained from Apteryx, with their relationships, are enumerated, and it would appear that closely allied species of birds are infested with closely related parasites. The second gives a general account of the respiratory system in the group, and the value

of its variations from the taxonomic point of view (*Parasitology*, vol. viii. No. 1, 1915).

Two further notes on descriptions and records of bees add to the already long series that have been made by Cockerell (*Ann. and Mag. Nat. Hist.* April and June 1915). Smith has a paper on the genera *Eglisia, Callostracum, Mesalia, Turritellopsis* and *Trachyrhynchus* (*Ann. and Mag. Nat. Hist.* April 1915). The gill-chamber of land-crabs appears to furnish a convenient resting-place for a number of different parasites that have been described by Bayliss, and include, strangely enough, an Oligochæte (*Ann. and Mag. Nat. Hist.* April 1915). Two papers concern new species of Coleoptera ; one by Arrow deals with those belonging to the Dermestidæ from the collection in the British Museum together with notes on that family (*Ann. and Mag. Nat. Hist.* May 1915) ; the other, by Lee, describes certain Malacodermidæ and Curculionidæ that were obtained by Mr. Bryant in Australia (*ibid.*). In the same publication Bergroth catalogues new Oriental Pentatomoidea, Distant some Rhynchota hitherto undescribed, and Schaus gives a long list of corrections in the nomenclature of Heterocera from Costa Rica that have previously appeared in the *Annals and Magazine of Natural History*. Bagnall treats of a small collection of Symphyla from Algeria (*Ann. and Mag. Nat. Hist.* May 1915), and of new Thysanoptera (*Ann. and Mag. Nat. Hist.* June 1915). Fossil Arthropoda are to be found in notes on fossorial Hymenoptera by Turner (*ibid.*) and also in a note on British fossil species of Woodlice, *Apodemus*, by Hinton (*ibid.*). The holotype of *Nymphon gracilipes*, one of the Pycnogonida, is ably discussed by Calman (*ibid.*). Couper's Snake in the Zoological Society's Gardens has yielded a new mite described by Hirst and placed in the genus Ichoronyssus, and it is probably to be regarded as a primitive form since the female markedly resembles the protonymph stage in other members of the genus (*Proc. Zool. Soc.* May 1915). The spiders collected in Dutch New Guinea by recent expeditions, including a new genus and new species, are recorded by Hogg (*ibid.*).

Vertebrata.—Moodie has written an account of recent studies in fossil Amphibia which provides a very useful resumé of practically all the work in this field for some years past, together with a certain amount of criticism on it (*American Naturalist*, June 1915). Nichols has shown that in Anurous

Amphibia the neural canal does not end blindly in the urostyle, but comes through on to the dorsal side (*Proc. Zool. Soc.* June[1] 1915). Two new tree-frogs from Sierra Leone are described by E. G. Boulenger (*Proc. Zool. Soc.* April 1915). G. A. Boulenger gives an account of all the known snakes from Madagascar, Comoro, Mascarenes, and Seychelles, where no terrestrial form is dangerously poisonous to man (*Proc. Zool. Soc.* May 1915), and also another account of those from the Belgian and Portuguese Congo, Northern Rhodesia, and Angola, in which two new forms are included (*Proc. Zool. Soc.* April 1915). The anatomy of certain Gruiform birds is dealt with by Mitchell (*Proc. Zool. Soc.* May 1915). Two other papers deal with birds; in the first Blakeslee and Warner discuss correlation between activity in egg-laying and the presence of yellow pigment in domestic fowls (*American Naturalist,* June 1915); in the second Haig-Thomas gives the numbers of dark-necked and ringed male peasants shot in two successive seasons, and claims that they indicate continual Mendelism in this character in hybrid birds (*Proc. Zool. Soc.* April 1915). Broom has published a very interesting series of papers, some palæontological and some anatomical. Of the former one describes a number of carnivorous Therapsids from the British Museum whose small size and state of preservation had not allowed of their earlier recognition (*Proc. Zool. Soc.* April 1915); another discusses the Anomodont genera *Pristerodon* and *Tropidostoma* (*Proc. Zool. Soc.* May 1915); and the third treats of certain Triassic Stegocephalians (*Proc. Zool. Soc.* June 1915). The two anatomical papers give a detailed account of the organ of Jacobson in the Insectivora, and show that considerable differences in its relations are to be found. Part I. concerns *Tupaia, Macroscelides,* and *Gymnura* (*Proc. Zool. Soc.* April 1915), and Part II. *Talpa, Centetes,* and *Chrysochloris* (*ibid.* May 1915). A new genus and species of fossil mammal related to *Æluropus* have been obtained from Burma and recorded by Woodward (*Proc. Zool. Soc.* June 1915). Thomas has a long series of papers relating to mammals in the *Annals and Magazine of Natural History,* viz.:—The "baculum," or penis bone, can be used as a guide in classifying certain squirrels; descriptions are given of (*a*) the various geographical

[1] With this one exception the month given is that in which the paper was read before the Zoological Society, and not the month of publication.

races of *Citellus fulvus*, and (*b*) Pteropine bats from islands off the north-east coast of Guinea (April 1915); an account of a small shrew from Lake Baikal and notes on the genus *Nyctophilus* (May 1915). Notes on bats of the genus *Coleura*, and a description of three new bats that were obtained in the Sudan by Mr. Willoughby (June 1915). The African Shrews of the genus *Crocidura* are dealt with by Dollman in two parts, Part I. (*Ann. and Mag. Nat. Hist.* May 1915), and Part II. (*ibid.* June). The structure of the skull and skeleton of a peculiarly modified Rupicaprine antelope, *Myotragus balearicus*, is described by Andrews, who also notices a new variety of it, *M. balearicus var. major* (*Phil. Trans. Roy. Soc.* June 1915). Pocock draws attention to certain external features of the marsupial *Cynogale bennetti* (*Ann. and Mag. Nat. Hist.* April 1915), and also deals with the external features, including the feet and scent-gland of Viverrids belonging to the genera *Paradoxurus*, *Arctogalidia*, *Arctictis*, and *Nandinia* (*Proc. Zool. Soc.* June 1915). Ñato cattle of the Argentine are now extinct, but luckily Gibson, who probably possessed the last survivors, has been able to give an account of them (*Proc. Zool. Soc.* April 1915).

A number of fairly important papers on mammalia fall in the period under review. The acquisition of a further specimen of an early stage in the development of Ornithorhynchus gives Wilson and Will an opportunity of reconsidering, in the light of subsequent criticism, the interpretation they had given of the embryonic area and " primitive knot " in the Monotreme egg (*Quart. Jour. Micro. Sci.* vol. lxi. pt. 1, 1915). Frazer gives a detailed account of the origin of the eye-muscles and head cavities in the marsupials. It is based mainly on *Trichosurus vulpecula.* A retractor bulbi muscle is present in this class, and although in the sub-class Diprotodontia a large pre-mandibular head-cavity is to be found, it appears to be small or absent in the Polyprotodontia (*Proc. Zool. Soc.* May 1915). The marsupials, this time mainly *Perameles*, furnish the material for an investigation on the early stges of the heart and anterior blood-vessels by Parker. The development is, on the whole, somewhat like that in the Eutheria (*Proc. Zool. Soc.* June 1915).

A paper investigating the relation of spermatozoa to Electrolytes, by Gray, although somewhat physiological, is of

interest to the zoologist because of its bearing on the problem of fertilisation. (*Quart. Jour. Micro. Sci.* vol. lxi. pt 1, 1915). Morgan has conducted a series of experiments and observations on the infertility exhibited by the females of *Drosophila ampelophila* that have rudimentary wings, and these he places on record with his deductions (*American Naturalist*, April 1915). One further contribution is to be noticed, although perhaps not to be considered an advance in the sense of the addition of new facts, and that is the presidential address to the Palæontological Society of America by Osborn. In it he discusses with great lucidity the available facts relating to the origin of single characters, and draws his data from animals and plants both living and fossil—a subject of great interest (*American Naturalist*, April 1915).

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc., Public Museum, Gloucester.

IT is now fairly generally known that geology has revealed the existence in the past of four distinct species of creatures who may be styled sub-human, and who bear much the same relationship to real man that the gibbons and the orang bear to the chimpanzee. There is now a fifth claimant to that position. The remains of this animal have been found in India, and they are described in great detail by Dr. Guy E. Pilgrim of the Geological Survey of India, in a paper entitled "New Siwalik Primates and their Bearing on the Question of the Evolution of Man and the Anthropoidea" (*Records of the Geological Survey of India*, vol. 45, pt. 1, 1915). The discovery is comparable in importance with that of the famous Heidelberg jaw. Pilgrim has named this new species—which also represents a new genus—*Sivapithecus indicus*, and the paper mentioned also contains descriptions of some fossil monkeys and true apes, of which several are new to science. The relics of *Sivapithecus* were found in the Nagri and Chinji zones of the Lower Siwalik Strata, which correspond to the Middle Miocene of Europe, and the bones discovered are as follows: (1) part of the right side of a mandible containing all the molars and premolars (although the first premolar and the last molar are not perfect), and part of the alveolus of the canine, but with the ascending ramus missing; (2) an isolated last right lower molar; (3) a fragment of the left side of a mandible

showing the symphysis and the canine and the roots of two incisors and of the front premolar; (4) a front left lower premolar; (5) another isolated lower molar. There are also two more molars and an upper canine which may possibly belong to the same genus. It will be seen that the specimens fortunately supplement one another somewhat, but they were not all found together, and in fact came, as already stated, from two different zones. The author gives a restoration of the entire mandible, but, since the ascending ramus was lost, this is largely imaginary. The chief features of this most important fossil may be briefly summarised. The third molars are very well developed and are longer than the second molars; this is a distinction from the modern apes, in which the last molars are degenerate, as in man. The premolars are well developed and are not human. A most peculiar conformation is to be seen, however, in the premolar portion of the jaw. The rami bend outwards here, so that the premolars are slightly out of line with the molars and are much outside the canines, the front of the jaw thus being shortened and widened. The author thinks that this curious widening of the jaw is an advance in the human direction. The canine is large and has a posterior cusp, resembling the same structure in the gibbon. The symphysis is very short, the backward slope of its posterior surface is very slight, and there is "an entire absence of any shelf." This is in marked contrast to the structure of the famous Piltdown mandible, which created so great a stir in 1913. The Piltdown jaw has a long symphysis, like that of the living apes, and even the Heidelberg jaw has a more extensive symphysis than *Sivapithecus*. The paper ends with a closely reasoned discussion of the evolution of the Hominidæ and the Simiidæ, and the author claims that *Sivapithecus* should be placed in the former family rather than in the latter, on the ground that the short symphysis and the widening of the anterior part of the jaw are human features. He thinks that *Homo sapiens* is probably directly descended from some species of *Sivapithecus*, though not from *S. indicus*, and that the line leading to the Piltdown, Heidelberg, and Neandertal species has been quite separate since the early Miocene, the peculiarly human characters of those species being due to convergence. The doctrine of polygeny is thus adopted in a very extreme form.

Without criticising this important paper in any detail, it may be remarked that since the most ancient known ape, the Oligocene *Propliopithecus*, possessed a short symphysis, this is evidently merely a primitive feature, which may well have been secondarily acquired by *H. sapiens*, just as Dr. Pilgrim thinks it was secondarily acquired by *H. neandertalensis*, and the line to *H. sapiens* may well have travelled further with the other Hominidæ and even with the apes than he believes. The reasons for including *Sivapithecus* in the Hominidæ do not appear very strong.

The above paper should be read in conjunction with one by Dr. A. S. Woodward on a Spanish specimen of *Dryopithecus fontani* (*Quarterly Journal of the Geological Society*, vol. 70, p. 316, 1914).

Of the short articles which appear in *Man* for the second quarter of the present year, perhaps one by Prof. Giuffrida-Ruggeri in the April number, entitled "Were the Pre-Dynastic Egyptians Libyans or Ethiopians?" will excite most general interest; and a further article on the same subject by Prof. G. Elliot Smith appears in the May number. The latter number also contains an "Ethnographical Sketch of Fiji," by A. M. Hocart, which should prove very useful to any-one making an anthropological study of those islands.

NOTES

Mr. Lloyd George, the Nation of Shopkeepers, and the Pied Piper of Hamelin.

The terrible but unproductive outbreak of war has interrupted most of the developments which were proceeding in the world before it happened. Perhaps this is just as well in the case of many things; but there are some movements which must not be allowed to languish, and among these is the question (which was receiving quite an unprecedented amount of attention in Britain early last year) of the position of science in this country. The public was almost commencing to understand that all is not well with science in Britain, and was even beginning to think of applying some remedy. Among the efforts made in this direction one of the most important was a step taken by Sir Ronald Ross, as first described by him in the *British Medical Journal* for the information of members of his profession. After consultation with distinguished Parliamentary lawyers, he determined to petition Parliament for compensation for his work in connection with tropical medicine. This petition was based upon the precedent of Edward Jenner, who was given on two occasions in the early part of last century a sum totalling thirty thousand pounds for his work on vaccination. By the rules of procedure of the House of Commons, monetary petitions cannot be considered by the House unless they are first recommended by the Crown—which means that such petitions must be allowed to proceed by the Chancellor of the Exchequer. Accordingly, as he has said, Sir Ronald Ross's petition was drawn up in proper form by the lawyers, and was submitted in due course to Mr. Lloyd George in November, 1913. It gave a detailed account of his work, and the Chancellor of the Exchequer was asked for his recommendation. In reply, Mr. Lloyd George's private secretary said that "notwithstanding the precedents to which you refer, it is not in accordance with modern usage for a petition of this character to be recommended to the House of Commons by the Crown on

the advice of a Minister. In these circumstances he fears that he is unable to accede to your wishes. I am to add that the Chancellor of the Exchequer has no funds at his disposal for grants in respect of eminent scientific services." Following upon this Sir Ronald Ross wrote a second letter to the Chancellor of the Exchequer asking for a reconsideration of the question, and pointing out that there was no other constitutional means by which he could obtain the compensation required; but the Chancellor replied that he remained unable to depart from the decision previously conveyed to the petitioner.[1] In July, 1914, a question was asked on the subject in the House of Commons by Mr. W. H. Cowan; and the Chancellor of the Exchequer replied that he had seen the petition referred to, "but did not feel justified in taking the course suggested, which is not in accordance with modern usage." A few days later, Mr. Cowan asked the Prime Minister "whether there is any existing fund available out of which men of science may be compensated for losses incurred by them in doing unremunerative scientific work when such work has proved to be of advantage to His Majesty's Government and subjects; and whether, if no such fund is available, he will consider the advisability of providing funds for meeting such cases in the future?" To this the Prime Minister replied that he was not aware of any fund other than the Civil List, and that he was not satisfied that further provision is necessary. Mr. Cowan asked "whether it is not the case that Civil List pensions are in the nature of charity and not a reward for services." To this Mr. Asquith answered, "No, sir, I never regarded them as in the nature of charity. They are a reward for services rendered by deceased persons who have left their families in necessitous circumstances." What else is this but charity? At the end of July, just before the outbreak of war, the Annual Representative Meeting of the British Medical Association resolved: "That in the opinion of the Representative Body of the British Medical Association the petition of Sir Ronald Ross, K.C.B., F.R.S., to the House of Commons for remuneration for his services to the Empire, in having discovered how malarial fever can be prevented, should be granted." And this resolution was immediately forwarded to the Chancellor of the Exchequer with an expression of opinion, which was

[1] Copies of the petition and correspondence can be had on prepayment of postage from the Secretary, SCIENCE PROGRESS.

strongly voiced in the meeting, that not only should this personal petition be granted, but that the case showed the great necessity for the institution of a State fund out of which grants could be made in similar cases. Apparently nothing but a formal acknowledgment of this resolution was received by the British Medical Association.

Of course the matter is by no means closed, and it would be a great pity for the sake of science in general if it were to be allowed to drop. The petition is in perfect order, and is backed by the strongest of precedents; and the legal opinion is that Mr. Lloyd George has no justification for suggesting that it is in any way out of order or date. As Mr. Asquith said, the Civil List pensions are paid to the relatives of deceased persons who have done good service to the State—quite another matter; and these pensions are at best nothing but the most miserable little charitable doles which serve only to prove the ingratitude of the nation towards persons who have benefited it in the higher lines of effort.

Those who are able to read between the lines of the petition and of the covering documents will find no difficulty in detecting that the petitioner's real motive was directed towards the general betterment of science in this country. In fact, there is not a little element of sardonic humour in the attempt. It is an effort to make the British nation remember that even great peoples have certain debts of honour which they should not forget. But the issue of the experiment has up to the present demonstrated that the British nation in general and Mr. Lloyd George in particular do not quite clearly appreciate this little point. Let us examine it further.

Since the beginning of time the public has been fortunate enough to have most of its really great work done for it for almost nothing—work in science, philosophy, poetry, literature, painting, sculpture, music, and indeed in all those directions which benefit humanity in general rather than the persons who undertake them. It may truthfully be said that we of to-day have been lifted above the savage by such efforts as these; and nations, like individuals, have the obligation to perform towards civilisation in general that they should encourage these supremely important labours. The British nation is not perhaps very much more lax in this respect than other nations have been, but, as a matter of fact, they, as a nation, have done almost

nothing in this direction during the whole of their history. Nearly all the great works of civilisation have been performed by individuals of whom many have been allowed to die either destitute or in great poverty or in comparative poverty. Men of science, poets, writers, artists, and musicians have the same story to tell. And yet that story does not appear at all disgraceful to the mass of humanity, and the fact is constantly dwelt upon as being quite a normal and natural phenomenon, if not an amusing one, which no nation need trouble itself about bettering. But the precedent of Jenner showed that Parliament and the whole British nation did at least once recognise its obligations in this respect. Jenner was indeed almost ruined by his magnificent discovery of vaccination. He was, as every one knows, a practising physician; but his work on vaccination brought him many enemies and, moreover, forced him to neglect his practice. At that time, however, the British were a rational and virile nation who easily understood the point, and therefore gave him due and honourable compensation. Since then they have scarcely ever done the same thing. They expend vast sums for the education of their young, but scarcely a penny for the encouragement of those labours which form the basis of the instruction which is given to their young. Yet, while they do nothing for their most meritorious workers, they allow every kind of trickster or charlatan to enrich himself enormously at the public expense. Great fortunes are made out of, let us say for example, patent medicines and many forms of "business" which are just a little upon the outside of dishonesty. These types of effort confer no advantage whatever upon the public, and yet often lead to the amassing of gigantic fortunes; so that it may almost be said that it pays more to be a rogue than to be a genius. But even apart from these merely evil pursuits, it can scarcely be maintained that many of the pursuits which bring wealth confer any great good upon any one but the persons who follow them. To define the absolute truth, therefore, the nation by its present policy encourages all kinds of mean work and discourages all kinds of the greatest work. Since the time of Jenner, Parliament itself has done nothing to remedy this state of affairs, but quite recently it has given itself the sum of considerably over a quarter of a million pounds a year. There are some who, on reviewing the work actually done by Parliament—the hasty

and immature legislation, the incessant quarrellings without which even this small modicum of result is not achieved, the degradation of the public mind by the low standards of party politics, and perhaps even the undoubted mismanagement which has been so largely responsible for the present war—will think that one-tenth of the sum which Parliament has taken to itself would have been ample payment for it. Perhaps the time will come when the historian reviewing the present period will decide that the work in science of a few humble individuals now living was of far greater value to humanity than the work of all those who have taken this little quarter of a million out of the public till. And yet our "Chancellor of the Exchequer has no funds at his disposal for grants in respect of eminent scientific services." What a criticism of the average mentality of the British public!

One of the most singular things about the mass of humanity is that, though they constantly read or hear the great lessons taught to them by their chief poets and seers, they never seem to take the lessons to themselves. Thus the sublime story which is read every Sunday in our churches, and which is largely based upon this very theme, never comes home to them. When Prometheus gave to humanity his discovery of fire, the only recompense which he received was the most dreadful of punishments. We also know well the story so graphically related by Browning. When the burghers of Hamelin were unable to get rid of the pest of rats which infested them, the Pied Piper arranged to do the work for the payment of a small fee. When, however, he had done the work, they refused him the fee; and the result was that he charmed away their children by exactly the same means as those by which he had charmed away their rats. Now we as a nation would look with righteous disgust at such conduct on the part of the burghers of Hamelin; but as a matter of fact this is precisely the conduct that we have adopted for centuries past towards almost all our leading benefactors. And Mr. Lloyd George has now decided that this shall be the declared policy of the British Empire towards all Pied Pipers in future!

The saying that we are a nation of shopkeepers is attributed to Napoleon. He implied by this not only that we kept shops (and shopkeepers are often excellent people), but that we have a certain national characteristic which, we suppose, did not com-

mend itself to him. We can imagine what would be the conduct of a man with this characteristic who has, let us say, made a large fortune out of the ideas of one of his employees. The latter comes to him and asks for some recompense. "Good gracious," replies the shopkeeper; "I did not ask you to do for me what you did; you yourself offered me your ideas; I took them and have had the capacity to use them. Before you gave your ideas to me you should have made a formal legal agreement with me as to what recompense you wanted. You did not do so, and therefore I now owe you nothing whatever. Here, however, is half a crown for you, which you may put by for the benefit of your children." That is precisely the attitude of the British nation towards those employees whose ideas have enabled it to become great; and we see it clearly in the detestable Civil List pensions by which we are trying to ease our consciences from the accusation of ingratitude which lies against us. Our benefactors have no legal claim against us; we therefore give them nothing; but out of the noble generosity of our spirit we preserve their wives and daughters from beggary by sparing them a few pounds a year to live upon. We once heard a Member of Parliament of the shopkeeper type declare that the Nobel Prizes are a shocking waste of money; and this man was a very wealthy person who had done nothing whatever for his country either in Parliament or out of it. We have also heard it said by satirists that Napoleon's aphorism is a just one, and that we have not only the occupation but the spirit and the manners of tradesmen, and that even many of our aristocracy (which is largely descended from the tradesmen of the past) possess the indefinable air of those magnificent persons in our "emporiums" who direct customers to the "gentleman in the next department." When George Meredith stated that we are the aristocracy of Europe, he was perhaps thinking of this type of man. But, after all, we are not much worse in this respect than other nations are, and the stories of Camoëns, Cervantes, Columbus, and a hundred others will easily be remembered.

The second part of the story of the Pied Piper contains a parable. The burghers of Hamelin suddenly lost their most cherished possessions for whom they had been toiling in their shops and who, they had fondly hoped, would have helped them in their old age. As a matter of fact, the revenge comes from no miracle but from the ordinary course of events, and the

punishment is certain and merited. How many great men have Spain and Portugal produced since the time of Cervantes and Camoëns? The reaction is silent but inevitable. It is more profitable to be a rogue than a genius. Now in the present case, tropical medicine is only a department of science, and perhaps not the most important or worst paid one, but SCIENCE PROGRESS of January 1914 gave some indication as to how it has been treated. The most scathing comment on the whole business may be found in a sentence of the Huxley Lecture recently delivered by Sir Ronald Ross, in which he says that in his opinion scarcely one-tenth of the work which might have been done to save human health and life in the tropics by means of recent researches has actually been performed. The same stupidity which governs our attitude towards the workers will certainly prevent us from utilising their work for our own benefit. That is the punishment which fate keeps in store for the instincts which the tradesman admires in himself but which the rest of the world condemns. And after all, the spirit of a nation is that of its individuals.

It may be answered that this moment when our soldiers are dying for their country is not the one when men of science should clamour for compensation. But it is precisely the moment. There is a close parallel between the cases of our sons who are toiling in the trenches of Flanders and our workers who are toiling in the trenches of science. The soldier is not allowed in this country to have a vote (disgracefully enough), and the men of science are too few to make their votes effective. So also with the other workers at great things. Therefore the politicians neglect them all. For years they have maintained an army too small for the needs of the country—though warned over and over again by the best military experts. Now, as a punishment, they are obliged to pour out the nation's money like water in order to retrieve their error, which, as many think, has been largely to blame for the war. In times of peace they gave their soldiers almost nothing compared with the wealth which any cheat may obtain in a few years by his mean efforts. Now, when war has broken out, they are obliged at the last moment to place themselves in the hands of a soldier and to shriek, clamour, and pay for the recruits which should have been obtained and trained long ago.

A nation must adopt one of two ideals—either to be a great

nation producing great work, men of high intellect and character, and men who prefer to serve the world rather than to serve themselves; or to be a nation of politicians, of persons who try to obtain wealth without labour, of those who look only to the main chance, of trumpery journalism, a contemptible stage, cinematograph shows, public-houses, silly processions, superstitions which call themselves religion, and streets full of untidy loafers with cigarettes in their mouths. We have not yet quite sunk to the latter level, but the efforts of our politicians before the war were perhaps largely tending in that direction. There is, however, a power above politicians which controls these affairs; and the evolution that has moulded man in the past will know which of these two ideals must be selected for the future.

The Board of Education's Scheme

Although the memorandum of the Board of Education outlining its plan for the organisation of scientific research was published as long ago as last July, we still desire to reserve our full comments upon it until we can learn more about the details. While warmly welcoming the movement (for which we have always appealed) and while wishing it every success, we feel it our duty to remark that it appears to contain no provision for instituting the best, and perhaps the only real, method for encouraging discovery as distinct from investigation—and the two things are quite different. *Prima facie*, the Board of Education's Scheme is one for the cultivation rather of petty science than of Science. It proposes to devote a Parliamentary grant of (we understand) £25,000 a year for the purpose, this being administered by a Committee of the Privy Council, consisting principally of the inevitable party politicians, advised by another committee of men of science. The money is to be expended upon instituting specific researches, developing existing institutions, and awarding research studentships. The last item we frankly object to, because, as frequently pointed out, they are mere baits to lure the young and inexperienced into paths which may appear flowery enough at first but which will in later life prove to lead into the deserts of scholastic poverty. No—we see in all this only a repetition of the old ideas, so popular in Britain, for creating discoverers on a salary of one or two hundred a year; for by the Scheme

it appears evident that none of the profits arising from "discoveries made by institutions, associations, bodies, or individuals in the course of researches aided by public money" are to go to the discoverers! What man then who is conscious of possessing genius or talent is likely to take such money? If he is wise he will prefer to work out his own ideas at his own expense, so that if there is profit in them, he himself will obtain it. As it stands, the Scheme appears to us to be rather like one for doling out loans to necessitous geniuses on a million per cent. usury. Some good results will probably be obtained, but we fear that most of the money will be wasted upon giving pifflers opportunities for writing pot-boilers, and (a little better) for providing instruments and small increments of salary to well-meaning but evident plodders. Great science is not made in this way. It requires immense labour, complete devotion, and, above all, an intellectual capacity which is not possessed by the vast majority of men—even of "scientists." But such men are not always "fools" in the worldly sense. They often perceive that though they may some day reach "Athene's gold," they may also die in the attempt. And so they turn away, and the ore remains concealed. Almost the whole problem of "the encouragement of science" lies in this—how to keep such men at their great task. They hear the cries of their children and abandon it. Who loses? The World; and there is no attempt in the Scheme to solve this problem.

The Sociological Society

The Sociological Society conferred a boon on many when it invited Mr. G. P. Gooch to read a paper on April 27, on "German Theories of the State." At the present time every one is speculating as to the real views of Germany, and such a lecture, obviously based on the result of half a life-time's study, goes far to raise the outlook of the public above the petty and narrow views which are continuously voiced in our daily papers, and gives some insight as to the point of view of nations other than our own. The lecturer sketched the growth of Germany's conception of the proper relation of the State to the individual, the individual to the State, and the attitude that one State should have to another, by presenting the opinions expressed by her thinkers of the past, such as Humboldt, Kant, Fichte, Hegel, Dahlmann, Stahl, and, in our own times, such men as Treitschke,

Delbrück, Prince Bülow, and Naumann. He showed how in the earlier period the State was considered as secondary to the individual, and, the conception of man's inherent goodness being high, government became in their eyes somewhat of a hindrance to the development of individuality, and was looked upon almost as a necessary evil. As these high ideals of mankind were found to be an unpractical basis for a workable constitution, the necessity of giving more authority to the State was recognised. But Hegel, who first realised this need, made the mistake of unduly exalting its majesty, placed the State in a kind of mystical position over everything, and absolved it from moral relations to its fellow States. From this time onward many of these writers, in adopting these concepts, failed to see that the State and Head of the State are after all but individuals; and the present catastrophe is therefore due to the error of carrying the idea of State power to excess and of dropping altogether the higher ideals of the past. It was Stahl who added to this error the pernicious theory of Divine Right, which Treitschke and Delbrück crystallised into the present system that crushes the claim for popular rights almost out of existence. Mr. Gooch seems to see in Naumann, the orator of the Freisinnig party, the germ of a new era which will restore to the people of Germany once more their rightful liberty.

In the discussion which followed, Prof. Graham Wallace's remarks are well worth recording. He said that, in these days of hate and bitterness of which we were already so heartily sick, we ought to be grateful to Mr. Gooch for giving us the chance of loving and admiring. He pointed out that Germany has suffered, and will suffer poignantly for all her mistakes; that she will feel the loss of her men and her trade very keenly; and that, if a real peace can come only when Germany thinks on different lines, then, when all this turmoil is over and done with it behoves us to wait lovingly and expectantly for her to return to her former ideals. That these thoughts struck a note which synchronised with the thoughts of the audience was abundantly attested by their loud applause. Up to the present, however, there is little sign that Prof. Wallace's prognostications will be verified.

Science Among the Nations

Prof. J. Arthur Thomson contributes an interesting article to *Knowledge* (May 1915) on the subject of German science, and

shows (what is evident) that it is by no means easy to make a reliable valuation of it compared with the science of other countries. He says: "What appears to be the truth is this, that each of the leading civilised nations has its fair share of scientific discoveries of first-rate importance, but that there is not sufficient evidence for correlating special fertility in scientific discovery with any nationality Speaking now, not of men of intellectual eminence but of real giants, we believe that the great discoverers represent individual mutations. In its finest expression the discovering spirit means a particular alertness, freshness, eagerness, insight, and cerebral potential—born not made. The spot of light which marks its emergence shifts from place to place, from nationality to nationality, from race to race, from university to university. . . . It is a rare spirit, sacred and inestimable, and moveth where it listeth, no one being able to tell whence it cometh or whither it goeth." True and fine words; but we must always remember that a large mass of science must be and is done by men who can only be described as of intellectual eminence. Science must not only take positions against nature, but must make those positions good, to use military language; must dig and strengthen the trenches and must bring up artillery and infantry in support. The fact is that quite a number of important discoveries have been made almost by accident. Oddly enough Prof. Thomson omits the name of Newton from his lists.

Brainless Britain

What should we think, we wonder, if we saw a number of grave and earnest men busily employed in a time of drought, some in tying up the drooping petals of faded flowers, and others in painting them to restore the freshness of their original tints? Would we not, with a contemptuous smile at their obvious folly, recommend the application of a little water as nourishment to the roots? And yet this is precisely what the whole of Britain is doing to-day. It sees that its industries are languishing and is trying to prop them up and revivify them as they stand, instead of finding the cause of the disaster and remedying that. The check of imports from Germany has shown us suddenly and forcibly how many industries we have lost. There is a shriek through the land for amendment, and every one is looking to the large manufacturing firms in the

hope that with them will lie the sovereign panacea. But a closer and calmer examination will show the manufactories to be an effect, not a cause. The cause is scientific research, and like the roots of the flower, it is hidden away from the sight of the public. When research is allowed to die, industry fades with it. Other countries are not so shortsighted as ours; they are keenly alive to this fact, and pour money out with a lavish hand, knowing that in the end it will return to them increased tenfold. The Engineering Correspondent of the *Daily Telegraph* of April 26 shows up strongly the contrast between the subsidies of the United States to the Bureau of Standards at Washington, which amount to £100,000, and ours to the National Physical Laboratory (a similar institution), which stop at the meagre sum of £7,000. And in *Nature* of April 1 there is an excellent article which, amongst other interesting matter, exposes the false policy of Mr. Runciman in employing only low-salaried chemists and the "lack of appreciation in this country of the services of the chemist and the absolute ignorance on the part of the Government of what remuneration should be given him." The same article also contains words which every Briton should take to heart: "Germany, on the other hand, has succeeded because she has placed science on a sound business footing, of which the fair remuneration of the scientific worker has been a striking feature." This is all undeniably true, but if the Government is representative of public opinion, does not the fault lie equally with the ordinary commercial man? He is so complacently satisfied with the British characteristic of the bull-dog of "digging his teeth into something and for ever hanging on." An excellent quality no doubt, but if he blindly butts in and digs his teeth into the wrong thing, what then? In this case the commercial man is convinced that if he spends his days in trying to grab business from a less wide-awake fellow business man, and then spends his evenings smoking and listening to the twaddle offered for his delight at a music-hall, success is still bound to be his. He prides himself most on being practical, and never gives a moment's thought as to what constitutes real practicality. He is so niggardly with his brains. If he took the trouble to survey business from a large standpoint, and not only from the point of view of his own small part in it, would he not find that nearly every commodity which is put on the market is first thought out in the mind

of the scientific research worker and then materialised by endless patient experiments in the laboratory? But no; to every purely scientific discovery that is made public he brings the same monotonous answer, "Oh, but I don't see what practical use it is to us." He demands that the discovery and its effects shall be almost simultaneous and shall immediately bring sovereigns into his pocket. But science does not work as he wishes and at his bidding, and he is too mentally lazy to perceive the none-the-less certain fact that the effects of scientific research, though sometimes indirect, inevitably appear finally in the manufactory, and materially increase the prosperity of the commercial world. If he once clearly recognised this, he would surely count no effort too strenuous to encourage scientific research and enable its votaries to earn a decent livelihood. The world of science is to-day making every effort to raise itself out of its ill-deserved poverty; but it is a small body of men—men of the highest order of intellect are necessarily numerically small—and what can they do struggling unaided against the apathetic masses of England? But if the commercial world of this country were to demand that the needs of their research workers, their own life-blood so to speak, should be adequately cared for, such an evil as this, which redounds only to their shame, would soon melt away. In these days the interests of all men are intimately bound together; business and science are indissolubly linked, and a wide-extended and whole-hearted co-operation are the only foundations on which humanity can safely build.

Prof. Woodward on the Needs of Research

In an address read on the occasion of the dedication of the Marine Biological Laboratory, Woods Hole, Massachusetts, July 10, 1914, Dr. R. S. Woodward took a comprehensive view of the "Needs of Research" and presented many sides of the question which the majority of educationalists and certainly the public at large usually overlook. He does not disdain to begin by defining the seemingly simple word "research," a definition which in this case especially will be particularly helpful, because the meaning the scientist attaches to the word and its signification in the mind of the public are at considerable variance. If the general mass of the people are to be roused to a sense of

responsibility to the requirements of the research worker, an exact knowledge as to the use of this term becomes a preliminary essential. "Research," he says, "as we now understand the word, means simply a systematic application of the methods of science," but "to journalists and to their readers it would seem that research is akin to necromancy. . . . Closely akin to this infantile fallacy is the more subtile error entertained by a majority, perhaps, of our highly educated contemporaries, that the more remarkable results of research are produced not by the better balanced minds, but by aberrant types of mind popularly designated by that word of ghostly, if not ghastly, implications, namely 'genius.'" Another interesting point is the differentiation between the inventor, who, in his opinion, works mainly for his own advantage, and the true investigator who labours unremittingly from purely altruistic motives. If such a view is correct it needs little discernment to discover to which of these two classes the most financial support should be given. He also puts forward a criticism on the attitude of educationalists in general and colleges and universities in particular, who, by forcing their finest minds to confine their attention exclusively to the training of youth, have materially hindered the progress of knowledge. To quote his own apt words, "Research has been and is still rarely regarded by the great majority of academic men and women as anything but an unimportant incident to the principal business of academic life. This principal business is the transmission from generation to generation of acquired learning; and it has been adhered to so generally and so rigorously in the past that until our own time educational institutions might be said, with only slight qualifications, to have been depositories of stationary thought." One of the chief difficulties which beset all institutions, whether large or small, is that they insensibly drift into a backwater—and not only do they fail to perceive whither they are drifting, but fail to recognise their backsliding even after they have reached the point of stagnation. Gratitude is therefore due to any clear thinker like Prof. Woodward, who can see and point out the trend of thought and action. He finally makes a strong appeal for adequate funds for the maintenance of an adequate system for the furtherance of research. He makes this appeal in America to Americans, whom he owns to have given generously towards this end. If their liberal endowment of research is not sufficient for its crying

needs, how great must be the needs of research in this country, where the public are not yet awake to the fact that it deserves a worthy support.

Types: Men of Principle

It is difficult to decide who is the more foolish, the aggressive militarist or the abject pacifist. The one concludes that might is right, and that any one who is strong enough to do so may seize anything he pleases from the rest of the world; and the other concludes that the best way to ensure peace is to take all punishments lying down. Thus the existence of the one type encourages the existence of the other type. The militarist could not exist but for the pacifist, nor the pacifist but for the militarist, and the two types have been jointly responsible for the disaster of the present war.

Men think of the aggressive militarist as being a loud-voiced, brutal, military personage; but, as a matter of fact, we have never known a British military man (at least) who is of this persuasion. The true-bred type is probably physically rather a feeble personage in high hereditary position—and we all remember what his father thought of the young Frederick the Great before the latter became the hero of militarists. Indeed, the aggressive militarist is apparently a neurotic person who attempts to cover his weaknesses by a profession of strength, and who, when he finds himself in high place, will too easily discover numerous enough sycophants to support him. But the successful parvenu often adopts the same attitude. Thus the Germans were so elated by their unexpected victory forty-three years ago over a people who had long previously established their superiority in arts and war that, like other parvenus, they came to think they were entitled to whatever they wanted.

On the other hand, the abject pacifist is often a person of the same neurotic type, who, however, has not been born in the same "high" place. His feeble and supersensitive soul shrinks at the thought of any pain, and his equally feeble intelligence prevents him from distinguishing between the sorrows which can be prevented and those which cannot. It is a mere chance whether he becomes a pacifist or a antivivisector, or even shudders at the mere suggestion that a doctor should inoculate him with a vaccine. As with the aggressive militarist, however, there is another type of pacifist—the robust parvenu, who by

success in his shop, has come to consider that he is always sure to be right in everything. And we know this type in the classical British doctrinaire with a decided manner, a clean-shaved lip, and a fringe of beard under his chin. Often again the pacifist is a rather stout but already rather bald-headed youth, who makes himself felt at discussions after tea by the weight of statistics with which no one else is acquainted. Now all these men are Men of Principle.

The Man of Principle begins to exist as soon as he reaches the years of indiscretion. Miserable mediocrities like ourselves, when we arrive at, say, twenty years, admit to ourselves that we have little learning, no experience, and a very ordinary brain, and that it would be altogether wiser in us not to form decided opinions upon everything until we are twenty-five or thirty years older. But this is just where the Man of Principle differs: for no sooner has he left school than he has already made up his mind about everything, and the whole of his subsequent life is devoted to the purpose of proving that he was right before he was of age. With him theories were not made to fit facts, but facts, theories; and when they will not fit, he claims an *à priori* mission. It is a mere accident whether he becomes an aggressive militarist or an abject pacifist. If, when a boy, he had succeeded in thumping another boy he would probably become the former; but if, on the other hand, it was he had been thumped, he would become the latter. Or he will decide according to his birth—to be a thumper if he is a king, and to be a thumped if he is not. In both cases the principles have not been arrived at by any careful analysis of all possible facts achieved by an open mind trained in the ways of correct reasoning, but by an instantaneous appreciation of the truth instilled into a superior brain by a direct flash of light from above. So given, the inspiration cannot be modified, and the whole universe is centred round the man of principle's principal principle. He is a Tory, or a Liberal, or a Radical; he is an aggressive militarist, or an abject pacifist, or an antivivisector, or an antivaccinator, or a woman's-rights man, or an everybody's-rights man, or a socialist, or a nihilist, or a total-prohibitionist, or an anticonscriptionist, or a misogamist, or any other kind of universal shrieker. But, strange to say, he often succeeds by the sheer weight of his stupidity in making his way among his more rational fellows, and has even been known to enter Parliament! For the point

of his principle cannot be turned by argument, and the boredom inflicted by his dulness stupefies his opponents like the trench-gas of the Germans.

We must always distinguish between intellectual principles and what are called moral principles. The former are not moral, because they assume an inspiration of reason which they do not possess. The intellectual principle is by its very nature the opposite of an intellectual theorem—which, even when it is wrong, must be based upon a careful analysis of arguments for and against. But when a man has intellectual principle he really pretends that he has performed an intellectual integration by intuition, and we dispute his pretence. On the other hand a moral principle is not a principle at all, but an ordinance laid down by the evolution of millions of years and always justifiable by the closest reasoning. For instance, that we should do to others as we would they should do to us is a moral principle sanctioned by evolution and by the closest argument; but it is disregarded by the militarist. But, on the other hand, the hypothesis that we should avoid all war under any circumstances is an intellectual principle which will not bear discussion, and is merely a pretended inspiration out of the inane. Perhaps the most crushing refutation of the latter fad was recently delivered by Mr. Garrison, the American Secretary of State for War. He is reported to have ridiculed the argument of the pacifists that if a nation was prepared for war it was more likely to be involved in war. Mr. Garrison concluded, "Evil preys upon virtue, the unjust upon the just, and the covetous upon those whose possessions they covet"—words which may be applied to many of the current irrationalisms of feeble people.

The British Science Guild

There has been much discussion in the papers during the past six months as to the advisability of Great Britain making more use of her men of science during the war, especially as the war necessitates the maximum output of scientific invention. Of all societies the British Science Guild has been t' most active, not only in endeavouring to obtain more adeq' remuneration and a better status for scientific workers t selves, but in striving to foster the manufacture of I scientific instruments in place of the German-made good held the market up to the outbreak of war.

On the 1st July the Guild held its General Meeting of Members at the Institution of Electrical Engineers, with the Right Hon. Sir William Mather in the Chair, and Sir Boverton Redwood, who moved the adoption of the Ninth Annual Report, summarised some of the more notable activities of the Guild. These included the successful efforts of the Medical Committee to quash the unpatriotic campaign of the Anti-Vivisection Society against the anti-typhoid inoculation of the British troops, and a scheme to induce users of scientific glassware to give a guarantee of continued support to the industry—a plan which has already achieved satisfactory results. The great feature of the meeting, however, was the able speech of Sir William Ramsay on the Organisation of Science. While giving due praise to the efforts of all the separate scientific societies during the past ten months to utilise to the full Britain's scientific assets, he pointed out, and emphasised the fact, that nothing of a really effective nature could be done until such societies were made subservient to one central body of scientific men, to whom the Government Departments should be compelled to apply for advice and assistance. He said that the Royal Society was eminently fitted to play such a part, and read a scheme drafted by Lord Sydenham which showed how such an idea could be made practical. Sir William Ramsay, in his attempt to rouse his audience to an adequate sense of the importance of such an undertaking, did not hesitate to bring to light all the shortcomings of our country by comparing it very unfavourably with that of France. He narrated how our Ally, as early as August 4, had called a general meeting of her Academy of Sciences, which decided to offer the whole scientific resources of the country to the French Government, and pointed to the lamentable fact that on the 1st July, eleven months later, such measures in England were still conspicuous by their absence. As a striking illustration of the slowness of Britain to alter its ideas he read an extract of an address, given sixty-three years ago by Lord Playfair on "Industrial Instruc-'on on the Continent," which is equally applicable at the ·sent day. In it Lord Playfair remarked, "For many s foreign States, acting upon the facilities for communi-ı, have expended annually large sums in sending highly ·ened men to our country, for the purpose of culling from ·erience, and of importing it into their own land; and

we see the effect of the experience thus readily acquired, when united with the high development of mental labour, in the rapid growth of new industries abroad. . . . With us, there is a wide-spread jealousy of science and a supposed antagonism between it and practice. . . . While we continue to rely upon local advantages or acquired experience, we allow a vast power to arise abroad, which is already telling against us with wonderful effect." Sir William Ramsay also deplored the fact that no publicity is given to the work that is being done in this country. The names of the workers are studiously suppressed without any need for such secrecy, and one body of men is barely cognisant of the aims and achievements of the others. At the close of the meeting Sir William Mather stated that a letter conveying the opinion of the meeting would be forwarded to the Prime Minister.

Artificial Production of Rain in Queensland

A correspondent in the *Brisbane Courier* speaks of experiments conducted in Queensland and in Japan, and calls attention to the fact that last year the radio-telegraph engineer to the Commonwealth, Mr. Balsillie, patented a process for producing rain. The matter has also for a number of years engaged the attention of Sir Oliver Lodge, and in reply to questions put to him by a Queensland farmer a few months ago, Sir Oliver said that the idea of producing rain by means of electric currents driven cloudward had engaged his thought thirty years ago, when he lectured before the British Association at Montreal on the subject. Since then he has conducted experiments on a larger scale; but what he desired was to see it tested on a still larger scale, and he has sought methods whereby large quantities of electricity can be discharged into the atmosphere from elevations, not only for the purpose of bringing rain, but of locally clearing away mist and fog, a process which he regards as more probable, indeed certainly possible.

CORRESPONDENCE

To the Editor of "Science Progress"

"THE MIRROR OF PERCEPTION"

Sir,—Will you kindly afford me space to reply to the criticisms contained in the review of my book, *The Mirror of Perception*, which appears in the current issue of Science Progress?

I desire, in the first place, to express my appreciation of the reviewer's generous recognition of merit in the book, all the more so that he does not agree with my conclusions.

The reviewer says: "The representation of matter in terms of consciousness does not, properly understood, involve any falling off in the 'reality' of material existence. It would be as reasonable to say that because colour can be represented in terms of ethereal undulations, therefore it ceases to be real."

This comparison would be valid against me if, and only if, I based my reasons for believing that matter is not real on the distortion which the secondary qualities of bodies (*e.g.* colour) have undergone in the process leading to their perception; but all these reasons are based on the distortion which the spatial qualities of matter have undergone, the first thing which I set out to establish being the fact of this distortion.

If the positions of the various points of a body are changed according to any fixed law, we get, as the result, an unreal image of the body, a thing which does not exist, but only appears to exist. Thus, as I say on p. 13, "the question of the reality of material bodies depends entirely on the question whether space is real or not." A few lines further on I admit that the distortion of the secondary qualities of bodies has no effect on their reality, if space is real; for I say, arguing on the supposition that space is real, "even if our knowledge of material bodies is not true knowledge of them, there is

still something real which occupies the real space which appears to be occupied by the body."

I give the following reasons for believing that space is not real:

1. An unreal concrete thing (*e.g.* the image of an object in a mirror), since it does not exist, but only appears to exist, depends for its apparent existence on the existence of some mind which falsely believes, perceives, or conceives it to exist. Therefore, it is impossible for an unreal concrete thing to exist alone in the universe. Now, if we apply this to a material body, and suppose it to be alone in the universe, what would be its position? It would have no position; for mathematicians have long given up regarding absolute position and absolute motion as real. Further, if absolute position were real, our conception of relative position is so clear that we should surely be able to conceive absolute position: yet it is inconceivable. But, if absolute position is not real, the difference between two absolute positions, *i.e.* relative position, cannot be real; for it is absurd to suppose that the difference between two things, neither of which exists, can exist.

2. In general, an effect is unlike its cause, especially so, if the effect is very complex. Now, perception is undoubtedly very complex. Therefore a material body must be very unlike the cause of its perception. We know that the body's apparent secondary qualities are very unlike their corresponding realities. It would be at least a very surprising thing if its spatial qualities should remain unchanged after undergoing a process which has changed the secondary qualities out of all recognition.

3. If space is real, then, when a man moves from England to Africa (say), either his mind moves through space, or it does not move. Either form of the dilemma presents very great difficulty. On the other hand, if space is not real, neither the mind nor the body moves, since both body and motion are appearances only. There are only changes of consciousness.

I endeavour to show that space is the appearance of time, time-relations being converted into space-relations in the process leading to perception, and that motion is the appearance of consciousness. I endeavour to meet the difficulties involved in regarding time as of three dimensions. On the

other hand, the difficulty of supposing that the consciousness of all minds living contemporaneously occupies the same indefinitely thin time of one dimension is one which it is impossible to exaggerate.

4. Finally, there is no doubt that the theory which I have advanced acts like magic in solving metaphysical difficulties; the interaction of mind and matter, the origin of consciousness in time, the substance of matter, what was originally a system of causes and effects only having suddenly introduced into it an entirely different system of purposes and ends, and several other difficulties, all easily yielding to this solvent. If all these difficulties are met by simply supposing that the spatial relations of bodies have undergone distortion as well as the secondary qualities of bodies, time-relations being converted into unreal space-relations, is this not a sufficient reason for adopting the view, especially as it is difficult to believe that space-relations have not undergone distortion in the process leading to perception, seeing that the secondary qualities of bodies are known to have undergone very great distortion in the same process?

I may say that the theory is not encumbered with the incredible Occasionalism which killed Berkeley's Idealistic theory.

With regard to the criticism of the expression "parts of minds," I admit that I ought to have been more explicit. I meant the subordinate, sub-subordinate, etc., minds of which, according to my theory, all minds are constituted.

Yours faithfully,

LEONARD HALL.

REVIEWS

MATHEMATICS

The Teaching of Algebra (including Trigonometry). By T. PERCY NUNN M.A., D.Sc. [Pp. xiv + 616.] (London: Longmans, Green & Co. Price 7*s*. 6*d*.)

Exercises in Algebra (including Trigonometry). By T. PERCY NUNN, M.A., D.Sc. Part I. [Pp. x + 421.] (London: Longmans, Green & Co. Without answers, price 3*s*. 6*d*. With answers, price 4*s*.)

Exercises in Algebra (including Trigonometry). By T. PERCY NUNN, M.A., D.Sc. Part II. [Pp. xi + 551.] (London: Longmans, Green & Co. Without answers, price 6*s*. With answers, price 6*s*. 6*d*.)

"THERE are situations," Dr. Middleton remarks, "too delicate to be clothed in positive definitions." Perhaps algebra is one of them; at any rate Prof. Nunn confesses in his opening paragraph that "the proper definition of algebra may easily be carried to a point where its interest becomes academic rather than practical." But though an author may, with propriety, evade the issue at the opening of his book, he cannot do so for long. His book is the answer to the question proposed in the title. Certainly the answer given in this threefold volume is one of striking originality.

Writers on algebra work in two fields—the well known and frequently ploughed tracts which extend "up to and including quadratic equations," and then the unenclosed pastures of higher algebra. Tradition seems alone to mark out the bounds of the latter course: its items are unrelated, their discussion is partial and generally unscientific. A mathematician who has not been acquainted in his training with our higher algebras would wonder at the arrangement and selection of the topics discussed in such a study. He would wonder why theory of numbers was included and might also be surprised at a chapter, or chapters, devoted to the reduction of quadratic surds; and he would certainly condemn the divorce of two such closely related subjects. Now Prof. Nunn has once and for all cut himself adrift from tradition and made every reader think for himself what should, and should not, be included in a course of algebra. For this every teacher owes him gratitude, and it is not only for originality that we should be grateful: there is throughout these books such an unusual refreshment and novelty of treatment that even when we are not convinced we are stimulated and helped.

The first volume is described by the title of the Teaching of Algebra: it is perhaps, judging from a hint in the preface, based upon lectures to teachers, and here and there, but fortunately not too often, it shows signs of its origin. It is really an illuminating and instructive talk about algebra and other mathematical topics. Philosophy and history pleasantly season the discourse. A general introduction, in which the nature of algebra is the theme, is followed by Part I., which is divided into three sections dealing with non-directed numbers, directed numbers, and logarithms, and Part II., which contains trigonometry of the sphere, complex numbers, periodic functions, limits, and statistics. Many of the sections

are preceded by a programme of the contents. Are we right in supposing that these contain the lectures referred to in the preface? Whether this conjecture is correct or not, they consist of admirable statements of matters which should be of the deepest interest to teachers, all presented in an attractive form by a writer of keen and vigorous intellect. Here are a few of the author's *obiter dicta*, which may give, in a fragmentary way, some idea of Prof. Nunn's scope:

"Over a large part of the field of mathematics the fundamental idea is not *magnitude* but *order*."

"The long-delayed rational interpretation of 'imaginary numbers' appeared almost simultaneously in three distinct quarters at the beginning of the nineteenth century."

"Motion is simply 'geometry *plus* time,' and any reason which justifies the study of geometry as a branch of mathematics must justify equally the inclusion of kinematics."

"Statistics constitute at once the oldest and the newest branch of mathematics: the oldest, for their practice, in some form, is one of the primary necessities of an ordered social life; the newest, for their theory is to a large extent a production of the present generation. For both these reasons it is greatly to be desired that an elementary study of the subject should come to be regarded as part of the normal programme of secondary school mathematics."

How strange all this sounds to an ear attuned to the frigid chants of the older school of algebraists!

In the bare enumeration of contents we have seen the wide scope given by the author to his subject: it is to include trigonometry, plane and spherical, infinitesimal calculus, and statistics. Is the author quite fair in the wide sweep which he takes? Elementary analysis might have provided a tent large enough to cover such a range; but algebra, even when it includes trigonometry, is hardly an appropriate title.

With regard to the Teaching of Algebra, Part I., and the Exercises, Part I., there is little that will raise controversy. The exercises are a very interesting set, interspersed with short notes and diagrams; there is a choice from which the teacher will obtain the routine examples which the average boy has to do, and also the harder questions which stimulate and interest the cleverer students.

But in the treatment of Part II. the author has deviated from the sound lines on which he has developed Part I. To show the author's method we will take Section VIII., Limits. Here we have on this subject in the first volume 22 pages, and in the third volume about 100. It is, apparently, in the third volume that the main discussion is contained, while the first volume is, as it were, a commentary upon the main treatment. Somewhere perhaps in the 1,588 pages of the volumes before us this is made clear; but an arrangement in which the natural order is violated constitutes a serious defect.

The headings of the sections of Part II. have been given above; but enumeration hardly gives an idea of their contents. Limits, in the writer's view, means the infinitesimal calculus and ordinary differential equations, and even such a subject as space-filling curves is treated in some detail; while spherical trigonometry covers map-making, great-circle sailing, and some parts of astronomy.

To sum up, the book contains a most valuable and admirable elementary algebra; but we do not expect that Prof. Nunn will find writers and teachers who will adopt his course in what some writers call complementary algebra. There are many w se and excellent things in this part, but they constitute a mathematical miscellany, and most of it forms no part of a course in algebra.

C.

Plane Trigonometry. By H. LESLIE READ, M.A. [Pp. xiii + 290 + xvi.] (London: G. Bell & Sons. Price 3*s*. 6*d*.)

THIS book is an attempt to present the principles of elementary plane trigonometry in a simple and practical light. It is edited with some care, and examples in large numbers are provided at all stages to suit many tastes. The methods used are those of the orthodox teacher, and need little description. It is, however, a little disconcerting to find that a single-valued function tan x has amongst its values $\tan 90^\circ = \pm\infty$. Again, we find $\pm \cos 90^\circ = 0$. The facts are perhaps explained satisfactorily in the text, but the author's notation surely misrepresents an argument which needs most careful treatment. It is a pity that a subject which after all is comparatively limited and concise should not be expounded in briefer compass. Brevity is the soul of other things besides wit, and if the subject could have been contained within 150 pages, the beginner would have had a less formidable task, and the end of his preliminary labours would have been reached with less expenditure of both time and money. Publishers and authors should remember that the reduction in price of mathematical text-books is an urgent educational need. We hope that schools will never buy bad books because they are cheap; we should prefer to see good books written concisely and published at a low price.

C.

Functions of a Complex Variable. By JAMES PIERPONT, LL.D., Professor of Mathematics in Yale University. [Pp. xiv + 583.] (Boston, New York, Chicago, and London; Ginn & Company, 1914. Price 20*s*. net.)

THE first six chapters of Prof. Pierpont's treatise are devoted to the usual preliminaries to the theory of functions, but many of the subjects are treated in a refreshing way; for example, we may instance the discussion (pp. 22–4) of the irreducible case in cubic equations. The seventh chapter begins with a definition (p. 210) of an analytic function, in which the condition that the first derivative is continuous is included. This is of course necessary if, as is done here (pp. 211–14), Cauchy's fundamental theorem is proved by using Stokes's theorem: the proof of Goursat, which avoids the assumption of the continuity of the derivative, not being used. Prof. Pierpont's account of the theory of functions may be roughly described by saying that little use is made of the work of Riemann, and for the most part the theory is developed by the methods due to Cauchy and his school. However, it was of course necessary to pay attention to some parts of the subject first clearly treated by Weierstrass. Thus we have an account of analytic continuation (p. 224), in which, by the way, a welcome feature is the working out of an application showing that an analytic relation of plane trigonometry is valid when the variable is complex, and the theory of essential singularities (p. 244). Weierstrass's factor-theorem is dealt with in Chapter VIII, and here it must be remarked that the application given on p. 293 is somewhat misleading. Weierstrass's theorem is to show that an integral function with certain assigned zeros exists; not to give a means of developing a particular function, like the sine, in a product form. Such a development is best effected by a method due to Cauchy or by the method sketched on p. 281. Weierstrass's formula gives a general expression for all the integral functions whose roots are distributed something like those of the sine, and it is only, so to speak, by good luck that a great part of this expression is the same as the well-known development for the sine function. Chapter IX is devoted to Beta and Gamma functions and asymptotic expansions; Chapter X to Weierstrass's elliptic functions; Chapter XI to the elliptic integrals

and functions of Legendre and Jacobi; and Chapter XII to the Theta functions. To return for a moment to Chapter VII, the definition of residues (p. 256) is the now usual one as the coefficient of the first term in the expansion in negative powers. This definition is rather an anachronism, for Cauchy defined residues and extensively used them long before Laurent's theorem was discovered. It would be a good thing if more use were made of the original definition and the many applications of the calculus of residues. In quite modern times the importance of these applications has been shown in the masterly work of Lindelöf.

The last three chapters of the book are both interesting and important, especially from the point of view of the students for whom the book was originally written. In these chapters Prof. Pierpont, having in mind the needs of students of applied mathematics, dwells at some length on the theory of linear differential equations, especially as regards the functions of Legendre, Laplace, Bessel, and Lamé. It is new and refreshing to find, in a text-book of function-theory, a good discussion of the modern point of view in the theory of differential equations. It is quite true that Cauchy ought to be regarded as the founder of the modern theory of these equations, but the remarks on p. 2 are misleading in so far as they mention Cauchy's work on the theory of functions of a complex variable only in connection with his theory of differential equations. Cauchy's theory of functions of a complex variable first grew up as a result of his work on the evaluation of definite integrals.

The only mistake which the reviewer has hitherto detected is a misspelling of Rodrigues's name (p. 502). The volume is well written and well printed, and is uniform with Prof. Pierpont's two volumes on the theory of functions of real variables.

PHILIP E. B. JOURDAIN.

Dialogues concerning Two New Sciences. By GALILEO GALILEI. Translated from the Italian and Latin into English by HENRY CREW and ALFONSO DE SALVIO of Northwestern University. With an Introduction by ANTONIO FAVARO, of the University of Padua. [Pp. xxvi + 300.] (New York: The Macmillan Company, 1914. Price 8*s.* 6*d.* net.)

THERE have been two previous English translations of Galileo's *Duscorsi e Dimostrazioni matematiche* of 1638: one by Thomas Salusbury published in 1665, and one by Thomas Weston published in 1730. Both are now very scarce, especially that of Salusbury; and the present handsome translation will be welcome to all English-speaking students. As a frontispiece there is a very good reproduction of Subterman's painting of Galileo, and the translation is made with great care from Favaro's national edition of the works of Galileo. In the national edition, the Leyden text of 1638 has been followed faithfully but not slavishly (p. xii), and the manuscript corrections and additions of Galileo himself have been used in this translation. However, all the other comments and annotations in the national edition have been omitted in this translation save here and there a footnote intended to economise the reader's time. To each of these footnotes has been attached the signature "[*Trans.*]" in order to preserve the original as nearly intact as possible (p. vi). The numbers of pages inserted in the text refer to the national edition.

The "two new sciences" created by Galileo are the theory of the strength of materials described in the first two days of the dialogue, and the theory of the uniform acceleration shown by falling bodies described in the third and fourth days. The dialogue is held between Salviati, who represents the opinions of

Galileo himself and to whom he refers as "our Academician" (p. 6), Sagredo, and Simplicio, who represents the opinions of Aristotle. It is generally, and with justice, supposed that Galileo's work shows a decisive breaking away from the physics of Aristotle and antiquity in general, but we must not lose sight of the fact, that is shown in the historical work of Wohlwill, Duhem, and others, that the breaking away only came about gradually; and it would have been very welcome if the translators had given an introduction about how the great gap between the ancient and the Galilean mechanics, as described in this maturest work of Galileo, is to be filled up. This work is, as Galileo himself said, "superior to everything else of mine hitherto published"; but it is very important, both for the historian and the intelligent student, that good translations of all Galileo's early work which is scientifically relevant should be published. In particular, the reviewer has heard that there are some important manuscripts of Galileo in the library of Eton College, and it is well known that Galileo's *Della scienza meccanica* contains an approximate statement of Newton's important third law of motion. It is especially important to us Britons that a detailed study should be made of Galileo's work both in mathematics and in dynamics; besides the more obvious effect of Galilean ideas on Newtonian mechanics, it is extremely probable that Galileo's frequent use of infinitesimal ideas and fluxional ways of considering these ideas influenced Newton through Barrow and possibly others.

The first day of these dialogues begins on the subject of the resistance offered by solid bodies to fracture, and continues with a large number of interesting digressions. Of these digressions the most important are the considerations on the subject of geometrical continuity (pp. 27 ff.), on infinity and indivisibility (pp. 30 ff.), the suggestion for the determination of the velocity of light (pp. 42 ff.), the refutation of Aristotle's opinion that heavier bodies fall more quickly than lighter ones (pp. 62 ff.), and the investigation of the vibrations of the pendulum (pp. 94 ff.). We must remember that such digressions were not formerly considered out of place in a treatise on physics: we need only remember Aristotle's own *Physics*, which is principally a discussion of infinity and continuity. Galileo's speculations on infinity are very remarkable; he observed that square numbers appear to be as numerous as whole numbers, because there is a one-one correspondence between the two infinite classes, though the former is a part of the latter, and concluded that the paradox is solved by denying that the attributes "equal," "greater," and "less" are applicable to infinite quantities. The dialogues on the other days do not contain any digressions. That on the second day is shorter and contains many theorems on the resistance of bodies to breaking or bending. That on the third day is on "local motion," or the motion of naturally falling bodies. After various propositions on uniform motion, we have a section on uniformly accelerated motion, and the subject is treated very much in Euclidean manner, interspersed with the ingenious experiments with which Mach's *Mechanics* has made us familiar and which form a splendid foundation for elementary instruction. The first mention of the problem of the Brachistochrone is interesting (p. 230). The dialogue on the fourth day consists of (i) researches on the paths of projectiles, and (ii) the beginning of a discussion on percussion, which has been praised by Mach as showing heights of Galileo's genius which have often been inaccessible to meaner mortals. Galileo contemplated a dialogue for a fifth day, which was to have treated "of the force of percussion and the use of the catenary," because in 1638 he had plunged more deeply than ever "into the profound question of percussion" and "had almost reached a complete solution" (p. xiii). Following the example of the national

edition, the appendix, written at an earlier date, containing theorems on centres of gravity, is omitted here.

The book is, on the whole, well translated and has a useful index. Such things as "alright" (p. 71) and "to thoroughly understand" (p. 283) are only, apparently, shocking to British eyes or ears or both.

PHILIP E. B. JOURDAIN.

PHYSICS

A Text-Book of General Physics, Electricity, Electromagnetic Waves, and Sound. By J. A. CULLER, Ph.D. [Pp. x + 321.] (Philadelphia: J. B. Lippincott Company. Price 7*s*. 6*d*. net.)

THE author states in the preface that he had, while preparing the book, three aims in view: (1) lucidity of description, etc., to the *average* student, (2) emphasis on the physical side of physics and some account of commercial applications, (3) incorporation in the body of the discussions and in their proper place, of the electronic and electromagnetic theories.

It is, of course, a matter of personal judgment as to what is the *proper* place in the development of a subject for the introduction of a particular theory; but it is certainly a novel procedure to begin a text-book with a discussion on "What Electricity is," and follow up with "Evidence for the Electron Theory," and an account of J. J. Thomson's and Lenard's experiments on kathode rays. One thing must be taken for granted in such a beginning, viz. that the *average* student whom the author has in mind is no mere beginner, and even so, it is still doubtful if the author is producing, as he claims in the preface, a "logical development of the live topics which, it seems, should be included in a text-book for college students." Exception could be taken on the same grounds to his introduction of electrons almost at the outset of the sections dealing with magnetism. It seems to the writer of this notice that a *logical* development requires the very opposite procedure to that adopted by the author. As he himself remarks, "the electron theory is not the result of a sudden discovery, but rather a growth from the accumulated evidence of years of experiment." Does that fact alone, then, not demand in an explanatory manual a fairly complete account of those experiments at the outset, so that the student may grasp the strength of the evidence on which the electron theory is based?

However, in other respects the author effects his purpose with considerable success. By omitting a good deal of the mathematical work usually introduced into manuals of this type, he finds room for lucid and interesting accounts of commercial and technical applications, and certainly makes his book strong on the "physical side of physics" in the sense that he devotes more space to the description of parts and dimensions of various types of apparatus than to the mathematical theory of their functioning. The subject of Electromagnetic Waves is introduced well on in the book, and serves, as it were, as a preface to a brief account of Physical Optics—too brief in the writer's opinion, considering the importance of that branch of physics. A short and well-written account of the essential phenomena of sound concludes the volume.

J. RICE.

Experimental Electricity and Magnetism. By M. FINN, M.Sc. [Pp. x + 436.] (London: G. Bell & Sons, 1915. Price 4*s*. 6*d*. net.)

THIS is a book of quite a different type. It is intended for beginners, and not only deals with the matter usually taught in schools up to Matriculation standard,

but goes somewhat further. Its method is experimental, and intends that the student should carry out a number of qualitative experiments as well as the usual measurements. The author is one of those who believe that a beginner should be introduced to the subject by means of electrokinetics rather than electrostatics. This point of view is not novel, but in no other book of the same type (as far as the writer is aware) has the author exhibited the courage of his convictions more openly, or set about putting them into practice in a more thorough and painstaking way. One novel point is the abandonment of primary batteries as the source of current, and the use, instead, of continuous current lighting mains, in connection with suitably wired "fool-proof" lamp-boards, which can be made with little cost. That this method has everything to recommend it in the case of pupils who live in modern towns, possessing electrical installations, goes without question. It will evoke their eager interest at the outset, and will find many of them already furnished with quite a fund of personal experience concerning the "mains" and "volts" and "amps.," which, although confused and even erroneous, will form an excellent foundation for further insight, and even for a rough illustration of the scientific method in sorting and classifying essential facts. The book is clearly written, well got up, and furnished with a large store of experimental work. One notable point is the unity which is introduced into the teaching; magnetism is introduced by means of the electromagnet; electrostatics is no longer a matter of sealing-wax and cat's fur; it falls into its natural place in the development of the subject, and electrification by friction "is relegated to a subordinate place as merely one method (although an important one) of producing electric charges."

The book can be heartily recommended to every teacher; it is, indeed, the outcome of the author's own experience as a science master. Even those who may disagree with his arrangement of the sections, or who may, through lack of electrical installation, be unable to avail themselves of his methods, will find much in the volume of which advantage can be taken.

J. Rice.

CHEMISTRY

Volumetric Analysis. By A. J. Berry, M.A., Fellow of Downing College, Cambridge. [Pp. viii + 138, with 7 diagrams.] (Cambridge: at the University Press, 1915. Price 6*s.* 6*d.* net.)

This volume, which appears in the publishers' well-known "Physical Series," is designed to fill a place between elementary and advanced text-books on the subject. The author has laid special stress on explaining the conceptions of equivalent weights and normal solutions. How far he has succeeded in the latter case is an open question. Junior students seem to have no difficulty in grasping the significance of "normal" when applied to acid, alkali, and ordinary salt solutions, but when they first make the acquaintance of, say, n/10 permanganate, they naturally apply to it the rules which have enabled them to arrive at values for these former solutions.

No matter how they have been taught the meaning of "normal" solutions, students seldom realise the full significance of the term until they encounter permanganate or similar oxidising solutions. The term n/10 permanganate, when the solution is to be used as an oxidising agant, is obviously a misnomer. It is only really correct to speak of n/10 "available oxygen solution (via permanganate)," and if this terminology were strictly adhered to, in the case of unior students, the difficulties now experienced would be largely minimised.

An omission which can be urged against this, as indeed against most other volumes on analysis, is the absence of any exact details as to the degree of accuracy of a particular method of analysis. Such terms as "accurate," "very accurate," or "only empyrical" are much too indefinite for a scientific work, and it is quite time they should be dropped and exact data substituted whenever possible. By experience in analysis one arrives at a time when such terms may have a more or less exact meaning, but the inclusion of a few details from actual practice or from the original papers, on the order of accuracy likely to be experienced in each particular method, is very much to be desired, and most particularly for junior students. Another point which could with advantage receive more attention than it does at present, is the exact degree of chemical purity of the commoner substances used in preparing standard solutions. Very often, in the absence of any exact data, many solutions have to be standardised by working back through various standard solutions to some solution, usually sodium carbonate, which can be prepared accurately by weighing out the chemically pure solid. To have to do this generally involves a considerable amount of time and labour, which could be obviated to a large extent if the exact analysis of the starting substance were known. The best British and foreign firms supplying chemicals for analytical purposes now give an accurate analysis of many of the commoner substances, and these ought to be available in the text or as an appendix.

As to the methods, the author has selected most of those which are thoroughly sound and proved by practice. Besides these there are several useful so-called "empyrical" methods which are not usually to be found in this type of book. On p. 44 the volumetric determination of SO_2 in aqueous solution by iodine is outlined, and the usual explanation is offered for the discrepancies which occur when titrating more concentrated solutions of the gas, unless the solution is run into excess of iodine solution, and the iodine back-titrated with thiosulphate. Even with the precaution of vigorous stirring as the SO_2 solution is being run in, which the author does not mention, we doubt if the improved method is altogether free from irregularities.

The book contains a very full chapter, illustrated by a few diagrams, on the theory of indicators, and containing a table of the dissociation constants for the majority of indicators, both basic and acidic. The ionic view is fully dealt with and the other theories are touched upon, but whilst the ionic theory has doubtless thrown very much light upon the mechanism of indicators, the author rightly points out that it by no means precludes the presence of other phenomena as essential adjuncts.

On p. 20, in connection with the reduction of iron and vanadium salts, the author takes to task the existence of the supposedly potent "nascent" state. It is very proper that such chemical fallacies should not become stereotyped and handed on to students with each new text-book. Again, it is satisfactory to find, as on p. 60, the theory of errors in indirect analyses clearly expounded, and further, on p. 10, that the uselessness of working out results to a decimal place, which can have no meaning, is pointed out. This latter is a fault to which young students are very prone, and one which very seldom receives any treatment in books intended for their use.

In a later chapter mention is made of useful applications of volumetric analyses, such as determinations of solubility, partition coefficients, and velocities of reaction.

C. S. G.

ARCHÆOLOGY

An Introduction to Field Archæology as illustrated by Hampshire. By J. P. WILLIAMS-FREEMAN, M.D. [Pp. xxii + 462, with 15 plates and 69 other illustrations.] (London: Macmillan & Co., 1915. Price 15*s.* net.)

ARCHÆOLOGY is a subject which is often associated with purely indoor work, but those who follow the line adopted by Dr. Williams-Freeman will find in the science an additional inducement to spend a vacation on a walking tour. The author has traversed Hampshire in all directions, and has made himself personally and intimately acquainted with the county's antiquities, especially with the ancient earthworks, sepulchral, defensive, and otherwise. In the course of his tours he has collected a considerable amount of new information, and the book is designed to serve the purpose of a text-book to those who desire to contribute something to general archæology by working out in detail the antiquities of some imperfectly known district. The book is in three parts. The first part gives general information upon this branch of archæology, the second comprises an itinerary through Hampshire, and the third consists of appendices giving most useful details of a technical character. The description of the influence of the climate and other geographical factors (forest-growth and so forth) upon the lives of our Neolithic and Bronze-Age predecessors is, we think, especially worthy of commendation. Among other points of interest Dr. Williams-Freeman finds that the long barrows of Hampshire do not bear out the idea that these tumuli are regularly disposed east and west.

Perhaps the least reliable portion of the book is the chapter (in Part I.) dealing with British ethnology. The author appears to us to introduce unnecessary complexities into the already complex subject of the intermingling of Iberian, Kelt, and Teuton. He regards the short darkish Welsh as being truly representative of the Brythonic (or later) Kelts, whereas he admits that both the Goidelic (earlier) Kelts, and of course the Teutons, were tall and fair, and that the Iberians were short and dark. It appears very unlikely that there should be such marked physical distinctions between two subdivisions of the Keltic race, and the probable explanation is that the Welsh are mainly Iberian in extraction—are in fact Brythonicised Iberians. Dr. Williams-Freeman is quite satisfied that the small brunet people of the west of Ireland are merely Goidelicised Iberians. He refuses, however, to identify the Bronze-Age Race with the Goidels.

The book is very interestingly written, and we infer from the amusing dedication that the author is far removed from that objectionable type of scholar who talks his "shop" in general company.

A. G. THACKER.

ZOOLOGY

Symbiogenesis. The Universal Law of Progressive Evolution. By H. REINHEIMER. [Pp. xxiv + 425.] (London: Knapp, Drewett & Son, 1915. Price 10*s.* 6*d.* net.)

WE cannot bring ourselves to accept the implication of the sub-title of this work, which is fully expanded later (p. xv) into: "I claim *the great principle underlying all Creative Life, all Progressive Evolution, to be that of 'Symbiogenesis,' i.e. the mutual production and symbiotic utilisation of biological values by the united and correlated efforts of organisms of all descriptions.*" Nearly all the words in this require definition, but even when used in the sense meant by the author it is still incorrect, in our opinion, to speak of symbiogenesis as *the* great principle

underlying creative life. Mr. Reinheimer has seized upon certain aspects of biology only too often overlooked or taken for granted in ordinary biological works. Firstly there is the remarkable interdependence of different organisms, which are almost as closely connected as the links of a chain, and secondly there is that phenomenon known as symbiosis. In passing, it may be noted that the meaning of the word symbiosis is strained far beyond common usage. Upon these two points the author builds up a theory, not claimed as subsidiary, but, as pointed out above, "the great principle." We must confess that we have not read the other books that the author with rather annoying repetition insists on pointing out he has written, and of which the present volume is in some measure an expansion. Perhaps because of this any attempt to trace a general theory running through the work has not met with success. Many interesting biological phenomena are referred to, and the author has evidently kept himself well informed of modern work, no small task in itself.

The vocabulary of biology is already so large as to be almost unwieldy, but when an author adds to it terms drawn from political economy the result does not add to clearness of thought. We find, for example, "What the animal wants is good currency—direct from the physiological mint, *i.e.* the plant"; "It is only prejudice which denies that we are justified in looking upon the activities of organisms as 'work' and upon results as remuneration in the economic sense of the term"; plant protoplasm possesses the secret of "storing the results of its surplus labour (capital) for future use," and so on. In the places where he speaks of evolution by co-operation he implies a great deal. Co-operation in the economic meaning implies a means of avoiding competition and its results, so that he infers the presence of a competition or, biologically speaking, a struggle for existence. This surely is not a principle to be lightly set aside. One more factor adds to the difficulty of following the argument in many places, and this is that the author has consciously or unconsciously adopted some sort of moral code into which the whole of the evolutionary phenomena must fit. It is stated that parasites have taken to "dishonest means" of obtaining food; animals adopt "bad food habits," are "ill advised," have "morbid inclinations," have a desire for "surfeit" when referring to feeding. To show the way in which such words are employed we quote the following, used with regard to *Convoluta roscoffensis* after "temptation leads them into the paths of indulgence": "The animal first rendered strong by auspicious ancestral dynamics proceeds to abuse its powers, and destroys and devours the weaker instead of cultivating and protecting its true complement. When *in extremis* of hunger and of bankruptcy it does not hesitate even to devour its own kith and kin. The diathesis so produced in turn provides the soil for other would-be profligates of every description."

In addition to this biological assumptions are made that are not justified; for example, we are told that structure follows function, that sex has evolved from a primitive hermaphrodite form, that the chloroplasts of Euglena are plant cells, and so on. These may, of course, turn out to be correct, but it detracts from the soundness of a deduction when it is made from statements which in the present state of our knowledge must be regarded as "non-proven."

From what has been pointed out above it will be seen that the whole book is written in a diffuse and vague manner, and moreover the flow of sentences or paragraphs is constantly interrupted by tags of Latin, Italian, French, and German. Perhaps if it were reduced to about an eighth of its present size it would make an interesting and probably stimulating essay.

Finally we should like to point out that the author has drawn upon the work of the reviewer, and even quoted phrases from it, without giving any acknowledgment thereof or indicating where it is to be found.

C. H. O'D.

Histoire de l'Involution naturelle. Par HENRI MARCONI. Traduite de l'Italien par Me IDA MORI-DUPONT. [Pp xii + 507, with 125 text figures.] (Paris: A. Maloine, 1915, prix 15 francs.)

TO attack a widely accepted idea in a science requires a great deal of courage, and this the author possesses. A well-established theory like the evolution theory is not to be overthrown by courage alone and it is necessary for the assailant to possess a wide knowledge of the subject-matter. This the author may or may not possess, but in reading through the book we have failed to find evidence of it.

It is impossible within the limits of a review to deal with all the points in the book, and it may be stated from the outset that instead of maintaining the "Involution Theory" the author in our opinion appears to have demonstrated nearly all the weaknesses of his case, and few if any of its strong points. The method adopted is first to show by means of quotations from authors (most frequently from Haeckel) that there are certain gaps in our knowledge or differences on small points, and to deduce therefore that the evolution theory is not correct and so its place must be taken by the involution theory. This form of argument will mislead no one save perhaps its user.

The chapter on Palæontology may be taken as more or less typical of the whole book. The sole palæontological authority that is quoted is Haeckel. Now Haeckel, although using palæontological data, was never a palæontologist. There is no mention of Dollo, Andrews, Broom, Smith-Woodward, Scott, Osborn, Williston, to mention but a few of the leading names in the palæontology of to-day, nor is there any indication of any knowledge of their works nor of the great advances that have been made. The only palæontological knowledge utilised seems to be that derived from Haeckel's works. It might of course be urged by the author that ignorance of the facts of palæontology does not in any way affect his argument, and so we will examine the latter. It is admitted that Amphibia appeared before reptiles, reptiles before mammals, and last of all man himself. Of this the author says (p. 102): "Le coup est formidable, je l'avoue, mais son importance n'est qu'apparente. Les faits existent, la science les interprète mal, et on subtilise la vérité pour prouver l'erreur."

Nowhere does he explain how these facts have been misinterpreted. The whole matter appears to be settled by remarking that their importance is only apparent. A further example of the kind of argument adduced may be judged by the following (p. 104):

"Mais ce qu'il importe surtout de faire remarquer à partir de maintenant, c'est que l'affirmation de l'existence en masse des Acraniens dans la période primordiale, n'est pas rigoureusement scientifique.

"Nous avons le défaut, nons y reviendrons, de toujours généraliser. Nous n'avons exploré que très peu des sédiments primordiaux et nous voulons leur attribuer l'importance de documents historiques de la Vie organique du monde entier.

"Qui nous dit que dans les sédiments primordiaux des régions devenues inaccessibles pour nous, on retrouverait les même fossiles et si cette exploration ne réserverait pas quelques surprises?"

The weakness of this is quite apparent.

It is asserted that Haeckel counted on the extension of geological researches,

and it is pointed out that it is unsound to build theories on hope. In a general way this last statement may be true, but in the particular case taken the hopes have been fulfilled in a wonderful manner. When the revelations of palæontology in the Karoo, in North America, in the Fayoum, and in the Siwalik deposits are considered, we can only gasp at the presumption that, ignoring all these things, can suggest that the measure of hope of the early palæontologists has not been filled to overflowing.

In concluding this chapter the author refers to a figure of Haeckel's in which the skeletons of the hands of nine animals are given and states that with the same anatomical reasons together with a correctly interpreted palaeontology "et intervertissant l'ordre, je dis que la taupe, la chauve-souris, le dauphin, le chien, l'orang et le gorille descendent de nous." To say that an evolutionist maintains that man is descended from the bat or the dolphin is absurd, and the converse statement is even more ridiculous.

So one could be wearied by going through this book page by page, hardly a sentence of which, save the quotations from other authors, would pass without challenge. The illustrations, with one or two exceptions, are poor.

This volume was taken up with hope but laid aside with great disappointment.

C. H. O'D.

Typical Flies. A Photographic Atlas of Diptera, including Aphaniptera. By E. K. PEARCE. [Pp. xii + 47, with 155 illustrations.] (Cambridge University Press, 1915. Price 5*s*. net.)

THE title of Mr. Pearce's unpretentious little book indicates both its contents and scope sufficiently so as to reduce further explanation to a minimum. For those commencing the study and collection of flies, however—and for such the work is primarily intended—many useful hints and suggestions regarding the collecting and preservation of these insects will be found in the preface. With the same object an outline sketch of Brauer's classification of the Diptera is given, together with the more important characters of the larger divisions and an enumeration of the families assigned to each. The fleas are here placed as the first family of the Nematocera, and a footnote informs us that "there is no reason whatever for separating the Fleas or Pulicidæ from the Diptera." To this statement, however, exception must be taken, as these pests are sharply differentiated by their structure and habits from the flies proper, and apparently are not very closely related to any other insects. The illustrations are arranged according to the second edition of Verrall's *List of British Diptera*. One hundred and twenty species of flies (often both sexes) and four species of fleas, representing in all thirty-three families, are depicted, and in addition three views are given showing the type of habitat usually selected by *Asilus crabroniformis* and *Hæmatopota pluvialis*. As far as possible brief remarks on the habits and haunts of both adults and larvæ are included under the figure of each species. The illustrations themselves are, on the whole, excellent, and the wing venation—so important, yet often so difficult for the beginner to interpret from descriptions—is usually clearly discernible. In a few cases only are the reproductions lacking in detail. Certain errors occurring in some of the legends to the figures are, perhaps, worthy of attention. The names of the fleas represented in figs. 3 and 4 should be, according to Rothschild (March 1915), respectively *Ceratophyllus styx* and *Palæopsylla minor*; the author of the specific name *Theobaldia annulata* (fig. 16) is Schrank not Meigen, and *Culex cantans*, Meig. (fig. 17), is stated by Edwards (1912) to be a synonym of *Ochlerotatus* (*Culex*) *maculatus*, Meig. The notes concerning the reproductive

habits of *Hippobosca equina* and *Melophagus ovinus* (figs. 153 and 155 respectively) are both peculiar and incorrect. With each species the larva matures within the body of the female and is deposited as such, although the subsequent transformation to the pupal stage or puparium is very rapid. The author, apparently, would have us believe that the puparium itself is deposited, and further that, in the case of the sheep ked, the length of the larval period within the female is so short as to be almost negligible.

To the novice this work cannot fail to be of great assistance. Perhaps all too frequently it has happened that, owing to the difficulty of obtaining an elementary and inexpensive treatise on the subject, the dipterological aspirations of a beginner suffer materially—even though not entirely repressed. This little volume will help considerably to overcome that difficulty, and thus may direct more general attention to a most interesting and important order of insects.

H. F. C.

BOTANY

A History of Botany in the United Kingdom from the Earliest Times to the End of the Nineteenth Century. By J. REYNOLDS GREEN, Sc.D., F.R.S. [Pp. xii + 648, with 2 illustrations.] (London: J. M. Dent & Sons, Ltd., 1914. Price 10*s.* 6*d.* net.)

THE author of this volume had, in addition to winning a distinguished place among plant physiologists by his work on vegetable enzymes, made for himself an enviable reputation as teacher, text-book writer, and, above all, as a brilliant historiographer of botany—indeed, it is not too much to say that in the last-named capacity his work stands supreme. His research work broke fresh ground in several respects, as might have been expected from the fact that he approached the study of plant physiology after having been trained in animal physiology and having acted as assistant to the late Sir Michael Foster. His text-book of botany, which has unfortunately been allowed to get out of date, if not out of print, is, in the present writer's opinion, by far the best that has been published in this country, and if revised and brought down to date would easily oust the German volume, which in an English translation holds the field at present simply because of the lack of enterprise shown by English publishers. The only reason why college students of botany have to use a thoroughly unsatisfactory text-book is, apparently, that paying a fee for the use of illustration blocks comes cheaper than having original ones made. Dr. Reynolds Green's *History of Botany from 1860 to 1900*, published a few years ago, is so extraordinarily good that we have never been able to see why it should have been necessary to restrict its scope and make it a mere "continuation of Sachs' *History of Botany, 1580–1860*" (to quote the sub-title). It is regrettable that Reynolds Green was not allowed to write a complete history of botany to replace, not merely to supplement, a previous work which, though admittedly good in parts, is marred by almost every fault from which an historical monograph should be free.

In the present volume, published after the lamented death of the author and prepared finally for the press by the pious and loving care of his friend and colleague Prof. Harvey Gibson, we have Reynolds Green at his best as an historian for here he had a free hand and a relatively limited though still wide enough scope. Botanists in this country have been in the past too much inclined to overrate the importance and value of botanical work done everywhere else under the sun. A humble frame of mind is in some respects a good one for the worker in science, and certainly a good deal of what is passed for publication by perfunctory

editors of botanical journals as original contributions to that science is original only to the authors who have failed to make themselves familiar with the literature of their subject—though this particular fault is perhaps more conspicuous in foreign than in British work; but it is about time to realise the fact that British workers have played a very important part in the making of modern botany, and have, indeed, as is freely admitted by unprejudiced experts in other lands, contributed a considerable majority of the publications that count for most in the progress of a science, namely, such as have led to the overturning of previously accepted views and have opened up new lines of investigation. In many cases, characteristically enough, it has been left to workers in other countries to pursue these lines and fill in the details, incidentally gaining credit for much more than this, and too often ignoring and even disparaging the pioneer labours which made their own possible.

The present volume is divided into eight "books," dealing with fairly natural periods into which the history of botany in this country may be divided. The first is concerned with the herbalists, whose works are too often regarded as a source of quaint quotations upon which silly compilers of popular books about plants may draw in order to eke out their blend of misinformation, bits of poetry, and pictures. As the author points out in the course of his account of the beginnings of botany, it was from the chaotic mixture of magic, astrology, and the healing arts that botanical science slowly emerged, and the foundation of modern botany was laid by the herbalists as they successively discarded first superstition and then the connection between botany and medicine, and began to study and classify plants, and herbals gave place to floras. The chief periods dealt with in the remaining "books" include the age of Gray, Morison, and Grew; the period dominated by the Linnæan system of classification, and by the founding of plant physiology in the eighteenth century (it is hardly an exaggeration to claim that plant physiology is essentially a British science); the revival of the natural system of classification, heralded by the brilliant work of Robert Brown and closing with the tremendous modern revival which followed the publication of *The Origin of Species*, and set in motion the "wave of progress" to which is devoted the last "book," bringing the story down to the end of the nineteenth century.

In this unique book the author has brought together and presented with his usual great literary skill an enormous mass of material, representing the result of many years of research in the field which he had so thoroughly and peculiarly made his own, and which, indeed, but for his labours had remained practically untouched. British botany could have found no abler or more reliable historian, and it is safe to assert that the story here presented will be read by generations of botanists who wish to learn something of the debt which modern botany owes to the labours of British workers from the herbalists of the sixteenth century onwards.

F. CAVERS.

The Evolution of Sex in Plants. By JOHN MERLE COULTER. [Pp. ix + 140, with 46 illustrations.] (Chicago: University of Chicago Press; London: Cambridge University Press; n.d. Price 4*s*. net.)

IN this volume Prof. Coulter has chosen to keep to eminently safe and easy, if already somewhat well-trodden, ground in dealing with the evolution of sex in plants, and for the non-biological reader it forms a sound introduction to the study of a biological problem which has in the past been left too exclusively to writers dealing with animals, in which matters are complicated by secondary and accessory characters of various kinds. He has wisely made no attempt to avoid

technical terms, for in this case they are absolutely essential to a comprehension of the homologies between higher and lower plants, but has given clear and full definitions of all the terms used. There are no facts or theories here which are not to be found in any good modern general text-book of botany, but nowhere have we seen these facts and theories presented in such a simple and skilful manner. The author has adopted the commonsense method of treating the evolution of sex in plants in relation to the progressive adaptation of plants to life on land, leaving aside all those difficult and abstruse questions connected with sex origin and sex differentiation which are still under close study and are the topics of equally active controversy. The result is a pleasantly written and therefore easily read little book, which certainly realises the intention of the series to which it belongs—to be serviceable "not only for the specialist but also for the educated layman." It will abundantly meet the needs of the latter class of reader, while as to the former we can but say that students of botany will find in it many interesting suggestions which may be profitably followed up, besides being helped and stimulated by the author's skilful presentation of principles which are apt to become obscure in the mass of detail encountered in the study of the evolution of plants.

The price of this little book seems somewhat high, for we have grown accustomed to pay one-fourth as much for books belonging to series of similar scope and intention. However, price is not everything, and if the forthcoming volumes in the series of which this is the first to be published ("University of Chicago Science Series") are of the same type, namely, dealing in a similar manner with a topic not covered by any other book published at a moderate price (and this certainly applies to the other botanical books announced for the series), they will meet with success even if it is not found possible to issue them at the modest price of one shilling, as in the case of the well-known "Cambridge Manuals of Science and Literature" or the "Home University Library."

F. CAVERS.

AGRICULTURE

Proceedings of the Third International Conference of Tropical Agriculture. Held at the Imperial Institute, London, June 23 to 30, 1914. Edited by the Honorary Secretaries. [Pp. xi + 407.] (London: John Bale, Sons & Danielsson, Ltd., 1914.)

EVEN in the old days before the war it needed an optimistic spirit to conduct an international conference, and when the subject of that conference was Tropical Agriculture, the spirit had need to be invincible. Ordinary agriculture is not a subject in itself, but a blend of many subjects, ranging from the purest pure science to the most hard-headed business. Tropical agriculture in its present stage of development has less science and more politics in its composition. The papers in these Proceedings range quite legitimately from sun-power engines to malaria. It is a very wide field, and our sympathy goes out to the plea for more specialisation in the matter of publications made by Mr. W. R. Dunlop (p. 384). The difficulty appears to have been felt acutely in this Third Congress, and some valuable suggestions are made in the report of the closing session by M. Leplae, the Belgian delegate, towards so organising future conferences that free discussion might be possible.

Under the circumstances of international disorganisation which followed this Conference at the short interval of five weeks only, we are grateful for obtaining any report at all; otherwise we should object that such a volume ought to con-

tain the full text of papers read, and not merely abstracts of them, while the less carefully considered statements of verbal discussion are reported in full. Even so, however, the wealth of subjects treated makes a volume of four hundred pages, which is a useful index to persons, and to the subjects in which they have specialised.

Discussion of the possibilities of a Technical College abroad, for training in Tropical Agriculture, occupied a notable part of the Congress's time, and a session was also devoted to the organisation of Research. This latter produced some pious aspirations, and the principal of Cirencester Agricultural College very rightly challenged the view that the problems of the Tropics are so very different in essentials from those of the temperate zones. In this, as in other parts of the report, the reader is left with the impression that many of the very important economic problems involved in tropical agriculture needs possibly less "science" as it is understanded of the people—long names—and more genuine scientific method in obtaining and dealing with honest facts, whether of cultivation or of manufacture. The sessions dealing with rubber show that even a comparatively new subject, not very deeply ingrained with precedent, can become very confusing. The pro-academic note sounded by Mr. T. Petch in this connection is refreshing.

Sometimes one thinks that all research in such matters ought to be handed over to universities. The proper study of governments is government, and the design of their organisation is primarily for executive purposes. "Where the password is MARCH, and not DEVELOPE, a body of men, to be a serviceable instrument, must consent to act as one." The different function of the scientist, whose economic *raison d'être* is development, is recognised by Dr. Van Hall of Java (p. 215), who distinguishes between the men doing investigation and those who apply the results. He further enumerates eight sciences as indispensable, which—allowing a qualified chief and one assistant to each—makes the scientific staff of an Agricultural Department add up to thirty-two trained men as the minimum. He is certainly right, but few British possessions are so well equipped as this with scientists to study and advise upon the development of their agricultural resources.

L. B.

The World's Cotton Crops. By JOHN A. TODD, B.L. [Pp. xiii + 460, with 32 page illustrations and 16 maps and diagrams.] (London: A. & C. Black 1915. Price 10*s*. net.)

THE purpose of this volume is to give an account of the sources from which the world's supply of raw cotton is derived, the account being presented in a form which shall be of interest and utility both to the grower of this important crop and to the user also, incidentally summarising the information which otherwise is only obtainable in a very large number of publications, of very various degrees of reliability, from all parts of the sub-tropical world.

The project was an ambitious one, but it has been carried out thoroughly and well, both by the author and the publishers. Those who are interested in the cotton trade are well aware of the unreliability of most published information concerning it; there is scarcely one modern book dealing with cotton which does not, for example, give the names of certain varieties as being now under cultivation in Egypt, which actually have been extinct for several decades; yet Egypt is the most accessible and compact of all the cotton countries. The present author has wisely submitted his proofs to experts on the subject of each

chapter, in order to avoid the last serious risk of such anachronisms, while his personal knowledge of Lancashire, of Egypt, and of the United States also has enabled him to give a living interest to the subject as a whole.

One feature of the book especially distinguishes it from its predecessors, being moreover one which other economists would do well to imitate, especially when dealing with portions of their topic wherein they can, of necessity, have but little personal knowledge. This feature is the statistical treatment given to the available data, which are brought together in an appendix running to some fifty pages odd. The individual value of such data varies greatly; some figures are accurate checks on every piece of cotton exported, bale by bale, from countries with no internal consumption of their crop; others are mere guess-work concerning an unseen crop, asserted by some commercial authority with a general knowledge of the circumstances. Thus, out of a total world's crop of 20·5 million bales in 1909–10, whereof more than one-half came from the United States alone, the crop of China is variously estimated from 4·0 to 1·2 million bales. Prof. Todd has taken the commendable, though infinitely tedious course, of setting out all the available authorities, figure by figure. This having been done, it at once becomes obvious that many such authorities are unreliable, and the direction which further investigation should take becomes clear.

The need for more accurate knowledge is obvious when we examine the figures of supply and demand. The demand for cotton goods is continually augmenting the supply, to such an extent that whereas the world's crop around 1908 was 20 million bales, that of the present period would have been 30 million bales, but for the war.

The most striking feature of the book, however, for those who would see the economics of British industries—whereof cotton is the largest—organised on scientific lines, is one which the author has wisely abstained from over-emphasising. It is the utter lack of balance shown by the trade in cotton, owing to the dominating influence of the United States crop. At the Cambridge meeting of the British Association in 1904, Mr. A. J. Balfour delivered an address on the economic danger resulting from this one-sided supply, and the British Cotton-Growing Association has been working since then to remedy it. This book not merely shows the potential danger of the situation, but has revealed the circumstances which actually exist, leading to an unhealthy lack of efficiency in the trade, when that trade is considered as a solid whole, consisting of Growers as well as of Spinners. This inefficiency will be doubly dangerous in the period after the war, which will tend to be one of alternate "boom" and bankruptcy. The United States crop is so large that its price controls the price of almost every other kind of cotton, except for some small and special crops, and the following is a rough outline of the vicious circle in which the cotton trade revolves:—

In a certain year the price of cotton is low. The small farmers in America, who grow cotton on a very narrow margin of profit, owing to the high labour-cost in the States, find it unprofitable; they decide to reduce their acreage in the following year. In this following year the demand has increased, as usual; the American crop is then insufficient, prices rise in America, and pull up prices all over the world with them. These high prices encourage the small farmers of America, who promptly decide to increase their acreage, with the result that in the year afterward there is over-production, the price of cotton falls all over the world, and the circle returns to its starting-point. Year in, and year out, this absurd oscillation has continued, above and below the mark of demand, but never hitting it.

Remedies have been proposed for stabilisation of the cotton trade; prohibition of gambling in "futures," establishment of cotton reserves, and the extension of cotton-growing in other countries, have all been suggested and acted upon with some result. There is, however, no dispassionate scientific knowledge at the back of these efforts, and the suspicion, often unfounded, that the other fellow is "out on the make" has always hampered them. Yet the economic waste resulting from the lack of such central co-ordination must be enormous.

The matter is one of peculiar interest to England, where the manufacture of cotton textiles by machinery was first evolved. There once was a time when we in England controlled this manufacture almost exclusively, and although we still have more spindles at work than any other country, although we still monopolise most of the fine-spinning trade, and although the cotton goods which we produce figure at the head of our list of exports, yet we do not now possess more than 40 per cent. of the world's total spindles. Again, about a quarter of the world's cotton crop is grown in British possessions, and Prof. Todd points out that it is likely this proportion will increase, India taking the place of the U.S.A. His plea for an organisation to be backed by capital equal to the amount which is now being spent on the war in a single day, is a plea worth serious consideration in Great Britain; otherwise, the other countries will do it.

The postscript chapter, dealing with the effects of the war, makes interesting reading; it plays with millions, and leaps from New York to Korea. The two preceding chapters on the uses of cotton and of cotton-seed contain the essence of the matter in readable form, and the book as a whole may be recommended not only to those who are intimately concerned with some aspect of the enormous trade it deals with, but even the casual reader will find some interest within its pages, such as the history of the development of the Nile Valley, or the list of sociological "limiting factors" which control the growth of cotton-growing projects, from deficiency of population in the Sudan, laziness of the nigger in the United States, and the supply of irrigation water in Russian Turkestan, down to a hankering after massacres in Asia Minor.

W. LAWRENCE BALLS.

MEDICINE

Preliminary Report on the Treatment of Pulmonary Tuberculosis with Tuberculin. By NOEL D. BARDSWELL, M.D., with a Prefatory Note by PROF. KARL PEARSON, F.R.S. [Pp. xxi + 141.] (London: H. K. Lewis, 1914. Price 6*s*. net.)

THIS is a valuable contribution to the literature on the debated question of the efficiency of tuberculin in the treatment of phthisis. The volume deals exhaustively with the observation of its effects on cases in the King Edward VII. Sanatorium, Midhurst, over a period of two years.

The earlier portions of the book are devoted to a description of the general technique of the administration of tuberculin. The physical examination of patients preparatory to treatment, the varieties of tuberculin used, the methods of inoculation, the dosage, and the manner of regulating "courses" of treatment are described.

The remainder of the volume is occupied with the details of the results obtained by the injection of tuberculin in a large number of patients.

The comparison is made between cases which were subjected to tuberculin treatment for six months in the Sanatorium, and cases in which no tuberculin was given for the same length of time. It is pointed out that the results must not

be taken as definitely settling the question at issue—namely whether tuberculin is curative or not—since it is possible that if the treatment were continued for a much longer time a greater immunisation might be obtained. It is claimed, however, that if tuberculin were as beneficial as some would have us believe, its good effects should at any rate be apparent within a few weeks, and this is not found to be the case.

The experience of the authorities at Midhurst appears to be distinctly unfavourable to the method of treatment. Tuberculin can in no sense of the word be regarded as a remedial agent. Its most obvious effect is that the patient may acquire a tolerance to considerable doses, but it is uncertain whether the tolerance is in any way beneficial. There is a warning note that tuberculin is not only inert in a proportion of cases, but may even be harmful. For this reason its indiscriminate use should be avoided.

The cases are arranged in groups for easy reference according to the Turban-Gerhardt system of classification. Details of the history of some fifty individual cases are given, and the volume contains twenty-two typical temperature charts.

A consideration of the value of tuberculin from a statistical review of the cases treated forms the subject of an interesting preface by Prof. Pearson, the conclusion being that tuberculin treatment is still in an experimental stage. In other words, statistics do not show that the use of tuberculin is any asset to routine sanatorium treatment.

The Report is a plain statement of observed facts derived from a large experience, and as such is presented to the unbiassed reader, who may form his own conclusions.

J. W. CROPPER.

Towards Racial Health: A Handbook for Parents, Teachers, and Social Workers on the Training of Boys and Girls. By NORAH H. MARCH, B.Sc., M.R.San.I., with a Foreword by J. ARTHUR THOMSON, M.A., LL.D. [Pp. viii + 326. With illustrations.] (London: Routledge & Sons. Price 3*s*. 6*d*. net.)

IN endeavouring to write a book, which would be of practical value to parents and teachers, on the very complex problem of the mental, moral, and physical training of children, the author has undertaken a task of great difficulty. However excellent the information and advice may be, the question as to whether these will be utilised properly depends on the competency of the reader. The writer recognises this, and suggests that organised meetings of parents for instruction on sex education of the child would be useful. Miss March rightly points out how important the well-being of the child is for the future welfare of the State. For this reason alone, any additional means to improve the training of children are welcome, and it is to be hoped that this volume will fulfil the good purpose for which it is intended.

One of the objects of the book is to guide parents and teachers as to the best way of giving information to the young on matters relating to the approach of puberty. It is also shown how to combat the inquisitiveness of youth, and how to guard against the wrecking of life which is brought about by exposure to evil surroundings and companionship.

The early chapters deal with the physical and mental development of the child. These are followed by suggestions on the care and supervision of children, an interesting illustrated chapter on Nature Study in the service of sex instruction, chapters on the Biology of Sex, Ethical Training, and Social Safeguarding.

Referencc is made to certain diseases which may adversely affect child life. It is, perhaps, unfortunate that the introduction of so much medical matter was found necessary. The lay reader would have been greatly assisted by a Glossary of scientific terms used in the text. Some of the appendices appear too technical for a popular treatise, and might possibly have been modified or omitted with advantage. The purpose of the author expressed in the book is worthy of praise, and the chapters are well written and instructive.

The writer has displayed great skill in discussing a delicate subject in language the diction of which may be commended. The principles of sex education on which the book is based should be valuable to those who can realise their importance, and who are prepared to act, to the best of their ability, in carrying them out.

J. W. CROPPER.

The Minor Horrors of War. By A. E. SHIPLEY, Sc.D., F.R.S. [Pp. xix + 178, with 64 illustrations. Second, revised and enlarged edition.] (London: Smith, Elder & Co., 1915. Price 2*s.* net cloth, and 1*s.* 6*d.* net paper cover.)

THE future of medicine has been rather aptly said to lie rather in prevention than in cure. While this is obvious enough to those with scientific knowledge, it is extremely difficult to bring home to the mass of the population the value of preventive measures in relation to insect-borne diseases. Nevertheless, the health not only of our troops in the field and in training, but also of our workers at home, depends largely on the adoption of various measures for the destruction of a number of pests, insects and others, that are concerned in spreading disease. Knowledge of the habits of such pests and of remedies against them is now within the reach of all. The information is presented in a simple and entertaining manner in Dr. Shipley's book, *The Minor Horrors of War*, which in a month has reached a large second edition—a sure index, both of its popularity and of its utility.

The book contains twelve well-illustiated chapters dealing with such parasites of the human body as lice and fleas, pests of dwellings such as bed-bugs, ticks, and flies, irritating animals like mites and leeches, and destructive insects that play havoc with the army biscuit and infest flour. The means of dealing with these minor horrors that almost invariably accompany the assembling of large bodies of men in relatively small areas are indicated clearly in each case.

Lice are among the most serious annoyances to men, and at times, when personal cleanliness cannot adequately be secured, are sources of great discomfort, if nothing worse. Irritation and broken rest are inevitable. Worse may occur. Lice are known to be the agents of spread of relapsing fever and of typhus fever. By rubbing or scratching, the lice are crushed on to the skin and the germs of disease (e.g. *Spirochæta recurrentis* of relapsing fever) are inoculated directly into the blood through the surface damaged by scratching. Similarly, the rubbing of the eye by a finger, soiled by a crushed louse, is another means of introducing the parasite into the body. As typhus and relapsing fever are endemic in certain areas of the eastern front of the present theatre of war, attention should be paid to the copious preventive measures against lice set forth in this book.

Plague is conveyed by fleas from infected rats to man. Hence, a knowledge of the life-history and habits of these insects is necessary. The commonest rat-flea in all tropical and subtropical countries, *Xenopsylla cheopis*—and to a lesser

extent, *Ceratophyllus fasciatus*—infests and bites man, and, should it have fed on a plague-infected rat, bubonic plague may be communicated to the victim. In certain parts of Africa and Asia, rats and rat-fleas, therefore, cannot be ignored.

Bugs in houses breed all the year, so long as the temperature is favourable and food abundant. In India, the bed-bug is strongly suspected of transmitting kala-azar, and it has also been incriminated in the dissemination of relapsing fever elsewhere. The methods given for the destruction of bugs should enable one to cope with the nuisance.

House-flies are mechanical agents in the transport of disease germs. Prominent among the diseases conveyed by them is typhoid fever. In Egypt, ophthalmia is very prevalent, and the eyes are often infested with flies. The contamination of food or drink with various disease germs, by the wanderings of flies from their favourite breeding grounds in filth, is a menace to the health of soldier and civilian alike. Similarly, the blowfly, the greenbottle fly, and the flesh-fly all are dangerous to man by polluting his food or even by depositing their eggs in wounds, where vigorous larvæ or maggots develop. An anti-fly crusade is indeed a necessary measure.

Harvest mites and itch insects are also nuisances, and can produce intolerable discomfort. The simple preventive measures set forth will doubtless be welcome to many in camp this year. Ticks will probably be familiar to troops in Africa and on the Persian Gulf. A knowledge of their habits and life-histories can be pleasurably obtained from this book. *Ornithodorus moubata* conveys *Spirochæta duttoni*, the causal agent of African tick fever in man, while other ticks transmit diseases causing great loss among cattle.

An interesting account of various leeches occurring in different parts of the world is given. The information about their habits should be of service to men in India and Ceylon, where voracious land leeches lie in wait for the unwary. Leeches in Syria, Palestine, and Egypt also frequent the stagnant water of wells, and, if swallowed with unstrained or unfiltered water, can cause severe suffering either to man or beast.

Finally, the facts set forth regarding these minor horrors of war, as given by Dr. Shipley, are well worth studying both at home and at the front. The book is well written, interesting, amusing, and instructive, and its popularity is well deserved.

H. B. F.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Engineering. By Gordon D. Knox. "Romance of Reality Series." London: T. C. & E. C. Jack, 67, Long Acre, W.C., and Edinburgh. With 17 Plates 1915. (Pp. xi + 276.)

Abstract of Proceedings of the Deputation from the Royal Society and the Chemical Society to the Government on the Position of Chemical Industries. May 6, 1915. From the Transactions of the Chemical Society, 1915. Vol. 107. (Pp. 13.)

Health in the Camp. A Talk to Soldiers. By H. R. Kenwood, Temporary Lieutenant-Colonel R.A.M.C., Professor of Hygiene and Public Health in the University of London, etc. London: H. K. Lewis & Co., 136, Gower Street, W.C. (Pp. vi + 58.) Price 3*d*. net. 100 copies, 21*s*. net; 50, 11*s*. net; 25, 5*s*. net.

Pro Lithuania. A Monthly Review. Published by the Lithuanian Information Bureau. No. 1, July 1915. Editorial Offices, 41, Boulevard des Batignolles, Paris, France. (Pp. 32.) Annual Subscription, 8*s*.

A Buoyant Jacket for the Prevention of Death by Submersion. By Fleet Surgeon C. Marsh Beadnell, R.N. Reprinted from the "Journal of the Royal Naval Medical Service," July 1915. London: John Bale, Sons & Danielsson, Ltd., Oxford House, 83-91, Great Titchfield Street, Oxford Street, W. 1915. (Pp. 5.)

The War and After. Short Chapters on Subjects of Serious Practical Import for the Average Citizen in A.D. 1915 onwards. By Sir Oliver Lodge, F.R.S., Principal of the University of Birmingham. London: Methuen & Co., Ltd., 36, Essex Street, W.C. (Pp. viii + 231.) Price 1*s*. net.

Introduction to Heat. By Arthur R. Laws, B.Sc., Senior Science Master, Royal Grammar School, Newcastle-on-Tyne, and George W. Todd, B.A., D.Sc., Science Master, Royal Grammar School, Newcastle-on-Tyne. With 106 Diagrams and Answers to the Problems. London: Mills & Boon, Ltd., 49, Rupert Street, W. (Pp. x + 212.) Price 2*s*. 6*d*. net.

The Fixation of Atmospheric Nitrogen. By Joseph Knox, D.Sc., Lecturer on Inorganic Chemistry, University of Aberdeen. Gurney & Jackson, 33, Paternoster Row, London, E.C. 1914. (Pp. 112.) Price 2*s*. net.

Surface Tension and Surface Energy and their Influence on Chemical Phenomena. By R. S. Willows, M.A., D.Sc., and E. Hatschek. Reprinted from the "Chemical World." With 17 Illustrations. London: J. & A. Churchill, 7, Great Marlborough Street, 1915. (Pp. viii + 80.) Price 2*s*. 6*d*. net.

A History of British Mammals. Parts XV., XVI., XVII., March 1914, November 1914, and July 1915. By Gerald E. H. Barret-Hamilton, B.A., M.R.I.A., F.Z.S. With many Full-page Plates in Colour, in Black and White, and Numerous Illustrations in Text, drawn by Edward A. Wilson, B.A.M.B., and Guy Dollman, British Museum of Natural History. London: Gurney & Jackson, 33, Paternoster Row, E.C., 1915. (Pp. 47 each Part.) Price 2*s*. 6*d*. each.

The Investigation of Mind in Animals. By E. M. Smith, Moral Sciences Tripos, Cambridge. Cambridge: at the University Press, 1915. (Pp. ix + 194.)

Life Histories of African Game Animals. By Theodore Roosevelt and Edmund Heller. With Illustrations from Photographs, and from Drawings by Philip E. Goodwin; and Forty Faunal Maps. Volumes I. and II. London: John Murray, Albemarle Street, 1915. (Pp. Vol. I. xxviii + 420, Vol. II. x + 798.)

Scottish National Antarctic Expedition. Report on the Scientific Results of the Voyage of the s.y. "Scotia" during the years 1902, 1903, and 1904. Under the leadership of William S. Bruce, LL.D., F.R.S.E. Volume IV.: Zoology. Parts II.-XX. Vertebrates by David Hepburn, M.D., F.R.S.E., and fourteen others. With 62 Plates, 31 Text Figures, and 2 Maps. Edinburgh: The Scottish Oceanographical Laboratory, 1915. (Pp. xi + 505.) Price 50*s*.

John Napier and the Invention of Logarithms, 1614. A Lecture by E. W. Hobson, Sc.D., LL.D., F.R.S., Sadleirian Professor of Pure Mathematics, Fellow of Christ's College, Cambridge. Cambridge: at the University Press, 1914. (Pp. 48.) Price 1*s*. 6*d*. net.

The Yearbook of the Universities of the Empire, 1915. Published for the Universities Bureau of the British Empire. London: Herbert Jenkins, Ltd., Arundel Place, Haymarket. [Pp. xii + 717.) Price 7*s*. 6*d*. net.

Fungoid Diseases of Farm and Garden Crops. By Thomas Milburn, Ph.D., N.D.A., N.D.D., Secretary of Agriculture, Lancashire County Council and Lecturer in Agriculture, Lancashire County Council Agricultural School, Harris Institute, Preston. With a Prefatory Note by E. A. Bessey, M.A., Ph.D., Professor of Botany, Michigan Agricultural College, East Lansing. With Diagrams. London: Longmans, Green & Co., 39, Paternoster Row;

New York: Fourth Avenue and 30th Street; Bombay, Calcutta, and Madras, 1915. (Pp. x + 118.) Price 2*s.* net.

Alcoholmetric Tables. By Sir Edward Thorpe, C.B., LL.D., F.R.S., late Principal of the Government Laboratory, and Emeritus Professor of Chemistry, Imperial College of Science and Technology, South Kensington. London: Longmans, Green & Co.; New York: Fourth Avenue and 30th Street; Bombay, Calcutta, and Madras, 1915. (Pp. civ + 91.) Price 3*s.* 6*d.* net.

Les Facteurs de la Guerre et le Problème de la Paix. By Eugenio Rignano. Extrait de "Scientia," Vol. XVIII., 93me Année, 1915. N. (XLII – 4). Bologna: Nicola Zanichelli. London: Williams & Norgate. (Pp. 47.)

The Present Position of the Theory of Organic Evolution. By Prof. Ernest W. MacBride, M.A., F.R.S. Being a Paper read before the Victoria Institute, at the 563rd Meeting, held February 1, 1915. (Pp. 32.)

The General Theory of Dirichlet's Series. Cambridge Tracts in Mathematics and Mathematical Physics, No. 18. By G. H. Hardy, M.A., F.R.S., Fellow and Lecturer of Trinity College, and Cayley Lecturer in Mathematics in the University of Cambridge, and Marcel Riesz, Dr.Phil., Docent in Mathematics in the University of Stockholm. London: Cambridge University Press, Fetter Lane, E.C., 1915. (Pp. 78.) Price 3*s.* 6*d.* net.

Selections from the Scottish Philosophy of Common Sense. The Open Court Series of Classics of Science and Philosophy, No. 2. Edited, with an Introduction, by G. A. Johnstone, M.A., Lecturer in Moral Philosophy in the University of Glasgow. Chicago and London: The Open Court Publishing Company, 1915. (Pp. vii + 267.) Price 3*s.* 6*d.* net.

The Science of Mechanics. A Critical and Historical Account of its Development. By Ernst Mach, Emeritus Professor of the History and Theory of Inductive Science in the University of Vienna. Supplement to the Third English Edition, containing the Author's Additions to the Seventh German Edition. Translated and Annotated by Philip E. B. Jourdain, M.A. Chicago and London: The Open Court Publishing Company, 1915. (Pp. xiv + 106.) Price 2*s.* 6*d.* net.

Contributions to the Founding of the Theory of Transfinite Numbers. The Open Court Series of Classics of Science and Philosophy, No. 1. By Georg Cantor. Translated, and provided with an Introduction and Notes, by Philip E. B. Jourdain, M.A. Chicago and London: The Open Court Publishing Company, 1915. (Pp. vii + 211.) Price 3*s.* 6*d.* net.

Numerical Examples in Physics. By H. Sydney Jones, M.A., Head Master of Cheltenham Grammar School, Late Head Mathematical and Science Master, University College, London, and Lecturer at Wien's; sometime Lecturer and Demonstrator in Natural Philosophy, King's College, London; formerly Scholar of Christ's College, Cambridge. London: G. Bell & Sons, Ltd., 1915. (Pp. xii + 332.) Price 3*s.* 6*d.* net.

Statics, Part II. By R. C. Fawdry, M.A., B.Sc., Sometime Scholar of Corpus Christi, College, Cambridge; Head of the Military and Engineering Side, Clifton College. London: G. Bell & Son, Ltd., 1915. (Pp. 145 + viii.) Price 2*s.* net.

Ordeal by Battle. By Frederick Scott Oliver. London: Macmillan & Co., Ltd., St. Martin's Street, 1915. (Pp. li + 437.) Price 6*s.* net.

The Poison War. By A. A. Roberts, Member of the Chemical Society of France, Member of the Society of Chemical Industry. With 10 Plates. London: William Heinemann. (Pp. 141.) Price 5*s.* net.

Calculus Made Easy. Being a Very-Simplest Introduction to those Beautiful Methods of Reckoning which are generally called by the Terrifying Names of the Differential Calculus and the Integral Calculus. By F. R. S. Second Edition, Enlarged. London: Macmillan & Co., Ltd., St. Martin's Street, 1914. (Pp. x + 265.) Price 2*s.* net.

The Tropical Agriculturist. Journal of the Ceylon Agricultural Society. Founded in 1881 by the late Mr. John Ferguson, C.M.G., F.R.A.S., F.R.C.I. April 1915. Vol. XLIV. No. 4. Peradeniya, Ceylon. Colombo: H. W. Cave & Co. (Pp. xii + 70.)

Elementary Studies in Plant Life. By F. E. Fritsch, D.Sc., F.L.S., Professor of Botany, East London College, University of London, and E. J. Salisbury, D.Sc., F.L.S., Lecturer in Botany, East London College, University of London. London: G. Bell & Sons, 1915. (Pp. xv + 194.) Price 2*s.* net.

This altogether admirable little book is practically a condensed and simplified edition of the same authors' *Introduction to the Study of Plants*, which was published last year and reviewed at the time in these columns. Apart from the excellent features which it shares with the larger work, the present volume is almost a miracle of cheapness, considering its attractive get-up and the large proportion of fine photographs included among its many illustrations. Physiology and ecology are kept well to the front and are presented in a singularly clear and attractive manner, and no better "first book in botany" could well be imagined.

The Theory of Proportion. By M. J. M. Hill, M.A., LL.D., Sc.D., F.R.S., Astor Professor of Mathematics in the University of London. London: Constable & Co., 1914. (Pp. xx + 108.) Price 8*s.* 6*d.* net.

A Treatise on the Analytic Geometry of Three Dimensions. By George Salmon, D.D., D.C.L., LL.D., F.R.S., late Provost of Trinity College, Dublin. Edited by Reginald A. P. Rogers, Fellow of Trinity College, Dublin. Fifth Edition, Vol. II.; Sixth Edition, Vol. I. London: Longmans, Green & Co., 39, Paternoster Row. New York: Fourth Avenue and 30th Street. Bombay, Calcutta, and Madras. Dublin: Hodges, Figgis & Co., 104, Grafton Street, 1914. (Vol. I., pp. xxiv + 470; Vol. II., pp. xvi + 334.) Prices, Vol. I. 9*s.* net, and Vol. II. 7*s.* 6*d.* net.

Handbook of Medical Entomology. By W. A. Riley, Ph.D., Professor of Insect Morphology and Parisitology, Cornell University, and O. A. Johannsen, Ph.D., Professor of Biology, Cornell University. Ithica, New York: The Constock Publishing Co., 1914. (Pp. ix + 348.) Price 2 dollars.

Amœbiasis and the Dysenteries. By Llewellyn Powell Phillys, M.A., M.D., B.C., F.R.C.P., F.R.C.S., Professor of Medicine in the Egyptian Government School of Medicine, Cairo; Senior Physician, Kasr-el-Ainy Hospital, Cairo; Physician-in-Ordinary to H.H. the Sultan of Egypt; Branch Medical Adviser, Gresham Life Assurance Society. London: H. K. Lewis, 136, Gower Street, W.C., 1915. (Pp. xi + 147.) Price 6*s.* 6*d.* net.

The Mutation Factor in Evolution. With Particular Reference to Œnothera. By R. Ruggles Gates, Ph.D., F.L.S., sometime Lecturer in Biology, St. Thomas's Hospital; Lecturer in Cytology, Bedford College, University of London; Huxley Medallist, Royal College of Science. London: Macmillan & Co., Ltd., St. Martin's Street, 1915. (Pp. xiv + 353.) Price 10*s.* net.

ANNOUNCEMENTS

Royal Meteorological Society. Meetings, 7.30 p.m., November 17, December 15.

Royal Astronomical Society Meetings, 5 p.m., November 12, December 10.

Royal Horticultural Society. Exhibition of Fruit and Annual Conference of Affiliated Societies, 3 p.m., October 6; Exhibition of Flowers, Fruits, etc., with Lecture at 3 p.m., October 12, 26, November 9, 23, December 7; Medical Exhibition, October 18 to 22; Exhibition of Flowers, November 11, 12, December 8; London Cage Birds' Association Show, December 10 to 13.

British Science Guild. Meetings of the Executive Committee, 3 p.m., October 19, November 16, December 7.

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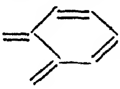
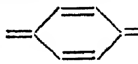
COLOUR AND CHEMICAL STRUCTURE

By S. C. BRADFORD, B.Sc
The Science Museum, South Kensington, London

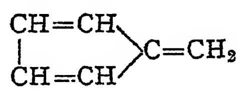
Sir I. Newton showed that white light is not a simple colour, but can be split up by means of a prism into the spectral colours : red, orange, yellow, green, blue, indigo and violet. White light is therefore merely the colour of sunlight and probably owes its apparently homogeneous character to the fact that it is the average colour of the light that enters our eyes under normal conditions.

With the exception of luminous and fluorescent bodies, the colours of the objects we see around us are not due to any power which these possess of creating light of a particular colour, but are produced by the absorption of certain constituents of sunlight and the transmission or scattering of the rest. Absorption may either be distributed generally through the spectrum, or it may be selective and confined to distinct regions. In the case of selective absorption, the " absorption bands," as they are termed, may occur in the visible spectrum, or in those regions extending beyond the range of vision known as the " infra-red " and " ultra-violet." Only those substances which exert strong selective absorption within the limits of vision appear to be coloured, but in considering the relations between light absorption and chemical constitution it has been found necessary to extend the study to the invisible regions of the spectrum which affect a photographic plate. An apparatus known as a spectograph invented by Sir William Hartley is used for this purpose. It consists essentially of a spectroscope attached to a camera with prisms and lenses of quartz instead of glass which would absorb the ultra-violet rays.

The first observations on the chemical structure of light-absorbing bodies were naturally confined to visibly coloured substances, and the earliest suggestion as to the cause of colour in organic compounds was made by Graebe and Liebermann in

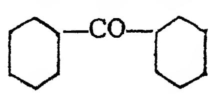
1867, who showed that all coloured bodies contain elements in the unsaturated condition. Ten years later Witt suggested that the development of colour is due to the presence of certain groups which he termed "chromophores," of which the most important are: C=C, C=O, C=S, C=N, N=N, N=O, and $\mathrm{N}\begin{smallmatrix}\diagup\mathrm{O}\\ \diagdown\mathrm{O}\end{smallmatrix}$ To these Nietzki added the ortho- and para-quinoid radicals,
 and , in 1888.

Colour production appears to be dependent on the compact linking of several chromophoric groups, the presence of a single chromophore being without effect. For example, the hydro-carbons of the ethylene series, such as diphenylethylene, $C_6H_5 . CH : CH . C_6H_5$, are colourless, but when several ethylenic linkages are closely associated colour appears, as in diphenylhexatriene, $C_6H_5 . CH : CH . CH : CH . CH : CH . C_6H_5$, which is yellow. Ring structure appears to be particularly favourable to the development of colour. This is exemplified by fulvene,

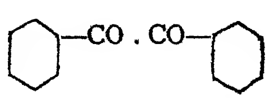


and its derivatives, which are yellow or red. From this it might naturally be expected that the hydro-carbon benzene would be coloured, as it is isomeric with fulvene and contains the same number of double linkages. Although this substance appears at first sight to be an exception, when we come to study the ultra-violet absorption spectrum of benzene we find a number of remarkable bands in this region.

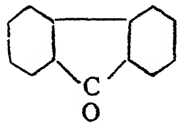
As in the case of the ethylenic linkage, the presence of a single carbonyl group is insufficient to produce colour, so that acetone, $CH_3 . CO . CH_3$, and benzophenone,



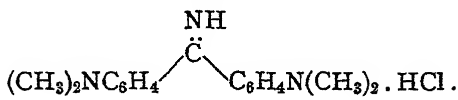
are colourless. When, however, the molecule contains two or more such groups closely linked, as in diacetyl, $CH_3 .CO .CO .CH_3$, and benzil,



a yellow colour appears. The effect of ring structure in intensifying the colour may be illustrated by comparing colourless benzophenone with red fluorenone,



The beautiful dyestuff auramine,

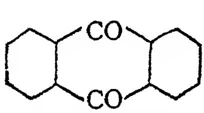


is a good example of the chromophoric effect of the $N=C$ group.

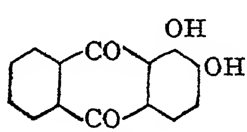
The nitro group is somewhat anomalous. It appears to act as a weak chromophore and to be dependent for its effect mainly on its association with other groups. When two nitro groups are present they often weaken one another in their chromophoric action. For instance nitrobenzene is yellow while dinitrobenzene is colourless.

Coloured substances do not necessarily act as dyes, and Witt showed in 1894 that the presence of a salt-forming group is required to convert such a substance into a dyestuff. Bodies which contain a chromophore without a salt-forming group have been called "chromogens," and the introduced dye-forming radicle, whose presence often intensifies the colour, is termed an "auxochrome."

The two chief auxochromes are OH and NH_2. The effect of the former may be illustrated by comparing yellow anthraquinone with its dihydroxy derivative, the beautiful red dye alizarin :

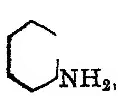


Anthraquinone.

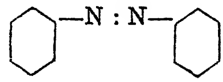


Alizarin.

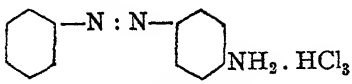
Anilin,



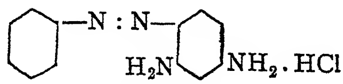
contains the auxochrome NH_2 but is itself colourless, while azobenzene



is an example of the chromophoric effect of the azo group. By the addition of amino groups this substance is converted into dyes such as:

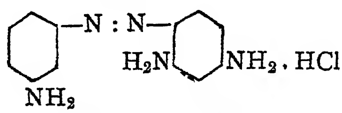


Anilin Yellow.



Chrysoidine.

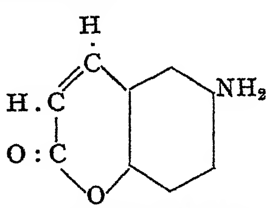
and



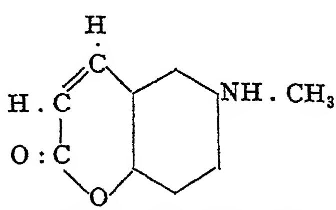
Triamido-azobenzene.

a constituent of Manchester brown.

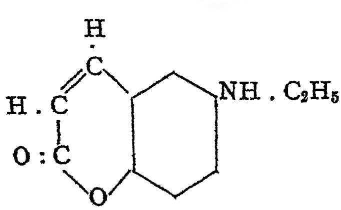
The effect of substitution in the auxochrome is to shift the absorption bands towards the red, namely to deepen the colour. This is well illustrated by the substance coumarin, the sweet-smelling principle of woodruff and hay. Coumarin is a colourless chromogen containing the compound keto-ethylenic chromophore, $-CO.CH=CH-$. The addition of unsaturated amino auxochromes such as NH_2, $NH.CH_3$, $NH.C_2H_5$ and $N(CH_3)_2$ causes the development of tinctorial properties which deepen with increasing molecular weight of the substituent:



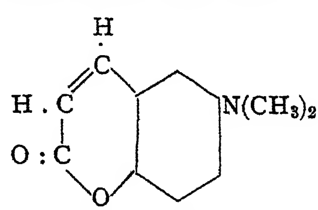
6. Amino-coumarin (pale yellow).



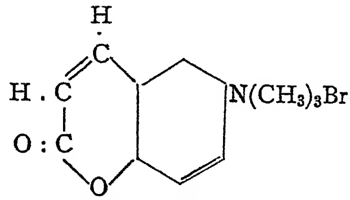
Methyl-6. Amino-coumarin (yellow).



Ethyl-6. Amino-coumarin (brown).



Dimethyl-6. Amino-coumarin (brown).

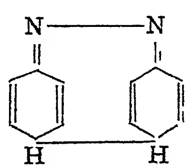


6. Coumaryl-trimethyl-ammonium bromide (colourless).

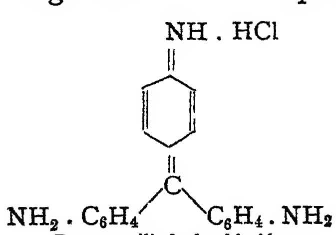
But when the amino group becomes saturated as in 6.coumaryl-trimethyl-ammonium bromide, above, and in the salts of the amino derivatives with mineral acids, the colour disappears.

That the chromophoric effect of auxochromes is due to their unsaturated character was pointed out by Kaufmann, who showed that the introduction of these groups into the molecule excites not only colour, but a number of other phenomena, such as increased luminescence of the vapour under the Tesla discharge and magneto-optical anomaly. He suggested that this may be due to the reaction of their residual affinities on those of the benzene nucleus, which is regarded as the seat of the phenomena, and which tends to assume the quinoid, or D condition.

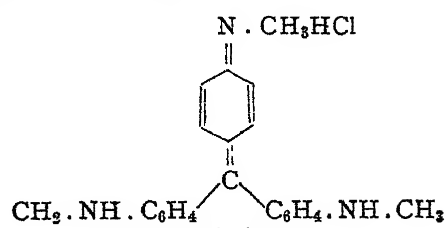
The ortho- or para-quinoid structure has so frequently been observed in the strongly coloured dyes that some chemists have assumed that all coloured substances possess this constitution. Armstrong, one of the chief exponents of this theory, proposed the formula



for azobenzene in 1882. In 1888 Nietzki suggested the quinone configuration for the triphenylmethane dyes such as :

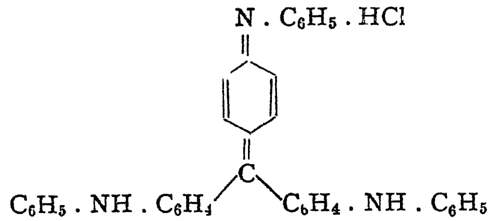


Pararosanilin hydrochloride.



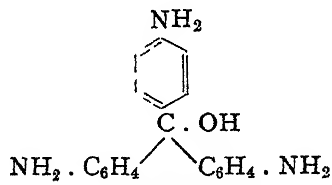
Hofmann's violet.

and



Aniline blue.

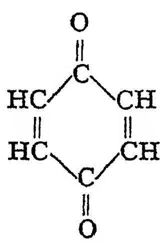
In the colourless



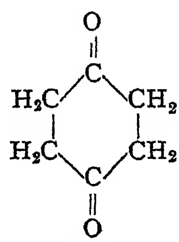
Pararosanilin base.

the quinoid structure is absent.

To account for the occurrence of colour in aliphatic substances, Armstrong subsequently modified his views and suggested that unsaturated groups or atoms might act as light-absorbing centres and produce colour by their co-operation. In most cases three such centres must be present to cause a pronounced colour. Bodies with the quinoid configuration, for example quinone itself, possess four of these centres : the two ethenoid linkages in the nucleus and the two C=O groups. When less than three are present, as in *p*-diketohexamethylene, no colour is seen :



Quinone



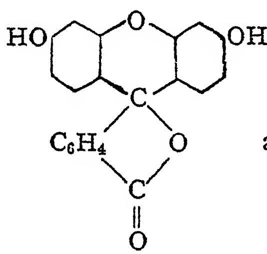
p-diketohexamethylene.

Aliphatic compounds which possess a sufficient number of light-absorbing centres are coloured, as for example iodoform, CHI_3, with three iodine atoms. If one of the iodine atoms is replaced by hydrogen we have the colourless body methylene iodide, CH_2I_2. Such coloured aliphatic substances have been

called pseudo-quinones. In this form the hypothesis is not very different from the theory of chromophores.

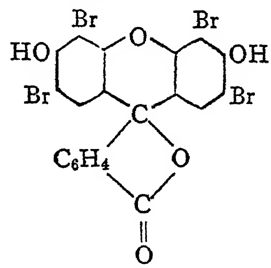
Hartley was the first to study the ultra-violet region of the spectrum, and in 1887 he showed that the benzene spectrum exhibits a number of narrow bands in this portion. He suggested that absorption is caused by the vibrations of intra-molecular particles synchronising with those of the incident light waves, and that by the introduction of certain groups such as Witt's auxochromes, or by the fusion of benzene nuclei, the vibration period is slowed and the absorption bands shifted towards the red. The residual affinity, if any, of the substituent also alters the character of the absorption bands, and Formánek has shown that colouring matters of the same class possess similar absorption spectra.

The effect of the mass of the substituent in slowing the molecular vibrations is exhibited in the case of



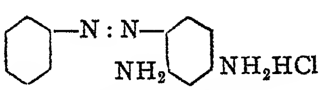
Fluorescein (yellow).

and

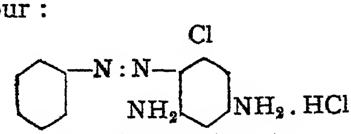


Eosine (orange).

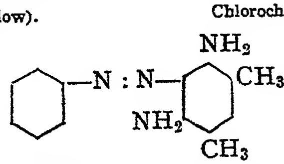
The former has an absorption band in the blue region of the spectrum, producing a yellow colour ; the introduction of four bromine atoms shifts this band towards the green so that eosine appears reddish. The addition of a chlorine atom, or of two methyl groups, to chrysoidine or related substances has the effect of deepening the colour :



Chrysoidine (yellow).



Chlorochrysoidine (orange).

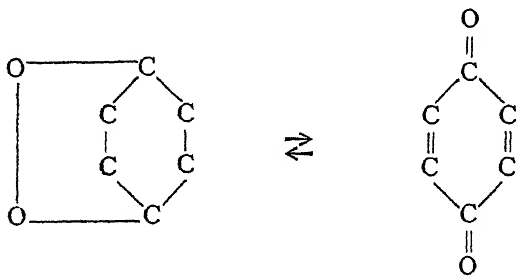


Benzene-5. azo-4 : 6. diamino- 1 : 3. xylene orange).

Following Hartley's work, Stewart, Baly, Desch and others have shown that light absorption may be due to tautomeric changes taking place within the molecule, as between the forms :

$$-CH_2-CO- \rightleftarrows -CH=C(OH)-$$

or



Which explains why absorption is not produced by the presence of a single unsaturated group. And from Drude's researches it is evident that Hartley's vibrating particles are subatomic and correspond to the valency electrons. The relations between absorption and chemical composition must therefore be sought in the dynamic condition of the valencies of the absorbing groups.

ON THE LIÈSEGANG PHENOMENON IN GELS

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The observation was made by Liesegang in 1896 that, when a drop of silver nitrate solution is placed on a gelatin film containing potassium bichromate, the silver bichromate formed is not distributed throughout the gelatin, but is deposited in concentric rings around the drop. The phenomenon has been studied by a number of subsequent investigators in the hope of throwing light on the structure of gels. In later researches the gel is usually contained in a test-tube, and the solution of reacting substance poured on top. The precipitation takes place in strata extending down the tube. Ostwald explained the formation of rings as the result of the sudden precipitation of a supersaturated solution of silver bichromate. Bechhold made a number of experiments in 1905, and showed that, in the case of ammonium bichromate and silver nitrate, the precipitate is more soluble in the ammonium nitrate resulting from the reaction, and suggested that the ammonium nitrate formed might prevent further precipitation in its immediate neighbourhood. In a lecture at the Royal Institution in 1912 Sir J. J. Thomson proposed the theory that the soluble substances produced might act in a similar way to certain bodies known as stabilisers which prevent the precipitation of colloidal solutions. By diffusion into the gel the concentration of these stabilisers would be diminished and further deposition of the precipitate allowed.

Liesegang showed in 1907 that the intermediate spaces between the rings contain no bichromate. Hatschek made a great many experiments in 1912, using a number of fresh compounds. He showed that the particles of the precipitates produced are much larger than could be obtained in aqueous solution, and explained, by his results, many features of the occurrence of gold in quartz. Some further experiments were

recently commenced by the author. And it is suggested that the phenomenon is due to adsorption, by the precipitate, of the substance dissolved in the gel, which serves merely to retain the precipitate in place.

Considering the surface bounding two phases, such as a solid and a liquid, Gibbs has shown that the concentration, at the surface, of a dissolved substance, will be increased, if thereby the surface tension is reduced, in accordance with the formula $U = -\frac{C}{RT}\frac{d\sigma}{dC}$, where U is the concentration at the surface, C that in the bulk of the liquid, and σ the surface tension. This effect is well known in the filtering of solutions through fine powders, where the dissolved particles are retained by the solid, as in Dreaper and Davis' Night-blue experiment. In the case under consideration the properties of the gel resemble those of a liquid. The precipitate formed by the interaction of "reagent" above and the "solute" in the gel will create a solid boundary surface between the liquid and the gel at which the concentration of both dissolved substances will increase. If the reagent is hypertonic to the gel, its surface concentration will be in excess of that of the solute, and the reagent will diffuse into the gel. With an hypotonic reagent the concentration of the solute will be the greater, so that the reagent will be precipitated as it collects at the surface, and will be unable to pass into the gel. This is in accordance with Pringheim's observation in 1895. Taking the case of the hypertonic reagent, formation of a precipitate will increase the surface, at the same time destroying the solute at the surface. Consequently there will be a rush of the particles of the solute towards the growing surface. If the solute is sufficiently dilute, it will presently happen that the layer of the gel next the precipitate will become practically devoid of dissolved substance, when the formation of the first zone will cease. The reagent diffusing through will pass on until it meets with more of the solute, where the production of a fresh layer will commence. From this it follows that there will be none of the solute remaining between the layers. So that if the Liesegang experiment is reversed, and potassium bichromate poured upon a gel impregnated with silver nitrate, the intermediate spaces between the rings will not blacken on exposure to light. Since the soluble products of the reaction will be more concentrated at the solid zones, it would appear

that the prevention of precipitation in the intermediate spaces is not due to their action.

Owing to the increasing dilution of reagent and solute, the layers get more diffuse, and increase in distance apart, as they are formed down the tube. In one experiment with silver bichromate in gelatin, the distances between the strata were: ·37, ·39, ·415, ·5, ·505, ·585, ·6, ·615, and ·885 mm. These approach to a G.P. with $r = 1{\cdot}068$, the terms of which are: ·37, ·395, ·42, ·45, ·51, ·55, ·58, ·63, and ·67. As the strata form at the rate of two or three in twenty-four hours, the irregularities are probably due to variations of temperature.

To compare the distance between the layers with the strengths of the reacting substances, experiments were made with 1 per cent. agar gels containing: (1) $\frac{N}{80} CaCl_2$, (2) $\frac{N}{50} CaCl_2$, (3) $\frac{N}{70} Na_2CO_3$, and (4) $\frac{N}{20} Na_2CO_3$. About 2N solution of sodium carbonate was poured on (1) and (2), and $\frac{N}{10}$ calcium chloride upon (3) and (4). The distances apart of the strata were:

(1)	(2)	(3)	(4)
0·57	0·35	0·65	0·15
0·73	0·45	0·65	0·25
1·0	0·55	0·85	0·225
		0·95	0·225
			0·275
			0·225
			0·5
			0·4
			0·55

These are, roughly, inversely proportional to the molar strength of the gel, and, within the limits of the experiment, independent of the strength of the reagent. The distances should also be dependent on the ratio $\frac{U}{C}$.

Probably by preventing the adsorption of the solute, a too great concentration of the reagent results in the formation of a continuous precipitate. If the reagents react without the formation of a precipitate, diffusion takes place regularly, in either direction. Strong ammonium hydrate was poured on a gel containing copper sulphate: the blue colour gradually diffused

both ways, until, after forty-eight hours, the solution and gel were the same colour throughout.

To determine whether the by-product of the reaction had any appreciable influence on the formation of layers, tubes were started, (1) and (2) containing 20 c.c. of $\frac{N}{20}Na_2CO_3$ in 1 per cent. agar gel, and (3) and (4) with the same strength $Na_2CO_3 + \frac{N}{20}NaCl$. Five c.c. of dilute calcium chloride were poured on each. Diffusion proceeded at the same rate in each tube throughout the experiment, with the formation of distinct though rather faint strata at distances below the top of the gel, in mm., as follows:

(1)	20·5	22·5	25	28	32†	35
(2)	20·5	22·5	25·5	28·5	32·5	36
(3)	20·5	22·5	24·5	—	33†	35·5
(4)	20·5	23	—	—	31·5†	35·5

The layers marked † were more pronounced. In another experiment 15 c.c. of $\frac{N}{5}K_2CrO_4$ were poured on top of (5) 15 c.c. $\frac{N}{200}Pb(NO_3)_2$ agar gel, and (6) the same strength agar gel containing in addition $\frac{N}{100}KNO_3$. After nine days numerous well-marked strata had been formed very close together. The average distances of the last twenty layers were 0·70 and 0·67 mm. respectively. Doubling or trebling the amount of the by-product of the reaction therefore appears to have little or no effect on the formation of the strata.

A convenient method of performing the experiment is to fill a U-tube rather more than half full with pure agar gel, and pour reagents of known strength into each limb. In one case 5 c.c. of $\frac{N}{10}K_2CrO_4$ were placed in one limb, and an equal measure of $\frac{N}{5}Pb(NO_3)_2$ in the other. The reagents gradually diffused through the gel, the progress of the diffusion being indicated by a slight opalescence of the gel containing the lead salt, and by a yellow colour in the other limb. At the point of meeting, a distinct, though somewhat faint, disc of lead chromate was formed. The ratio of the average distance of the outer and

inner edges and centre of the disc, from the surface of the gel impregnated with lead nitrate, to that of the corresponding measurements from the other surface, was 1·335; while the ratio of the square roots of the molecular weights of the ions is 1·34. At the moment of formation of the first disc, the yellow colour of the chromated gel was apparently uniform throughout, except within few mm. of the disc, where it was paler. The lead nitrate, being hypertonic, diffused further in the direction of the potassium chromate with the formation of eight successive discs, approximately 1·15 mm. apart. The ninth and following discs were of basic chromate approximately 0·9 mm. apart. The striking fact was observed that, during the formation of discs in that part of the tube where the colour had previously been uniformly yellow, the layer of gel next to the precipitate was always colourless, affording occular proof of the depletion of chromate from that portion of the gel.

An additional feature was noticed in the case of a test-tube prepared with 20 c.c. $\frac{N}{20}K_2CrO_4$ agar on which 15 c.c. of $\frac{N}{5}Pb(NO_3)_2$ were poured. This gel had been strained through muslin instead of filtering, so that it was slightly cloudy. After twenty-four hours a clear space appeared below the precipitate, as if the cloudiness had begun to subside. This space grew to a depth of 4·75 mm. in eight days. At the same time the yellow colour of the gel adjacent to the precipitate gradually became paler. That the gel was still solid was evinced by its power of supporting the growing precipitate. And it appears that the clarification was caused by the breaking up of the agglomerated portions of the gel by the rapidly moving chromate particles.

Since NaCl is negatively adsorbed, U-tubes were prepared containing, respectively, $\frac{N}{10}NaCl + \frac{N}{20}AgNO_3$ and $\frac{N}{10}AgNO_3 + \frac{N}{20}NaCl$, separated by 1% agar. At first only a diffuse precipitate was formed in the latter case, while two clear discs resulted in the former. But later, owing to adsorption of $AgNO_3$, the $\frac{N}{20}NaCl$ began to diffuse into the $\frac{N}{10}AgNO_2$ with the production of distinct strata.

THE IRON-BACTERIA

By DAVID ELLIS, Ph.D., D.Sc., F.R.S.E.
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Among the higher bacteria is to be found a certain class of organisms that almost always inhabit ferruginous waters. As a result of their growth and multiplication, their mucilaginous outer membranes become impregnated with ferric hydroxide. This gives them a rusty red appearance, and on this account, and because of their constant presence in iron-laden waters, they have been named Iron-bacteria by Winogradsky. Now part of the iron in these waters is in solution in the ferrous form, and the changes for which the iron-bacteria are responsible consist in a transformation of these soluble ferrous compounds into the insoluble ferric hydroxide. If a microscopic examination be made of the red deposit which forms the bed of ferruginous streams, or the deposit which rings the mouth of ferruginous springs, in nine cases out of ten the deposit will be seen to consist of a multitudinous number of small hollow tubes such as are figured in Plate I. These tubes are the remains of Iron-bacteria. During their lifetime the enveloping sheath which each individual possesses becomes impregnated with ferric hydroxide, which is an excellent preserving medium, with the result that when the organisms die the sheath which covered them does not disintegrate, and in time their number becomes so great that the bed of the stream is coloured a deep red and in many cases a thick deposit is formed. The majority of these sheaths originally formed a covering to *Leptothrix ochracea* (syn. *Chlamydothrix ochracea*), the best known and most widely distributed of the Iron-bacteria ; others, however, may have covered other Iron-bacteria such as *Cladothrix dichotoma* or *Crenothrix polyspora*. A more detailed description of the sheaths will be given when we come to consider the individual species. It will suffice here to state that in the living organism the sheath is at first a delicate pliable covering

which gradually grows into a thicker and stiffer membrane as it becomes impregnated with ferric hydroxide. When the protoplasm dies the sheath sinks to the bottom and contributes its mite to the general stock.

To observe *Leptothrix ochracea* in the living condition the ochre-waters are kept under observation, particularly at the close of winter and during the autumn months. There will then be found fluffy brown-red streamers waving in the water, and attached to bits of grass or to stones under the water. These will probably be living threads of *Leptothrix ochracea*, and we will describe it first.

Leptothrix ochracea (syn. *Chlamydothrix ochracea*) (Kützing)

The cylindrical, unicellular threads of Leptothrix are easily recognisable. The ends are rounded and the membrane, or sheath, is a delicate covering which a judicious staining renders visible. The threads measure from 1 μ[1] to 2 μ in breadth according to the age and to the amount of deposition which has taken place (fig. 1). Old threads may show a measurement of 3 μ or about double the average width. This happens when the deposition of ferric hydroxide has been very active. Reproduction takes place by fragmentation, small or large portions being liberated by a process of constriction. The liberated portions grow in length and repeat the process. When this takes place the length of the thread is not very great, but when the period of fragmentation comes to an end the threads elongate and may then reach a length of 200 μ or more. Another method of reproduction is by the formation of *conidia*. A little swelling is formed on the outer surface, and when this reaches a certain size a process of constriction takes place resulting ultimately in the liberation of a small oval cell, the conidium. These conidia are formed over the whole surface. Germination takes place, probably by direct elongation, and a new thread is formed. Pure cultures have been obtained in artificial media by Molisch. In such cultures conidia formation does not take place, but there is instead a liberation of "Schwärmer." These are short threads which have evidently

FIG. 1.—*Leptothrix ochracea*; ×2,000.

[1] 1 μ = 1/1000 mm.

not arisen by fragmentation, but have sprung out laterally from the Leptothrix thread, and assumed motility as soon as they have been cast loose. There seems little doubt that these motile threads developed each as a protuberance as conidia do, but, instead of being cut off to form conidia, continued their elongation until a certain length was reached, when their liberation from the parent thread was effected by the setting in of a process of constriction.

Gallionella ferruginea (Ehrenberg)

In nature we scarcely ever find Leptothrix without at the same time meeting a peculiar organism in the shape of a long, flexible, thin thread which from its earliest stages shows a marked tendency to coil one end of the thread round the other. Examples are shown in fig. 2 and Plate II. This coiling tendency is shown in the very youngest and ceases only when deposition of ferric hydroxide, by stiffening the periphery, and old age by the loss of capacity, make further coiling impossible. The process must be regarded as a case of contact-irritability, not essentially different from the irritability of the higher plants which enables tendrils, for example, to climb round their supports. In thickness the threads vary from $\frac{1}{2}\,\mu$ to $1\frac{1}{2}\,\mu$. Migula claims to have found a delicate membrane limiting the organism on the outside, but neither Adler nor the writer has been able to verify his statement. It certainly is not present in older threads impregnated with iron, when, as in the case of Leptothrix, if present it would be a fairly well marked structure.

FIG. 2.—*Gallionella ferruginea*; × 1,000.

Multiplication takes place as in Leptothrix by fragmentation, small portions of coiled threads being cast loose into the surrounding water. These elongate and coil until the normal size is attained. The writer has also observed a method of conidia formation in Gallionella which is identical in all essentials with the same process as observed in *Leptothrix ochracea*. Sometimes the deposition of iron results in a complete obliteration of the form of the organism. In such cases the deposition increases the breadth of the organism to two or three times its normal dimensions.

PLATE I.—Sheaths of *Leptothrix ochracea*, × 500

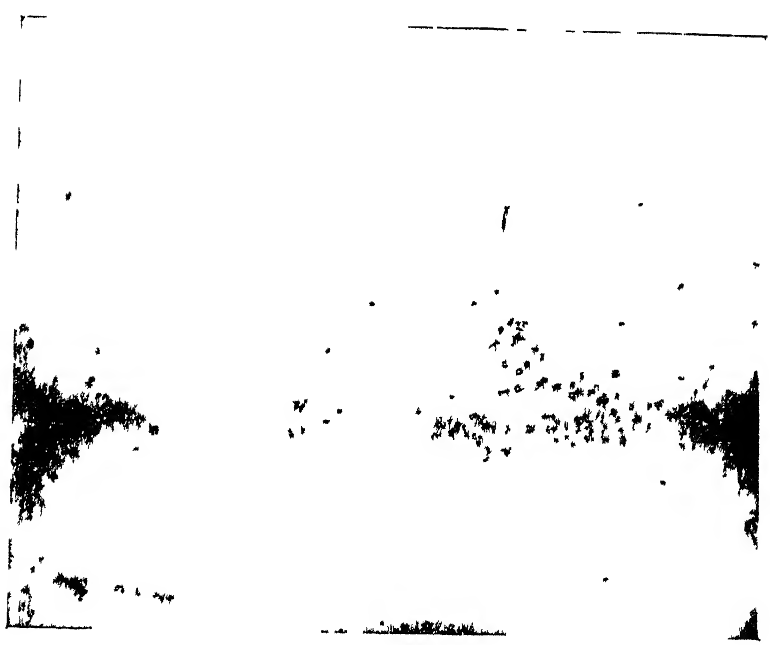

PLATE II.—*Gallionella ferruginea*, × 450

Spirophyllum ferrugineum (Ellis)

A new genus of Iron-bacteria was found by the writer in the neighbourhood of Glasgow. Like Gallionella it exhibits the coiling habit, but differs from this organism in that the body takes the form not of a thread, but of a flat band. An example is shown in Plate III. The presence of two individuals coiled round each other is not an infrequent aspect in cultures of this species (fig. 3, *B*). Multiplication takes place as in the

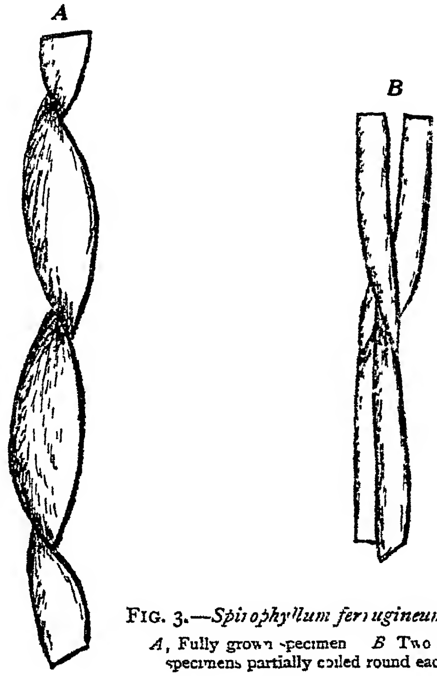

FIG. 3.—*Spirophyllum ferrugineum*; × 1,500.
A, Fully grown specimen *B* Two half-grown specimens partially coiled round each other.

two preceding organisms, by fragmentation and by conidia-formation. Like these two also its power of collecting ferric hydroxide round itself is very considerable. As seen in Plate III in the figure to the left, the deposition of iron has completely obliterated the form of this organism. The other figures in the same plate show that there is much variety in the number of twists, in the width of the bands, and in the lengths of the individual twists. The bands vary in width from 1 μ in the youngest forms to about 6 μ in the fully matured specimens. The thickness is, roughly speaking, from a sixth

to a tenth of the width. The youngest specimens that were observed consisted of very thin half-twisted bands measuring $1\,\mu$ in width and from $4\,\mu$ to $6\,\mu$ in length. These exhibited an independent movement, partly of a trembling and partly of a pendulum nature. The deposition of ferric hydroxide, however, seems to effect an early stoppage of this movement. The fully grown specimens (see central figure in Plate III) may show thirty to fifty twists, and are correspondingly wider and thicker. Except in the very youngest individuals the colour of the bands is rusty red and assumes a deeper tint with increasing age. Since the discovery of this organism its presence has been observed in various parts of Great Britain and lately in America also. On the Continent its presence has not so far been recorded, though there can be little doubt that a search there for it would be attended with successful results.

Inter-Relationships of Leptothrix, Gallionella, and Spirophyllum

An intimate study of these three organisms engenders the thought that perhaps they are more closely connected than appears on the surface. With regard to Spirophyllum, Molisch maintains that it is identical with Gallionella. This view, however, is based on the superficial resemblance which undoubtedly exists between old iron-laden bands of Spirophyllum and similar threads of Gallionella. An accidental resemblance between the "corpses" of these two organisms cannot, however, weigh against the fact that during life from their earliest beginnings the band-shaped structure of the one and the thread form of the other are respectively the distinctive features of the two organisms. Molisch apparently presumes that Spirophyllum is identical with dead Gallionella threads, the spaces between the coils of which have become filled with ferric hydroxide, thus converting the coiled threads of this organism into a band-shaped structure. We must look deeper for an insight into the relationships of these forms. It is noteworthy that Gallionella is never encountered except in the presence of Leptothrix. The same is very nearly true of Spirophyllum. In fact their association is too close not to suggest a very close relationship. Add to this that in their methods of reproduction, in their powers of attracting iron, and in every other respect, except in the shape they assume, these three organisms are identical. Some biologists, like

Schwers, have already suggested that Leptothrix and Gallionella are probably identical. The writer is of the same opinion, and considers that Spirophyllum, Gallionella, and Leptothrix will probably in the future be discovered to be one highly pleomorphic genus. If so, whatever the factors are which determine the shape, they are brought to bear in the very earliest phases of the existence of each individual. At present, however, we do not possess any evidence in support of the above hypothesis, and we can give expression to what at the present is a matter of feeling and not of fact.

We are now about to discuss three organisms which likewise form a natural group. They are of greater interest to water-engineers and to medical officers of health than the preceding on account of their tendency to spasmodic immoderate multiplication causing a serious hindrance to the flow of water through pipes. Their influence on water is not of a pathological nature, no poisonous excretions being liberated as a result of their growth; but they seriously inconvenience the engineer and give a little anxiety to others who are concerned to see their water reservoirs assume a rusty red colour. We shall not anticipate here the discussion on the physiology of these bacteria further than to state that as a result of their activity the iron which is in solution is precipitated, causing a deposition of ferric hydroxide on all substances with which the water comes in contact. When a certain at present unknown sum-total of conditions holds, these organisms multiply at an extraordinary rate, with the result that the number in the water of individuals each coated with ferric hydroxide is so great that the whole mass of water in a very short time takes on a very disquieting rusty red colour. These sudden rapid multiplications never last long because the organisms themselves probably make the water unfit for their continued existence. In the intervening period their activities are subdued, in fact they take their place on an equal footing with other water organisms, and it is possible at most seasons of the year to detect their presence by a diligent search in their haunts.

Crenothrix polyspora (Cohn)

This organism has been known since 1852, and since then attention to the details of its life-history has been stimulated

by the trouble and expense incurred when rapid multiplication has taken place. In Germany it is known as the "Brunnenpest." Among its visitations may be mentioned Breslau in 1870, Berlin in 1878, Lille in 1882, Rotterdam in 1888, and Cheltenham in 1896. At Cheltenham in 1896 the water supplied to the town became red and turbid, and from it there emanated an offensive odour. Within a fortnight the filters had become clogged. This state of affairs continued for about six weeks, after which the water once more began to assume its normal appearance. In spite of this alarming state of affairs Garrett reports that there was no evidence to show that the organism had any pathological influence. The appearance presented by an adult specimen of Crenothrix is very characteristic, and when once seen is subsequently easily recognised. This is shown in fig. 4, *A*. It consists of a hard cylindrical sheath, narrow at the base, but widening at the top. Inside the sheath are a number of short rods. When growing under normal circumstances these rods in young plants are arranged in a single row (fig. 4, *B*), but in older specimens the rods at the apex tend to break up into a number of round cells or "cocci" as they are called. This is shown in figs. 4, *A* and 4, *D*. If a young specimen be compared with an old one it will be seen that in the former case the sheath completely invests the enclosed rods and is equally wide at base and apex (figs. 4, *B* and 4, *C*), but that in the latter case the sheath, now somewhat hardened, has burst at the top and shows much wider at the apex than at the base (fig. 4, *A*). A young Crenothrix plant is shown in Plate IV. The bursting of the sheath at the apex enables the rods or cocci, as the case may be, to gain access to the outer world, and when liberated they elongate and in their turn form new threads.

With regard to the dimensions of this organism, the rods vary in length from 2 μ to 7 μ or 8 μ. The cocci measure from 2 μ to 4 μ in diameter. The sheath closely invests the contained rods, and whilst measuring 2 μ to 4 μ at the base may attain five or six times this width at the apex in a mature specimen. In normal times the rods or cocci are gently thrust out through the apex of the sheath, but when, as stated above, inordinate multiplication takes place, the individuals appear to be galvanised into leading a very strenuous life. The whole contents of the sheath break up into cocci, with the result that each

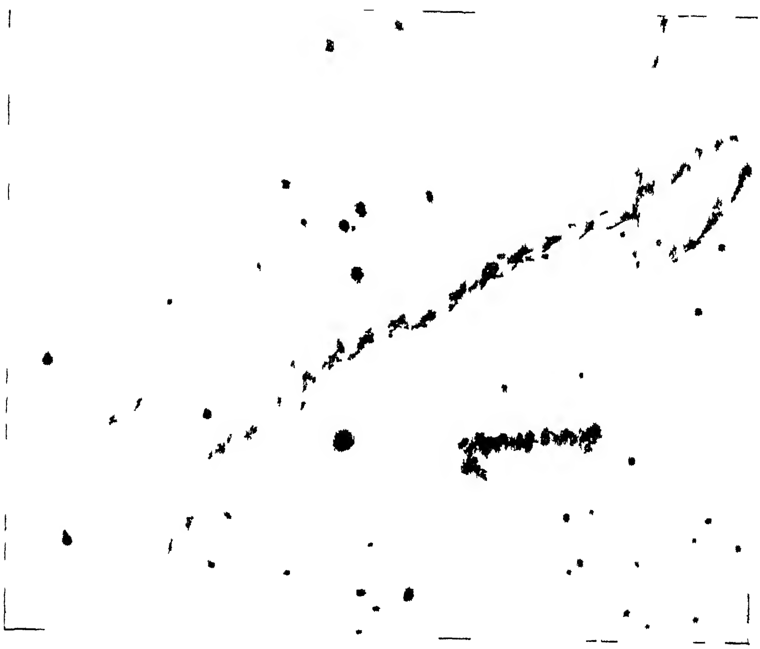

PLATE III.—*Spirophyllum ferrugineum*; × 450

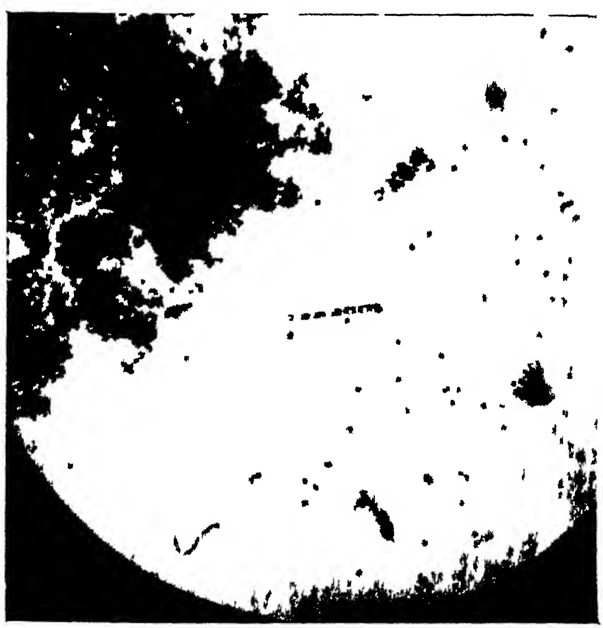

PLATE IV.—*Crenothrix polyspora* (young thread); × 500

sheath will contain several hundreds of these bodies. Then not only do the latter emerge from the apex, but they also break through the sides. Many even elongate into new threads from inside the sheath. In this condition the sheath is covered with a fine tangled mass of projecting filaments as shown in fig. 4, *D*. On these occasions we may presume also

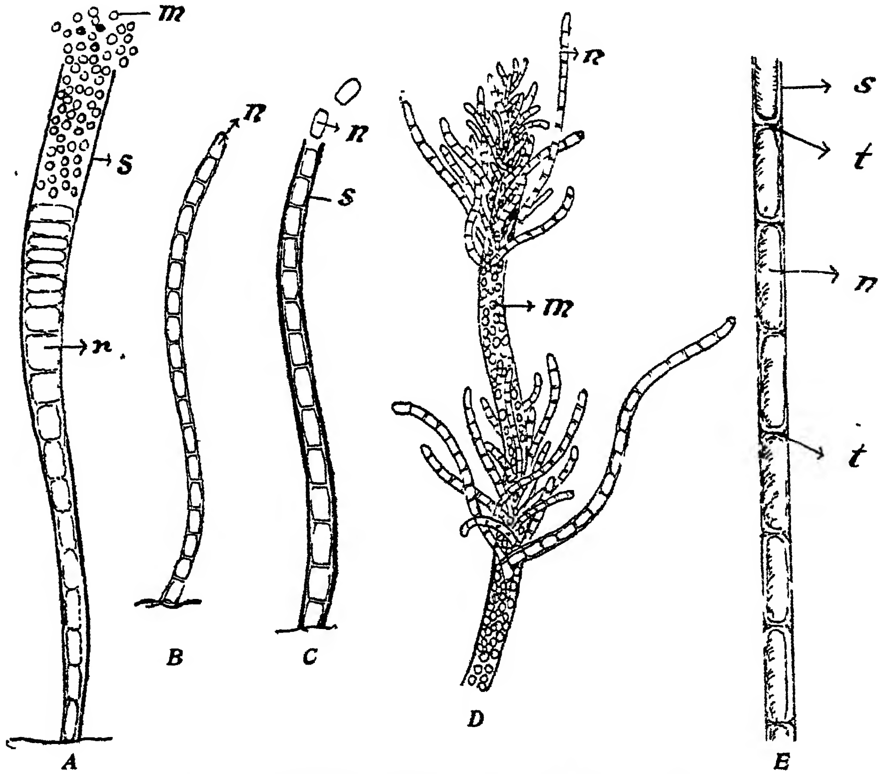

FIG. 4.—*Crenothrix polyspora*; *A–D*, × 1,000; *E*, × 2,000.

A, Mature thread open at the top. *B*, Young thread. *C*, Thread with rod-cells in process of liberation. *D*, Appearance of thread during period of very rapid multiplication. It is filled with cocci, which in germinating push out laterally through the sheath. *E*, Young thread stained to show transverse walls of the sheath. *s*, sheath; *n*, rod-cells; *m*, cocci; *t*, transverse walls of sheath.

that the rate of growth of each filament is considerably quickened, so that it is small wonder that there is such a phenomenal increase in their numbers, and that they are able to impress their colour on the water in which they thrive. Garrett calls attention to the fact that in young threads the sheath is in reality a tube divided up into compartments each containing a single cell. The writer has some unpublished notes

on this organism which bear out the accuracy of this statement (fig. 4, *E*). As will be seen below, the same cellular structure of the sheath holds good for *Cladothrix dichotoma*. Garrett further alludes to another characteristic of Crenothrix that is very remarkable. He states that when the cocci are massed together in a common mucilaginous envelope after extrusion from the sheath, the further development is effected by numbers of these cocci forming themselves into line, each line becoming presumably a new Crenothrix thread. It is far more probable that each thread is formed by the growth of a single coccus, but at the same time during its period of phenomenal activity we must expect unusual peculiarities of growth. The statement at any rate demands confirmation. Searchers after *Crenothrix polyspora* will have a little difficulty in distinguishing young Crenothrix from young Cladothrix threads. The identification is considerably facilitated by noting the fact that, as seen in Plate IV, the individual cells of Crenothrix are distinguishable without staining. The cells of Cladothrix do not become individually distinguishable without subjecting the threads to the action of appropriate staining reagents.

Cladothrix dichotoma (Cohn)

This organism consists of long thin threads attached at one end to various objects at the bottom and sides of stagnant streams. Like Crenothrix each thread is made up of a number of rods enclosed by a common sheath, but unlike Crenothrix the rods are always in single rows, are of uniform length in each thread, and usually longer and thinner. Also, except in rare cases, the threads are uniform in thickness. Each rod measures from 3 μ to 6 μ in length with a width of approximately 2 μ. A representation is given in Plate V and fig. 5. Occasionally the threads appear as if branched. This is due to the fact that some of the rods slip out laterally through the sheath and develop into new threads without severing their connection with the parent plant. This gives the appearance of dichotomous branching, and has given the name to the organism, but in the writer's experience this false dichotomous branching only rarely takes place. When stained with iodine the rods become visible. Each one possesses a distinct cell membrane. The sheath is formed by a contribution from

each of the rods. In the younger stages if the rods were removed the sheath would appear as a uniform cylindrical tube divided up into compartments by means of transverse walls (fig. 6 and Plate VI). Each compartment contains a single rod. A little farther on in the development the sheath

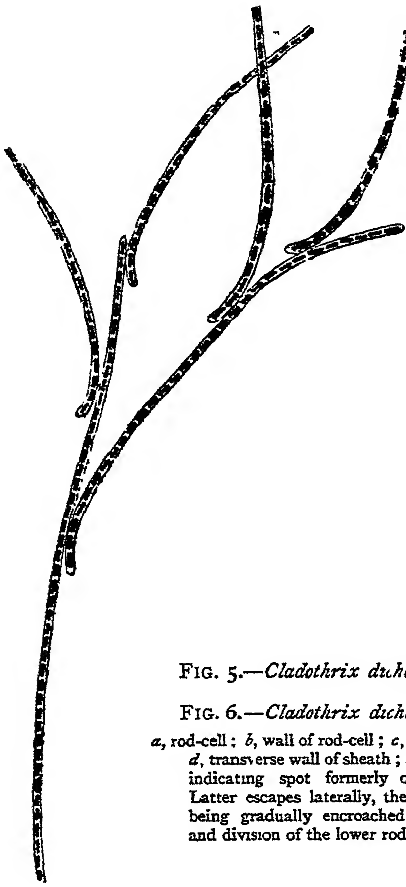

FIG. 5.

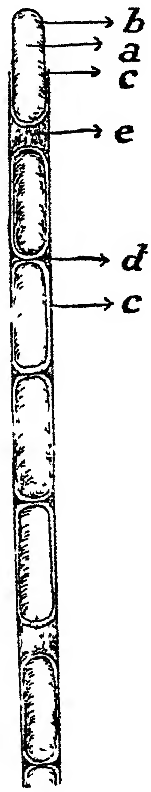

FIG. 6.

FIG. 5.—*Cladothrix dichotoma*; × 1,000.

FIG. 6.—*Cladothrix dichotoma*; × 3,000.

a, rod-cell; *b*, wall of rod-cell; *c*, lateral wall of sheath; *d*, transverse wall of sheath; *e*, thick transverse wall indicating spot formerly occupied by rod-cell. Latter escapes laterally, the space occupied by it being gradually encroached upon by the growth and division of the lower rod-cells.

hardens, and while this is taking place the basal rods are dividing and elongating, and by the pressure of their growth forcing themselves upwards. The result is that the transverse walls are broken and obliterated, and an opening is effected at the apex (fig. 7, *B*). The sheath therefore becomes a hollow, more or less firm tube, permanently open at the top. In the final stage we see nothing but a hollow sheath from which all the

rods have departed. This sinks to the bottom, and takes its place along with the other empty sheaths that have preceded it. If the life of Cladothrix has run its course in ferruginous waters the sheath is impregnated with ferric hydroxide, and in appearance is exactly similar to a dead Leptothrix thread. When the rods slip out of the open sheath they are either carried away as inert bodies or else are endowed with cilia and

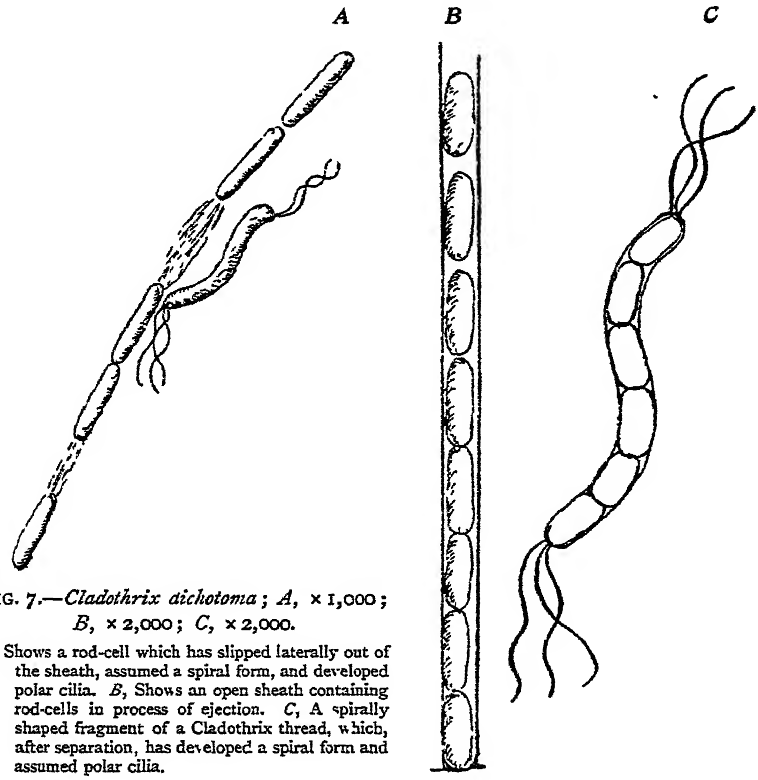

FIG. 7.—*Cladothrix dichotoma*; *A*, × 1,000; *B*, × 2,000; *C*, × 2,000.

A, Shows a rod-cell which has slipped laterally out of the sheath, assumed a spiral form, and developed polar cilia. *B*, Shows an open sheath containing rod-cells in process of ejection. *C*, A spirally shaped fragment of a Cladothrix thread, which, after separation, has developed a spiral form and assumed polar cilia.

effect their own locomotion. In the Cladothrix figured by Fischer the cilia are placed in a bunch immediately below one of the ends of the rod, but in the British species, so far as they have been examined, the cilia are placed exactly at the poles. The liberation of single rods is not confined to the apex; some may take a short cut through the sheath, and either slip away or else develop in attachment to the parent thread. Further non-motile or motile *fragments* may be liberated. In the latter case each rod composing the fragment is possessed of one or

PLATE V.—*Cladothrix dichotoma*, × 500

PLATE VI.—*Cladothrix dichotoma*; × 1,100

more polar cilia. In a very exceptional case the writer has observed these detached fragments assuming the form of spiral threads after leaving the parent organism. Each fragment was made up of a few rods enclosed in a common sheath. Motility was effected by means of polar cilia, the fragments also assuming a wavy structure (fig. 7, *C*). Without the use of stains they could not have been distinguished from members of the Spirillaceæ. A still more remarkable occurrence in the same culture was the lateral detachment of single rods, which on liberation assumed a *spiral* form with cilia at both ends, and swam off in the true spirillum manner (fig. 7, *A*). The detachment of spiral fragments from Cladothrix threads was noted by Zopf more than thirty years ago, but subsequent observers have either ignored his announcement or doubted its accuracy. These phenomena were observed in one, and only in one, culture out of an extensive series of observations carried out by the writer, and extending over eighteen months. The theoretical importance of this fact is great, for it shows that one and the same organism can produce both the bacillus and the spirillum forms of bacteria, which at present are regarded as being fundamentally distinct. A closer examination of the rods with the help of staining reagents reveals a delicate, well-defined membrane. In well-nourished cultures each cell has granular contents. Further, the presence of oil and glycogen can be demonstrated. As regards division, that process is essentially the same as in the genus Bacillus. A delicate transverse membrane is thrown across, this is followed by constriction, with eventually the separation of the rod into two cells. Each then grows until the mature form is reached. The distribution of this organism is very wide. As will be seen in the discussion of the physiology of the Iron-bacteria, it is only incidentally a member of the Iron-bacteria, for it is found in many non-ferruginous waters where there is a slight organic decomposition. Further, there can be little doubt that what we call *Cladothrix dichotoma* is either a group of very closely related species or else is one species capable of much pleomorphism in adjustment to the varied conditions of life.

Clonothrix fusca (Schorler)

The discovery of this organism by Schorler in the waterworks of Dresden added another member to the group of Iron-

bacteria. It resembles *Cladothrix dichotoma* in all respects except that the threads taper out at the ends. It is described as varying in thickness from 2 μ at the apex to 5–7 μ at the base. When covered with manganese hydroxide, which substance it attracts as well as iron compounds, the thickness of the

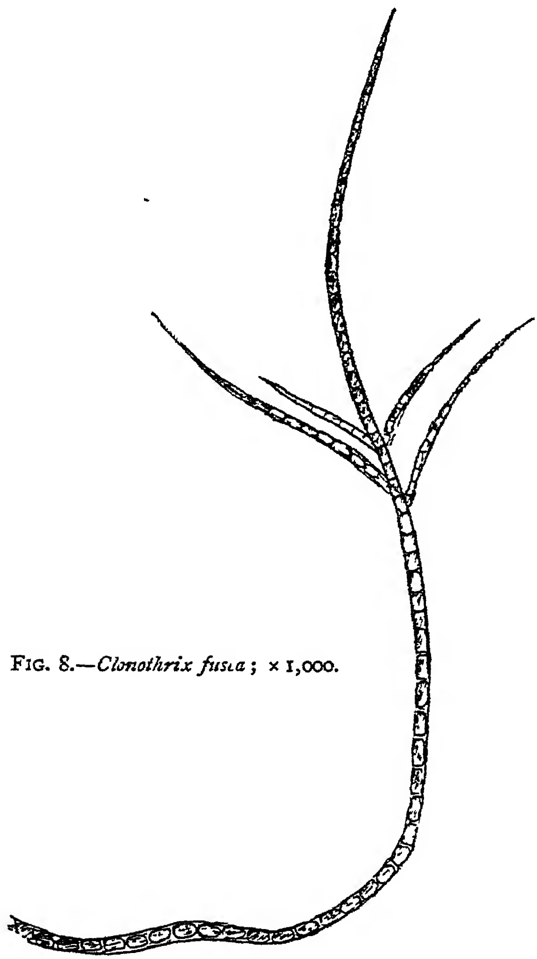

FIG. 8.—*Clonothrix fusca*; × 1,000.

organism may reach 24 μ. In view of the fact that it differs from *Cladothrix dichotoma* only in possessing a tapering apex, and considering that the latter name is given rather to an assemblage of closely allied species than to a single specific organism, the suggestion may be made that perhaps it would

have been advisable to have inserted Schorler's organism within the folds of the genus Cladothrix. It is shown diagrammatically in fig. 8.

Siderocapsa Treubii (Molisch)

A very peculiar organism was described by Molisch in 1909. This lives as an epiphyte on water plants. Its presence is indicated by the appearance of a rust-coloured crust on the surfaces of these plants. In the crust numerous roundish clear spaces appear, each about $1 \cdot 8\,\mu$ to $3 \cdot 6\,\mu$ in diameter. In these spaces numerous round little cells or cocci may be discerned, and it is due to their activity that the crust is formed. Each clear space encloses a colony of cocci, and the crust of iron always begins round these colonies. Another species of the same kind found by this investigator has been named *Siderocapsa major*. This new genus differs very widely from any of the genera hitherto included under the Iron-bacteria; it is to be hoped that we shall be furnished with some details of its life-history, and more particularly with its physiological activities in so far as its relation to iron compounds is concerned.

Artificial Cultures of the Iron-Bacteria

Of the seven organisms mentioned above, artificial cultures have not yet been successfully accomplished in so far as *Gallionella ferruginea*, *Clonothrix fusca*, and the genus Siderocapsa are concerned. Impure artificial cultures only have been obtained of *Spirophyllum ferrugineum* and *Crenothrix polyspora*. Pure artificial cultures have been obtained only of *Leptothrix ochracea* and *Cladothrix dichotoma*, the two best known and most widely distributed of the Iron-bacteria. The artificial cultures have brought to light several interesting features. They have in particular demonstrated that the term "Iron-bacteria" in the sense in which the term was originally employed is a misnomer.

The absorption of iron-compounds is not necessary to their growth—except of course that infinitesimal amount that all organisms need—and the best artificial cultures have been obtained from nutrient media which did not contain this substance in any appreciable quantity. This detracts somewhat from the glamour which has surrounded these peculiar

organisms, but we are well repaid by the fact that we have approached appreciably nearer to an understanding of their peculiar habits. It is no longer true to say that they derive their energy by the oxidation of ferrous to ferric compounds. What their physiological relations with iron are will be discussed in the next section; it will suffice here to state that the absorption of iron compounds is not necessary for their growth. Another curious fact in connection with these organisms is the great uncertainty which attends all attempts to cultivate them artificially. The virility of the growth of Crenothrix at certain periods has already been mentioned. Garrett mentions most unlikely media on and in which he during such a period secured growth. But at ordinary times the artificial growth of Crenothrix has not been found possible. The same applies to Cladothrix. Whilst Hoflich secured growth with the greatest of ease, others have succeeded only after much trouble, and when successful the growth has never been a prolific one. There is no difficulty in the matter of the preparation of the nutrient medium, for it consists merely in the addition to ordinary drinking water of small quantities of such substances as flesh-extract or peptone or ferrous-ammonium citrate. The sole difference between the ingredients of the successful and the partially successful or unsuccessful media has lain only in a difference of the water employed, this naturally varying with the place of abode of the investigator. Whether the determining factor is the presence in certain waters of minute quantities of substances which play the same rôle as the hormones in the animal kingdom, it is impossible as yet to say. Bottomley claims that they exist, but his evidence as yet is unconvincing. Molisch obtained his best artificial cultures of *Leptothrix ochracea* by the addition to water of 0·25 per cent of mangan-peptone, and obtained pure cultures by the use of this medium solidified with 10-per-cent. gelatine. He describes its activities in a number of liquid and solid media. It is aerobic, liquifies gelatine, grows in light and in darkness, and shows a tendency to form short rods which exhibit a decided motility.

Höflich gives a very complete account of artificial cultures of *Cladothrix dichotoma*. The addition of half a gramme of flesh extract to a litre of water was sufficient to ensure an abundant growth. Cladothrix liquefies gelatine very slowly;

it is an obligate aerobic organism, and there is a tendency to the formation of short rods. In the case of both organisms the addition of iron-compounds is not essential to growth. If we are to class them under a physiological group both can fitly be included under the peptone-bacteria, as this substance seems to be able to supply them with both the carbonaceous and nitrogenous essentials for the elaboration of protoplasm.

Artificial cultures of *Crenothrix polyspora* have been obtained by Garrett and by Rossler. The accuracy of the latter's results have been challenged owing to the fact that attempts along the same lines by other investigators have invariably produced negative results. Rossler states that he has been successful in cultivating Crenothrix on bricks to which a little ferrous sulphate had been added. It is very probable that Rossler did succeed in making impure cultures of Crenothrix and that the necessary food materials were supplied by organic impurities on the surface and in the substance of the bricks. He states that he obtained pure cultures, but it is difficult to see from his description how this was possible. Still more remarkable artificial cultures were obtained by Garrett, who states that during the period when the growth of Crenothrix became so overwhelming at Cheltenham he was able to obtain artificial cultures. The cocci liberated from Crenothrix threads offered no resistance to growth on a gelatine plate, on coagulated cleared serum at 30° C., on potato, etc. There is also here a want of conviction as to the purity of the cultures. No details are given as to the methods adopted to secure pure cultures. Whilst there cannot be any reasonable doubt that artificial growths were obtained by these investigators, one hesitates to accept without further proof that those growths which appeared on serum, potato, etc., were due to Crenothrix, and not to extraneous micro-organisms that had crept in as impurities. In the case of Crenothrix, as in the other two, the results show that the source of energy is not the oxidation of ferrous to ferric compounds, but is rather obtained from the absorption of organic substances.

The writer has obtained slight impure cultivations of *Spirophyllum ferrugineum,* but the results are too scanty and inconclusive to form a basis for discussion.

THE PHYSIOLOGY OF THE IRON-BACTERIA

The outstanding feature of these organisms is their power of attraction for iron-compounds. In the waters in which they abound, unlike their co-inhabitants, large quantities of ferric hydroxide are found deposited on the sheaths or membranes of these bacteria. Complete agreement as to the explanation of this phenomenon does not at present obtain. Not only the Iron-bacteria, but also a few algæ and protozoa possess the same peculiar characteristic. It has been shown above that except in infinitesimal quantities the presence of iron-compounds is not necessary for the successful development of Iron-bacteria. One may therefore dismiss at once Winogradsky's theory that the oxidation of ferrous to ferric compounds was the source from which this energy was obtained. He argued that without the oxidation of ferrous bicarbonate to ferric hydroxide no growth was possible. This oxidation takes place according to the equation :

$$\left.\begin{matrix} 2FeCO_3 \\ H_2CO_3 \end{matrix}\right\} + 3H_2O + O = \begin{matrix} Fe_2(OH)_6 + 2CO_2 \\ CO_2 + H_2O \end{matrix}$$

Obviously the whole theory falls to the ground when artificial cultivations can be made from which iron has been rigorously excluded ; and further, in Nature there are many iron-waters that harbour Iron-bacteria which do not contain any iron in the form of the bicarbonate. Winogradsky's theory still holds sway in many of the text-books, although he produced no supporting facts.

Another theory is supplied by Molisch. He argues that the accumulation is due to a chemio-tactic irritability for iron-compounds. These substances are attracted by the bacteria, detained by the mucilaginous sheath, and there oxidised into ferric hydroxide. This does away with the necessity for iron-compounds, and is supported by a strong body of facts, as will be seen presently. A third theory is advanced by Campbell Brown, who contends that the activities of the Iron-bacteria are in all cases directed towards the assimilation of organic compounds. When iron is combined in solution with these compounds the removal of the latter brings the iron out of solution, with the result that the precipitated iron collects on and in the membrane or sheath and even invades the cell.

In considering the relative merits of the second and third theories the following facts, which have been proved, must be borne in mind.

1. Iron compounds are not in themselves necessary for growth.

2. Manganese can equally well replace iron.

3. Iron-bacteria obtain their nitrogen from organic compounds as do the vast majority of bacteria.

4. The more organic matter there is in solution in ferruginous waters, the more iron these waters are able to hold in solution.

5. Bacteria in general do possess chemio-tactic attractions and repulsions for a variety of substances.

6. Iron is found not only on or in the membrane or sheath, but actually inside the cell.

7. The growth of Iron-bacteria in ferruginous waters undoubtedly expedites the accumulation of ferric hydroxide in these waters.

8. Other minute organisms, *e.g.* some of the Euglena family, some of the Flagellates, *e.g. Anthophysa vegetans* and some of the Œdogonia group, as well as the Iron-bacteria, possess the same physiological characteristic. When we now apply the theories of Molisch and of Campbell Brown to these proven facts it will be seen that both theories are able to bear the test.

Some of the facts, viz. Nos. 2, 5, and 8, strengthen Molisch's theory considerably; on the other hand, Nos. 3, 4, and 6 give greater force to Campbell Brown's theory. They are, however, not necessarily mutually exclusive, and we shall not be far wrong in granting that each contains a certain measure of truth. If these organisms have a chemio-tactic affinity for iron-compounds, and at the same time remove iron out of solution in their absorption and assimilation of organic compounds, the effect of their growth in expediting the oxidation of ferrous compounds can readily be imagined. The question has lately arisen as to whether the organisms designated as Iron-bacteria are the only members of the group of Bacteria capable of producing these oxidising changes. Mumford has described an iron-bacterium which oxidises a ferrous compound to ferric hydroxide. This work, however, does not adduce incontestable proof that the effects claimed to be produced by the bacterium were due to that organism and not to other agencies such as chemical oxidation. The absence of control

cultures is much to be deplored. It further assumes that the oxidation of ferrous compounds is the work of an enzyme secreted by the organism. As shown above, the facts now at our disposal render this standpoint indefensible. Lately also Harder, of the U.S. Geological Survey, has promised us the proof that a large number of the lower bacteria are capable of oxidising ferrous compounds. There is every probability that quite a large number of micro-organisms are capable of doing this, for, if we assume the correctness of Campbell Brown's theory, all bacteria capable of abstracting soluble organic matter incidentally combined with iron will by the absorption of this organic matter remove that to which is due the retention of so much iron in the soluble form. It is probable that interesting results will be obtained along these lines.

Are the Iron-Bacteria to be regarded as Rock Builders ?

This question has been dealt with from time to time, but it is only recently that the issue has been placed on a satisfactory basis. Within recent years Molisch has examined sixty-one samples of ferruginous stones from various parts of the world, and in fifty-seven he found no traces of Iron-bacteria. In the remaining four Iron-bacteria were undoubtedly present, but it is difficult to remove the impression that these bacteria were not obtained from a modern sediment on the surface. It is to be hoped that we shall later receive a more detailed treatment of the four stones in question. If the bacteria were found in the body of the stone we should be in possession of a fact of great interest. The writer has recently examined forty-eight of the ferruginous rocks of Great Britain and found four to contain the remains of iron micro-organisms. These four stones were all obtained from the Frodingham ironstone of Lincolnshire, and contained the remains, not of bacteria, but of an iron-mould to which the name *Phycomycites Frodinghamii* was given. This organism was present in the organic remains that are plentifully found in this rock; and while during its lifetime it probably had the physiological characteristics of the Iron-bacteria of the present day and thus enriched the ferruginous content of the stone containing it, it cannot in any sense be regarded as a rock builder.

THE SOLUTION OF EQUATIONS BY OPERATIVE DIVISION.—Part II

By SIR RONALD ROSS, K.C.B., F.R.S., D.Sc.

CONTENTS OF PART II

VI. (1) We have now studied operative division when the divisor is of the form $o + b$, or $o + bo^2 + co^3 + \ldots$, that is when the divisor commences with the first power of o; and in these cases the powers of o in the quotient are integral if they are integral in the first dividend. But how shall we carry out the division if the divisor commences with other powers of o, as for example when we wish to divide o by $o^3 - 2o$ or by $o^2 + bo^3 + co^4$? We proceed in exactly the same way except that instead of expanding the divisor for integral powers in order to obtain each successive subtrahend, we may now have to expand it by negative or fractional powers. If the dividend (either the first or any successive dividend) commences with o^r and the divisor commences with o^n, then the corresponding power of o in the quotient must be such that when it operates on o^n it converts it into o^r—that is, it must be $o^{\frac{r}{n}}$, whatever numbers positive, negative, integral, or fractional n and r may be. Then, in order to obtain the corresponding dividend, $o^{\frac{r}{n}}$ must operate on the whole divisor; that is, the dividend will be the expansion of the $\left(\frac{r}{n}\right)$th power of the divisor by the binomial or the

multinomial theorem. The following examples will make this clear:

A. $$x^3 - 2x - 5 = 0$$

Newton's equation. In Section III (5) Example A we obtained the single real root of this by simple ascending division, but only after shifting the origin by dividing by $\mathbf{o} - 2$. Now write the equation in the form

$$x^3 - 2x = 5;$$

that is, $$[\mathrm{o}^3 - 2\mathrm{o}]\, x = 5;$$

and $$x = \frac{\mathrm{o}}{\mathrm{o}^3 - 2\mathrm{o}} : 5.$$

Carrying out the division to three terms,

$$\begin{array}{rl}
\mathrm{o}^3 - 2\mathrm{o}]\mathrm{o} & \qquad\qquad [\mathrm{o}^{\frac{1}{3}} + \frac{2}{3}\mathrm{o}^{-\frac{1}{3}} - \frac{8}{81}\mathrm{o}^{-\frac{5}{3}} + \dots \\
 & \underline{\mathrm{o} - \frac{2}{3}\mathrm{o}^{-1} - \frac{4}{9}\mathrm{o}^{-3} - \frac{40}{81}\mathrm{o}^{-5} + \dots} \\
 & \quad \frac{2}{3}\mathrm{o}^{-1} + \frac{4}{9}\mathrm{o}^{-3} + \frac{40}{81}\mathrm{o}^{-5} - \dots \\
 & \quad \underline{\frac{2}{3}\mathrm{o}^{-1} + \frac{2}{3}\frac{2}{3}\mathrm{o}^{-3} + \frac{2}{3}\frac{8}{9}\mathrm{o}^{-5} + \dots} \\
 & \qquad\qquad\qquad - \frac{8}{81}\mathrm{o}^{-5} - \dots
\end{array}$$

Applying the operator $\mathbf{o}$ in the quotient to the subject 5, we have

$$x = 5^{\frac{1}{3}} + \frac{2}{3}5^{-\frac{1}{3}} - \frac{8}{81}5^{-\frac{5}{3}} + \dots$$
$$= 1{\cdot}70998 + 0{\cdot}39000 - 0{\cdot}00677 = 2{\cdot}09321 \dots$$

The root is 2·09455 . . .; so that we have obtained three figures of it correctly from only three terms of the invert and *without any transformation.*

B. $$x^3 - 5x + 1 = 0$$

This is the "centred" form of $y^3 - 3y^2 - 2y + 5 = 0$, dealt with in Section III (5) Example B. It has three roots, of which we will now obtain two at once by a single descending

division. For this purpose divide algebraically by x and take the " setting "

$$x^2 + x^{-1} = 5.$$

Then $\quad 0^2 + 0^{-1}]0 \qquad\qquad [0^{\frac{1}{2}} - \frac{1}{2}0^{-1} - \frac{3}{8}0^{-\frac{5}{2}} - \ldots$

$$\begin{array}{r} 0 + \frac{1}{2}0^{-2} - \frac{1}{8}0^{-5} + \ldots \\ \hline -\frac{1}{2}0^{-2} + \frac{1}{8}0^{-5} - \ldots \\ -\frac{1}{2}0^{-2} + \frac{1}{2}0^{-5} - \ldots \\ \hline -\frac{3}{8}0^{-5} + \ldots \end{array}$$

Thus
$$\begin{aligned} x &= \pm 5^{\frac{1}{2}} - \frac{1}{2}5^{-1} \mp \frac{3}{8}5^{-\frac{5}{2}} - \ldots \\ &= \pm 2{\cdot}2361 - 0{\cdot}1000 \mp 0{\cdot}0067 \\ &= 2{\cdot}1294 \text{ or } - 2{\cdot}3293 \end{aligned}$$

The actual roots are $x = 2{\cdot}1284 \ldots$ and $- 2{\cdot}3300 \ldots$; so that, in this case, with only three terms of one invert we have obtained three correct figures for the first root and nearly three correct figures for the negative one.

The middle root, $x = 0{\cdot}201637, \ldots$ can be obtained by ascending division from the setting $5x - x^3 = 1$, as already given in Section III (5). Or, putting $x = \frac{1}{z}$ and then dividing by z^2, we get the setting $z^2 + z^{-1} = 5$, which yields the same root by descending division. The reader may obtain it and also explain the facts.

C. $$x^3 - 3x^2 - 2x + 5 = 0$$

This has the same roots as the previous example, but increased by unity. The setting $x - 2x^{-1} + 5x^{-2} = 3$ gives the greatest root by descending division, but slowly. Putting $x = \frac{5}{z}$, we have $z^3 - 2z^2 - 15z + 25 = 0$; and from the setting $z^2 - 2z + 25z^{-1} = 15$, we obtain from one invert, but slowly, $z = 4{\cdot}237 \ldots$ and $= - 3{\cdot}901 \ldots$, which, when the substitution is made good, approach the negative and the middle roots of x. The setting $x^2 - 3x + 5x^{-1} = 2$ gives two series (for the two roots of $\surd 2$) which converge very slowly if at all towards the greatest and least roots; and the centred form of the previous example is evidently preferable.

D. $$x^3 - 7x + 7 = 0$$

The setting $x^2 + 7x^{-1} = 7$ appears to converge towards the greatest and the least roots when the positive or negative value of $\sqrt{7}$ is taken, but in the former case the convergence is very slow. The middle root has been laboriously studied by ascending division from the setting $x - \frac{1}{7}x^3 = 1$ (Section III (5) Example D); but we may also approach it by descending division by putting $x = \frac{7}{z}$, which gives $z^3 - 7z^2 + 49 = 0$.

As suggested in these examples, any equation can be put into many different settings by dividing it algebraically by different powers of x. This process renders each coefficient in turn independent of x—so that each coefficient in turn may be made the subject of a different inversion, either by descending or by ascending division. We may also obtain a second series of settings by putting $x = \frac{1}{z}$ and simplifying; and it would appear that different settings yield different roots, while others give quite divergent or inconclusive series, and in others again the subject involves the square root of a negative quantity. Generally speaking, the subject should be as large as possible for descending division and as small as possible for ascending division; and where it involves a real square root the inversion often yields simultaneously two real roots of the original equation. The reader will probably find considerable difficulty in explaining these things, but may read further before attempting to do so.

(2) The expansions of fractional or negative powers of the divisor needed for descending division are considerably more laborious than the expansions of integral powers required for simple ascending division; but, as a matter of fact, the labour is avoided by calculating the invert from the algebraic series which can easily be obtained by observing the law of formation of the coefficients of the successive powers of o in the quotient—as already shown for simple ascending division in Section III (3). This law may be readily worked out by the reader in the case of the general trinomial equation $x^n + px^{n-r} = y$, from which it will be seen that the coefficients of the invert of $o^n + po^{n-r}$ reduce to the ordinary binomial coefficients multiplied by a factor. Both this and the general cases have been discussed

at length in "Verb Functions," and it is unnecessary to do more here than to give the results.

Let us denote by $(m)_t$ the coefficient of the t th power of x in the expansion of $(1+x)^m$, so that

$$(m)_t = \frac{m(m-1)\ldots(m-t+1)}{t!}.$$

Then if

$$x^n + px^{n-r} = y,$$
$$x = [0^n + p0^{n-r}]^{-1}y,$$

and

$$[0^n + p0^{n-r}]^{-1} = 0^{\frac{1}{n}} + \frac{1}{r-1}\left(\frac{r-1}{n}\right)_1 0^{-\frac{r-1}{n}} - \text{etc.}$$

Put in terms of x and y (which will be more familiar to the reader), this gives

$$x = y^{\frac{1}{n}} - \frac{1}{r-1}\left(\frac{r-1}{n}\right)_1 py^{-\frac{r-1}{n}} - \frac{1}{2r-1}\left(\frac{2r-1}{n}\right)_2 p^2y^{-\frac{2r-1}{n}} - \frac{1}{3r-1}\left(\frac{3r-1}{n}\right)_3 p^3y^{-\frac{3r-1}{n}} - \text{etc.}$$

Here n, r, p, y may be positive, negative, integral, or fractional. If r is positive, the series is the applied quotient of a descending division; if negative, of an ascending division. But of course the series is not always convergent and not always real—matters which will be examined later (Section V *et seq.*).

In the general case, if

$$x^n + bx^{n-1} + cx^{n-2} + dx^{n-3} + \ldots = y,$$

then $[0^n + b0^{n-1} + \ldots]^{-1} = 0^{\frac{1}{n}} - \frac{1}{n}b0^0 - \text{etc.},$

and
$$\begin{aligned} x = y^{\frac{1}{n}} - \frac{1}{n}by^0 &- \left\{\left(\frac{1}{n}\right)_1 c + \left(\frac{1}{n}\right)_2 b^2\right\}y^{-\frac{1}{n}} - \frac{1}{2}\left\{\left(\frac{2}{n}\right)_1 d + \left(\frac{2}{n}\right)_2 2bc + \right.\\ &\left. + \left(\frac{2}{n}\right)_3 b^3\right\}y^{-\frac{2}{n}} - \frac{1}{3}\left\{\left(\frac{3}{n}\right)_1 e + \left(\frac{3}{n}\right)_2 (2bd + c^2) + \left(\frac{3}{n}\right)_3 3b^2c + \right.\\ &\left. + \left(\frac{3}{n}\right)_4 b^4\right\}y^{-\frac{3}{n}} - \frac{1}{4}\left\{\left(\frac{4}{n}\right)_1 f + \left(\frac{4}{n}\right)_2 2(be + cd) + \left(\frac{4}{n}\right)_3 3(b^2d + bc^2) + \right.\\ &\left. + \left(\frac{4}{n}\right)_4 4b^3c + \left(\frac{4}{n}\right)_5 b^5\right\}y^{-\frac{4}{n}} - \frac{1}{5}\left\{\left(\frac{5}{n}\right)_1 g + \left(\frac{5}{n}\right)_2 (2bf + 2ce + d^2) + \right.\\ &+ \left(\frac{5}{n}\right)_3 (6bcd + 3b^2e + c^3) + \left(\frac{5}{n}\right)_4 (6b^2c^2 + 4b^3d) + \left(\frac{5}{n}\right)_5 5b^4c + \\ &\left. + \left(\frac{5}{n}\right)_6 b^6\right\}y^{-\frac{5}{n}} - \text{etc.} \end{aligned}$$

The values of the coefficients are not restricted. The number of them is unlimited; but if it exceeds n the original equation contains negative powers of x—as happens when we employ the various settings suggested in the last subsection.

It is easy to construct further terms of the series. If instead of $b, c, d, \ldots$, we use the *weighted* coefficients $p_1, p_2, p_3, \ldots$, we shall see that for the groups contained within the *large* brackets the sum of the products of weight and power is the same for all. Thus if we write $p_1^3p_3$ for the group b^3d occurring in the last major term given above, we obtain for it the number $1 \times 3 + 3 \times 1 = 6$; and we shall find that the weight of all the other groups affected by $y^{-\frac{5}{n}}$ is 6 also. Moreover, the expressions contained within the *small* brackets are each evidently made up of all possible groups of the first six coefficients p_1 to p_6 which have the same weight and are of the same order. Lastly, the sum of the numerical coefficients of the groups within the small brackets is an appropriate binomial coefficient—it is $(5)_r$ in the case of the last major term written out. In short, the major coefficients of the invert are nothing but the multinomial coefficients multiplied by a numerical factor, and are therefore well known. The law is as follows. Let

$$(1 + bx + cx^2 + dx^3 \ldots)^m = 1 + \{m\}_1x + \{m\}_2x + \{m\}_3x + \ldots \{m\}_rx + \ldots;$$

and
$$\phi_n = 0^n + b0^{n+1} + c0^{n+2} + \ldots,$$

and
$$\psi_n = 0^n + b0^{n-1} + c0^{n-2} + \ldots;$$

then
$$[\phi_n]^{-1} = 0^{\frac{1}{n}} + \frac{1}{2}\left\{-\frac{2}{n}\right\}_1 0^{\frac{2}{n}} + \frac{1}{3}\left\{-\frac{3}{n}\right\}_2 0^{\frac{3}{n}} + \frac{1}{4}\left\{-\frac{4}{n}\right\}_3 0^{\frac{4}{n}} + \ldots$$

$$[\psi_n]^{-1} = 0^{\frac{1}{n}} - \frac{1}{0}\left\{\frac{0}{n}\right\}_1 0^{-\frac{0}{n}} - \frac{1}{1}\left\{\frac{1}{n}\right\}_2 0^{-\frac{1}{n}} - \frac{1}{2}\left\{\frac{2}{n}\right\}_3 0^{-\frac{2}{n}} - \ldots$$

It is shown in "Verb Functions," page 51, that these results depend upon the property of the multinomial coefficients which is contained in the equation

$$\{m\}_{r-1} + \frac{1}{2}\{-2m\}_1\{2m\}_{r-2} + \frac{1}{3}\{-3m\}_2\{3m\}_{r-3} + \ldots + \frac{1}{r}\{-rm\}_{r-1} = 0,$$

and which holds equally well, of course, for the binomial coefficients.

Some useful inverts and other details will be given in the tables at the end of this paper.

(3) This is not the place to make a minute algebraic study of the series; but some verifications and necessary corollaries are now furnished in the form of examples.

E. Invert $(x+1)^n = 1$

by descending division. The invert vanishes.

F. $$[\psi_n]^{-1} = [\sqrt[n]{\psi_n}]^{-1}\sqrt[n]{o}$$

This identity is obvious since $\left[[o^{\frac{1}{n}}][\psi_n]\right]^{-1}[o^{\frac{1}{n}}] = [\psi_n]^{-1}[o^n][o^{\frac{1}{n}}]$. That is, if $\psi_n x = y$, then $\sqrt[n]{(\psi_n x)} = \sqrt[n]{y}$, as often written. In fact it can be seen at once from the series that

$$\psi_n^{-1} = [o - \frac{1}{n}bo^0 - \left\{\binom{\frac{1}{n}}{1}c + \binom{\frac{1}{n}}{2}b^2\right\}o^{-1} - \ldots][o^{\frac{1}{n}}].$$

Hence we infer that the superior operation on the right side of this equation is nothing but the invert of $\sqrt[n]{(\psi_n)}$; and if we expand this last operation by the multinomial theorem and then invert it by descending division, the supposition will prove to be true. This enables us to reduce descending division by any such form as ψ_n to one of simple descending division by the form ψ_1. For example

$$[o^2+bo]^{-1} = [\sqrt{(o^2+bo)}]^{-1}\sqrt{o} = [o+\frac{1}{2}bo^0 - \frac{1}{8}b^2o^{-1}+\ldots]^{-1}\sqrt{o}.$$

In this case, however, the form ψ_1 will contain an infinite number of terms.

G. Find the values of $(\phi_n^{-1})^r$ and $(\psi_n^{-1})^r$;

that is, the rth algebraic power of the inverts. These can be obtained at once by the multinomial theorem; or by dividing o^r instead of o by ϕ_n and ψ_n, since

$$\frac{o^r}{\phi_n} = [o^r]\phi_n^{-1} = (\phi_n^{-1})^r.$$

Or they can be obtained by dividing o by $[\phi_n]o^{\frac{1}{r}}$. The results are ("Verb Functions," p. 59),

$$(\phi_n^{-1})^r = o^{\frac{r}{n}} + \frac{r}{r+1}\left\{-\frac{r+1}{n}\right\}_1 o^{\frac{r+1}{n}} + \frac{r}{r+2}\left\{-\frac{r+2}{n}\right\}_2 o^{\frac{r+2}{n}} + \ldots$$

$$(\psi_n)^r = o^{\frac{r}{n}} + \frac{r}{r-1}\left\{-\frac{r-1}{n}\right\}_1 o^{\frac{r-1}{n}} + \frac{r}{r-2}\left\{-\frac{r-2}{n}\right\}_2 o^{\frac{r+2}{n}} + \ldots$$

H. Prove that $(\phi_{-r}^{-1})^{-r} = (\psi_n^{-1})^r$

Put $-n$ and $-r$ for n and r in either of the series just given and we shall obtain the other.

Hence $$\phi_{-n}^{-1} \cdot \psi_r^{-1} = 1.$$

Obviously from the definitions of ϕ_n and ψ_n in the previous subsection
$$\psi_n = [\phi_{-n}]o^{-1} \text{ and } \phi = [\psi_{-r}]o^{-1}.$$

Hence $$\psi_n^{-1} = [[\phi_{-n}]o^{-1}]^{-1} = [o^{-1}]\phi_{-n}^{-1} = \frac{1}{\phi_{-n}^{-1}};$$

with a similar identity for ϕ_r^{-1}; and these identities are now seen to verify the series for ϕ^{-1} and ψ^{-1}. (Note that $[o^{-1}]^{-1} = o^{-1}$.)

This proposition enables us to reduce ascending to descending division and *vice versa*. The same results are obtained by putting $x = \frac{1}{z}$ in numerical equations; and the reader should study the details. It will be found that the two forms of division if taken according to the equation give the same series as a result.

I. Prove that $\psi_n\psi_n^{-1} = o$, $\psi_n^{-1}\psi_n = o$, and $[\psi_n^{-1}]^{-1} = o$; with similar identities for ϕ_n. For each term of the superior operation substitute the proper power of the inferior operation as expanded in Example G, and collect the coefficients of the same powers of o in the result. The issue will be found to be $o = o$ — thus proving that we are dealing only with absolute identities. But *all* the terms must be considered.

J. Prove that the invert by descending division of an equation which possesses only positive powers of the variable contains all its real and imaginary roots. Take for example $x^n + px^{n-r} = y$, which has n roots. Then by (2) above, its invert is a function of $\sqrt{y}$, which also has n roots. Let these be $ma, ma^2, ma^3 \ldots$; so that, by a well-known theorem,
$$a + a^2 + a^3 \ldots + a^n = o.$$

Insert these values of $\sqrt{y}$ successively in the series for x, that is, in $[\psi_n]^{-1}x$. We thus obtain n different series. It will be found that the sum of these series and the sum of their products two, three, four . . . at a time all vanish, until we come to the sum of their products r at a time, which $= \pm p$. The sums of their products more than r times together also vanish, until we come to the product of all the series together, which $= \pm y$. This satisfies the familiar law that the sums of the products of

the roots of an equation once, twice, thrice . . . together are successively equal to the successive coefficients of the equation with their signs alternately changed and not changed. The demonstration requires the use of the symmetrical functions of the roots and is laborious but elegant. It is given for the general equation in "Verb Functions," pages 46 and 60–63. Hence the n series obtained by substituting the n values of $\sqrt[n]{y}$ in the operative series for ψ_n^{-1} are actually all the n roots of the equation. *The series for ψ_n^{-1} (when ψ_n possesses no negative powers of* o*) is therefore the complete invert of ψ_n.* It should be added that I have not yet found how to recover more than one or two numerical roots from this series: it generally furnishes one; but the remaining real roots must be got from the secondary inverts obtained from the equation divided by various powers of x, as shown in Examples B, C, D of (1) above; or from "vicarious functions" (Part III).

K. Prove that $[[\psi_n][o + p]]^{-1} = \psi_n^{-1} - p$.

On dividing ψ_n operatively by $o - p$, we obtain $\psi_n\ (o + p)$, which is ψ_n when the origin has been shifted. Inverting this, we find that p vanishes from the coefficients of all the powers of o in the invert except from that of o^0. These coefficients are therefore invariants; but from this point the study of ψ_n^{-1} takes a direction which does not concern us at the moment.

L. Splitting the Subject.

This is often useful for arithmetical work. For example, the complete invert of $x^3 - 2x = 5$ has for subject the cube root of 5. This being troublesome, we may take the equation in the form $x^3 - 2x + 3 = 8$ — the invert of which is a function of $\sqrt[3]{8}$, that is, 2. We now have to invert, not $o^3 - 2o$, but $o^3 - 2o + 3$, which, however, is nearly as easy by use of the tabulated series. If the original subject was $\sqrt[n]{S}$, the convergence is quickest if the new subject is $\sqrt[n]{S + \frac{X}{n} f'X - X^n}$, where X is the root and n is the degree of the equation. In this example the new subject ought to be about $\sqrt[3]{\frac{11}{3}}$, if we take $X = 2$; but the subject $\sqrt[3]{\frac{27}{8}}$ will be nearly as quick and easier to work with —quicker than $\sqrt[3]{8}$ and nearly as easy.

M. Find the value of ϕ^{-1} and ψ^{-1} from the general theorem of operative multiplication. This theorem is

$$\begin{aligned}
\Phi_1\phi_1 = {} & [Bo + Co^2 + Do^3 \ldots][bo + co + do \ldots] = \\
& = Bbo + (Cb^2 + Bc)o^2 + (Db^3 + 2Cbc + Bd)o^3 + \\
& + \{Eb^4 + 3Db^2c + C(2bd + c^2) + Be\}\, o^4 + \{Fb^5 + 4Eb^3c + \\
& + 3D(b^2d + bc^2) + 2C(bc + cd) + Bf\}o^5 + \{Gb^6 + 5Fb^4c + \\
& + 2E(2b^3d + 3b^2c^2) + D(3b^2e + 6bcd + c^3) + C(2bf + 2ce + d^2) + \\
& + Bg\}o^6 + \ldots
\end{aligned}$$

Each term of the operator Φ raises the whole of the operand ϕ to the appropriate power, and the coefficients of the successive powers are then collected. Put $\Phi = \phi^{-1}$ or $\phi = \Phi^{-1}$. Then the coefficients of all the powers of o, except of the first, must be zero in the result. We thus obtain a series of equations $Bb = 1$, $Cb^2 + Bc = 0$, etc.; from which $B, C, D \ldots$ can be deduced if $b, c, d \ldots$ are given, and *vice versa*. Similarly, to obtain ψ_n^{-1}, let ψ_n operate on $o^{\frac{1}{n}} + co^0 + do^{-\frac{1}{n}} \ldots$, equate the coefficients in the result to zero, and solve the series of simple equations thus given. The series for $\Phi\phi$ can also be obtained by the operative division of Φ by ϕ^{-1}, and by Maclaurin's theorem, and in other ways. Note that the result is more involved if the operand contains an absolute term *a*—as in the value of $\Phi_0\phi_0$.

N. Deduce ϕ_1^{-1} from $[\phi_1]^n$.

We have only to put -1 for n in the value of the latter; but it is not quite easy to obtain this. It is laborious to form ϕ_1^2, ϕ_1^3, ... and to observe the law connecting the successive coefficients; and almost impossible to do so in the case of ϕ_0 (which contains an absolute term—see Section V (3) below). But we shall succeed more easily if in the series for $\Phi_1\phi_1$ just given we put $\Phi_1 = \phi_1^n$. Then, remembering that $\phi^{n+1} = \phi^n\phi = \phi\phi^n$, that is, that in this case the two operations are commutative, we can obtain another series for $\phi\phi^n$ which must be the same as the first. Equating the coefficients, first putting $B = b = 1$, and assuming that $C = nc$, we shall obtain $D, E, F \ldots$ in succession; so that

$$\begin{aligned}
[o + co^2 + do^3 \ldots]^n = {} & o + n_1\, co^2 + (n_1 d + n_2 2c^2)o^3 + \\
& + \{n_1 e + 5n_2 cd + (n_2 + 6n_3)c^3\}o^4 + \{n_1 f + n_2(6ce + 3d^2) + \\
& + (5n_2 + 26n_3)c^2d + (10n_3 + 24n_4)c^4\}o^5 + \ldots,
\end{aligned}$$

where n_1, n_2, n_3 . . . denote the binomial coefficients. Secondly, retaining b and assuming $B = b^n$, we evolve another series the sub-coefficients of which contain powers of b in geometric progressions (Part III).

(4) The expansion of ϕ_1^n is homologous with that of $(\phi_0)_n$ and may therefore be called the *operative multinomial theorem* or the *multinomial iteration theorem.* I do not know whether it has ever been fully set out and discussed; but it is fundamental, covers most of the present subject, and contains many approximations, continued fractions, periodic functions, etc. It may be obtained by other routes and put in other forms—but not easily when ϕ contains an absolute term. I will soon proceed to show that the inverts given by operative division are nothing but the first term of the expansion when ϕ does contain an absolute term—that is, are the first term when $n = \infty$ of the expansion of ϕ_0^n (Section V (3, 4, 5)).

These examples emphasise the extraordinary manner in which the algebraic symbols meet all their obligations. The permanence of algebraic form already notorious in the case of $(\phi)^n$ is now observed to hold equally well in the case of $[\phi]^n$. So far as I can see there is no *a priori* reason for this permanence: at all events it comes from the wonderful properties of the binomial coefficients.

By the use of o we obtain a merely formal algebra—almost an algebra of mere empty form. Questions of convergence do not arise because expressions in o have no numerical value: they commence only when we apply the operations to concrete numbers—that is, fill in the empty moulds given by the former; as will next be indicated.

V. (1) I now propose to approach the operative inverts from quite another direction, and to give instances in which they are evidently the algebraic expression of the arithmetic methods of Dary and Newton for approximating to the roots of numerical equations, or of the general procedure of which these methods are special cases. I have space only to touch upon the theme in this Part.

The method of Michael Dary,[1] Philomath, was first an-

[1] See Mr. Walter Stott's article in SCIENCE PROGRESS, October 1915. I regret that as this paper is being written on active service I cannot now give the references to the literature. Dary's original example was more general than the one which I use here—see Mr. Stott's article.—*Alexandria, October* 1, 1915.

nounced by him in a letter to Newton in 1674; has been subsequently rediscovered and somewhat elaborated by Legendre, Vogel, Lemeray, W. Heymann, C. Isenkrahe (1897), myself (1908, 1909), and others; but is not mentioned in the text-books which I have searched for it. Newton's method, which I think may have been derived from Dary's, is well known, but still very inadequately dealt with in such books.

I will use Newton's equation to illustrate both methods. For Dary's method write it in the form

$$x = \sqrt[3]{5 + 2x}.$$

Take any number x_0, substitute it for x in the right side of this equation, and let the result be x_1—that is, let

$$x_1 = \sqrt[3]{5 + 2x_0}.$$

Again substitute x_1 for x_0 in the right side of this equation and let the result be x_2: and so on, *ad infinitum*. Then, in this case, whatever real number x_0 may be, and whatever slips we may make in our numerical work, the *iterants* $x_0, x_1, x_2 \ldots x_m$ will infallibly approach the real root of the equation, namely 2·09455.... For examples let x_0 be 100, 0, or − 100; then we obtain the following:

100, 5·896, 2·560, 2·163, 2·0959, 2·0946, 2·0945, ...;
0, 1·710, 2·034, 2·085, 2·093, 2·0943, 2·0945, ...;
− 100, − 5·798, − 1·515, 1·253, 1·941, 2·071, 2·091, 2·094, ...

The figures are roughly calculated by the aid of Barlow's Tables, and purposely contain some errors. It is curious that this surprising and beautiful result should not be deemed worthy even of mention in many books on the Theory of Equations.

For Newton's method (which was however stated by him in another way), write the given equation in the form $x_1 = x_0 - \dfrac{f(x_0)}{f'(x_0)}$; which is in this case

$$x_1 = x_0 - \frac{5 + 2x_0 - x_0^3}{2 - 3x_0^2}.$$

Substitute for x_0 any number greater than $\sqrt{\frac{2}{3}}$ and proceed as in Dary's method. If $x_0 = 1$, or $= 100$, we have

1, 7, 5, 2·5, 2·163, 2·097, 2·0946, ...;
100, 77, 51, 34, 23, 15, 10, 7, 5, ... 2·0946, ...

It suffices to record integers only until we approach quite near the root. In this case, starting at 100, Newton's method is slower than Dary's, but outstrips it near the root. In both methods we shall save labour by commencing as near the root or roots as possible. In Newton's method, if we commence with $x_0 = 0$, or $= -100$, we shall obtain quite divergent series.

One reason why both methods receive such scanty exposition in the books is that while both may succeed at times both may also, perhaps more frequently, go wrong; and it cannot be said that subsequent writers (whose works I have seen) have altogether shown how to apply the methods, first with certainty, and secondly with the quickest possible results—much less how to obtain algebraic expressions for the series which the methods produce. It is time then that these questions be considered.

(2) The process of successive substitution employed in these methods is obviously the same as that of *operative volution* or *iteration* defined in Section I (4). For if

$$x_1 = [\phi]x_0 \quad x_2 = [\phi]x_1 \ldots \quad x_n = [\phi]x_{n-1};$$

then

$$x_n = [\phi]^n x_0.$$

In such cases we may speak of *the iteration of ϕ upon the base* x_0; and may employ the names *iterand* for ϕ and *iterants* for the series of operations $\phi, \phi^2, \phi^3 \ldots$, or for the series of numbers $x_0, x_1, x_2 \ldots$

The methods of Dary and Newton are special cases of a more general and complete theorem which I have not yet seen fully stated, but which may be put as follows.

First, if the equation $0 = fx$ possesses only one real root $x = X$ between any two values of x, $x = \alpha$ and $x = \beta$; and if f is continuous between α and β, and $f\alpha$ is positive, and x_0 is any real quantity between α and β: then the iteration of $0 + f$ upon x_0 will infallibly approach the root X if the tangents $f'x$ are always greater than -2 for values of x between α and β. And if the tangents $f'x$ are always greater than -1 for these values the iterants $x_0, x_1, x_2, \ldots$ will successively increase if $x_0 < X$ and will successively diminish if $x_0 > X$, until in both cases the root is nearly reached; and if the tangents $f'X$ lie between -1 and -2 the iterants will ultimately be alternately less than and greater than the root, but will still approach it; and the iteration will be quickest near the root if $f'X = -1$.

For ascending division by Section III (2) we divide o by $o - o^2$ and apply the quotient to a. Thus if X is the lesser positive root

$$X = a + a^2 + 2a^3 + 5a^4 + 14a^5 + 42a^6 + 132a^7 \ldots + \text{remainder}.$$

But by iteration we have

$$X = [o + (a - o + o^2)]^{\infty} x_0 = [a + o^2]^{\infty} x_0.$$

Hence $x_1 = a + x_0^2$

$$x_2 = a + (a + x_0)^2 = a + a^2 + 2ax_0^2 + x_0^4$$
$$x_3 = a + a^2 + 2a^3 + a^4 + 2(a + a^2)(2a + x_0^2)x_0^2 + (2a + x_0^2)^2 x_0^4$$
$$x_4 = a + a^2 + 2a^3 + 5a^4 + 4a^5 + 4a^6 + 4a^7 + a^8 + Rx_0$$
$$x_5 = a + a^2 + 2a^3 + 5a^4 + 14a^5 + 22a^6 \ldots + a^{16} + Rx_0$$

where Rx_0 means a definite number of terms in x_0. We see at once that the first r terms in the value of x^r agree exactly with the first r terms of the operative series. If $x_0 = 0$, as it may do in this case, Rx_0 evidently vanishes, leaving a set of terms beginning, in x_5, with $22a^6$ and ending with a^{16}, which must be equal to the remainder of the operative quotient. But it is not immediately clear why Rx_0 should vanish for *all* appropriate values of x_0.

Similarly for the equation $0 = a - x + cx^n$, we find that the first terms of $[a + co^n]^r x_0$ agree exactly with those obtained in Section III (2) as the value of $[o - co^n]^{-1} a$.

For the general case $\phi o = a - bo + co^2 - do^3 + \ldots$, we shall find it laborious to obtain even the first few iterants; but have

$$\phi o^2 = a - b(a - bo + co^2 \ldots) + c\{a^2 - 2abo + (b^2 + 2ac)o^2 - - 2(bc + ad)o^3 \ldots\} - d\{a^3 - 3a^2bo + 3(ab^2 + a^2c)o^3 \ldots\} + + e\{a^4 - 4a^3bo \ldots\} - \text{etc.}$$

$$\phi o^3 = a(1 - b + b^2) + a^2c(1 - 3b + b^2) + a^3\{2c^2(1 - b) - - d(1 - 4b + 3b^2 - b^3)\} + a^4\{e(1 - 4b) - cd(5 - 6b) + + c^3\} + \ldots + Ro.$$

Now if $\phi o = o + f$, then $f = a - (1 + b)o + co^2 - do^3 + \ldots$; and to obtain the least positive root of f by ascending division we employ the formula of Section III (3) and find, if $B = 1 + b$,

$$[o - \frac{c}{B}o^2 + \frac{d}{B}o^3 \ldots]^{-1}\frac{a}{B} = \frac{a}{B} + \frac{c}{B}\frac{a^2}{B^2} + \left(\frac{2c^2}{B^2} - \frac{d}{B}\right)\frac{a^3}{B^3} + + \left(5\frac{c^3}{B^3} - 5\frac{cd}{B^2} + \frac{e}{B}\right)\frac{a^4}{B^4} + \text{etc.}$$

Obviously, so far as we have examined the iterants, the two results agree, since the same groups of the original coefficients occur in both and because the powers of $\frac{1}{1+b}$ in the quotient are probably the sums of the powers of b within the small brackets in the iterants (ϕ_0^3) when continued to infinity. But we have still to explain why the powers of x_0 in the iteration series do not appear in the quotient.

For descending division take the form used in the preceding subsection to illustrate Dary's method, namely $[\sqrt{5+20}]x_0$. Expanding this by the binomial theorem we obtain

$$x_1 = g + \frac{1}{3}g^{-2} . 2x_0 - \frac{1}{9}g^{-5} . 4x_0^2 + \ldots$$

where $g = \sqrt[3]{5}$. This is the "general case" just briefly dealt with; and it will be easily seen that x_3 is taking the value of the operative invert given in Section IV (1), Example A. This suggests that the descending inverts represent the iteration, not of the original operation, but of *vicarious operations*.

Perhaps these conclusions were to be expected; but now consider Newton's vicarious form in the case of $0 = a - x + x^2$; that is, iterate $0 + \frac{a - 0 + 0^2}{1 - 20}$. By simple algebraic division this becomes

$$x_1 = a + 2ax_0 + (4a - 1)x_0^2 + (4a - 1)2x_0^3 + \ldots$$

This is the "general case" again. Iterating directly (and easily if $x_0 = 0$) or by the formula for ϕ_0^3 just given, we have

$$x_3 = a + a^2 + 2a^3 + 5a^4 + 14a^5 + 42a^6 \ldots + Rx_0.$$

This then is *precisely the same series as the one first obtained by simple iteration.* Observe however that in Newton's form x_3 approximates as closely as x_6 did by simple iteration; and note that the operative solution therefore gives the quickest one.

If, then, this theorem is generally true we shall have obtained two important results. *First,* operative division enables us to give, at least in many cases, an algebraic expression to the arithmetic process of iteration; and, *secondly* and conversely, iteration often enables us easily to determine the conditions of convergence in the operative series.

But we have still to explain the disappearance of the terms in x_0.

(4) I have space in this Part to select only an incomplete

proof of the propositions contained in the two previous subsections; but it has the merits of being brief and of dealing with both propositions together.

If possible, let k be a number such that $\phi k = k$. Then obviously $\phi^2 k = k$, $\phi^3 k = k$, . . . and $\phi^n k = k$.

Next, find the successive tangentials of $\phi^n k$. By the ordinary rule

$$\frac{d}{do}\phi^n k = [\phi'\phi^{n-1} . \phi'\phi^{n-2} . \phi'\phi^{n-3} \ldots \phi'\phi . \phi']k$$
$$= (\phi' k)^n.$$

To form the second tangential take logarithms of both sides of the first of these equations, and differentiate. Hence

$$\frac{d^2}{do^2}\phi^n k = (\phi' k)^{n-1} . \phi'' k . \{1 + \phi' k + (\phi' k)^2 + (\phi' k)^3 \ldots\}.$$

The subsequent tangentials must therefore all contain the factor $(\phi' k)^n$ multiplied by groups of geometric progressions in $\phi' k$ affected by $\phi'' k$, $\phi''' k$, . . . and so on. Now if $\phi' k$ is numerically less than unity, $(\phi' k)^n$ becomes infinitely small when n is made infinitely large. Hence as $\phi' k$, $\phi'' k$, $\phi''' k$ . . . are supposed finite, and the geometric progressions referred to tend to finite limits when $\phi' k$ is numerically less than unity, it follows that, under these circumstances, $(\phi^n)' k$, $(\phi^n)'' k$, $(\phi^n)''' k$ . . . all vanish. Thus by Taylor's Theorem

$$\phi^n(k + h) = k;$$

at least if h is not too large. This is the equation of a straight line parallel to the axis of x and passing through the point (k, k), h being a length along x.

Now let the continuous curve f have a succession of real roots X_1, X_2, X_3 . . . Then the curve f' must be alternately positive and negative at these roots. Let it be negative at X_1, X_3, X_5 . . .; and let $\phi = o + f$. Then at all the points $X_1, X_2, X_3 \ldots, \phi X = X$; that is, $\phi^n X = X$; that is, $[o + f]^n X = X$. But, as just seen, we have also $[o + f]^n (X + h) = X$ if n is very large and if $\frac{d}{do}[o + f]X$ is numerically less than unity; that is, if $[1 + f']X$ is less than 1 and greater than -1; that is, if $f'X$ is negative and greater than -2. Putting $x_0 = X + h$, we therefore have $[o + f]^\infty x_0 = X$, if $f'X$ is negative and greater than -2.

But this condition can hold only for the roots $X_1, X_3, X_5 \ldots,$

since it is only at these roots that $f'X$ is negative. At the roots X_2, X_4, X_6 . . ., $f'X$ is positive; therefore $1 + f'X$ is greater than unity; therefore $\frac{d}{do}[o + f]X$ is greater than unity; therefore $\frac{d}{do}[o + f]X$ is infinitely large when n is so; therefore $[o + f]^n (X + h) = X$ only when $h = 0$.

(5) We find then the remarkable fact that if f', or a vicarious curve, lies within certain limits, the curve $[o + f]^n$ approximates when n is indefinitely increased to a series of finite straight lines alternately parallel and perpendicular to the axis of x; the parallel straight lines passing successively through the points (X_1, X_1), (X_3, X_3), (X_5, X_5) . . ., and the perpendicular lines through the points (X_2, X_2), (X_4, X_4) . . .; the lines meeting end to end; and the lengths of the inner lines parallel to x being equal to the differences between the even roots, and those of the inner perpendicular lines being equal to the differences between the odd roots—in fact, like the section of a flight of steps. Or, perhaps, we ought to consider that the perpendicular lines do not exist at all, and that the curve $[o + f]^\infty$ becomes discontinuous at the even roots of $fx = 0$.

The reader will have no difficulty in constructing geometric illustrations of these propositions, especially by the use of the operative-unit curve, or mid-axis, o. The equation $0 = 0{\cdot}6 - 1{\cdot}1\, x + 0{\cdot}6\, x^2 - x^3$, of which the roots are 1, 2, 3, affords a convenient example; but he should observe that the propositions apply to many equations besides rational integral ones. For the latter the results may be summarised as follows:

If X is a root of the equations $0 = f(x)$ and $0 = F(x)$, and if F and x_0 are properly selected, then

$$X = \sum_{n=0}^{n=\infty} [F][o + F]^n x_0 = [o + F]^\infty x_0$$

$$= \frac{o}{F(o) - F(x)} : F(o).$$

Further proofs with geometric illustrations, and with corollaries and applications, will, I hope, be given in the third Part.[1]

[1] The validity of solution by iteration is usually based upon the familiar theorem that if $f^n(x)$ comes to a limit that limit is a root of the equation $x = f(x)$. But this is of itself insufficient because there is no *a priori* reason why $f^n(x)$ should come to any limit at all, and secondly because the usual proof of the theorem referred to, though easily obtained, does not indicate the conditions of convergence.

THE INFLUENCE OF RESEARCH ON THE DEVELOPMENT OF THE COAL-TAR DYE INDUSTRY.—PART II

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THE THIRD PERIOD: 1880–1897. SUBSTANTIVE DYES, AND RESEARCH ON INDIGO

UP to this date it was invariably necessary when dyeing cotton to mordant it first (that is, to deposit certain metallic compounds on the material by a preliminary treatment) in order to render it capable of taking up the dye, whereas most dyes, with the exception of alizarine, were capable of dyeing unmordanted wool direct from a suitable dye-bath.

The extra cost of mordanting the cotton was thus a serious item, and in addition it was often very difficult to dye a mixed cotton and wool material to a uniform depth of colour in both materials. The announcement by Boettiger, therefore, in 1884 that he had succeeded in preparing a new red disazo dye-stuff, which was named Congo red, capable of dyeing cotton directly without the use of a mordant, might have been expected to arouse the greatest interest; but unfortunately the fact that Congo red is very sensitive to acids (it is, in fact, much used as an indicator for acids and alkalies) caused dyers to lose all interest in it.

It was soon found, however, that various other related dyes also had the power of dyeing cotton directly and possessed greater fastness than the original dye, so it was not long before systematic chemical investigations brought about the discovery of numerous direct or "substantive" dyes in all colours, and to-day we have a very large variety of such dyes in the market to choose from.

The Fourth Period: 1897 and on. The Technical Synthesis of Indigo, New "Vat-dyes"; Sulphur Colours

From 1880 onwards there was one question overshadowing all others in importance, namely the possibility of synthesising indigo from coal-tar products.

It will be recalled that in 1826 Unverdorben had prepared aniline for the first time by distilling indigo, but all attempts to reverse the process, if any had been made, were fruitless.

Adolph von Baeyer, whilst working at Berlin University from 1866–1870, had proved the constitution of indigo and had indeed synthesised the dye by the reduction of isatin, which was however of no practical importance, since the latter was only obtainable from indigo itself.

Nevertheless the fact that the annual production of indigo in those days was some 4,000 to 5,000 tons a year, worth £4,000,000 to £5,000,000, convinced chemists that the problem was one well worth attempting to solve.

The final impulse undoubtedly came from the synthetic production of alizarine during the years 1867–1870, where for the first time a naturally occurring dye was produced in the chemical works from coal-tar, and thus encouraged chemists to attempt the yet more difficult problem of indigo.

From 1870 to 1880 renewed attempts were made to work out a commercially practicable method for synthesising indigo, but without success.

In 1880 a step was taken which could only have happened in Germany and where the boards of directors were composed largely of able and far-seeing chemists: the two great firms of the Badische Company at Ludwigshaven, and Meister, Lucius and Brüning at Hoechst joined forces in order to attack the problem systematically, and entered into an agreement to carry on researches conjointly on the commercial synthesis of indigo, sharing expenses and results.

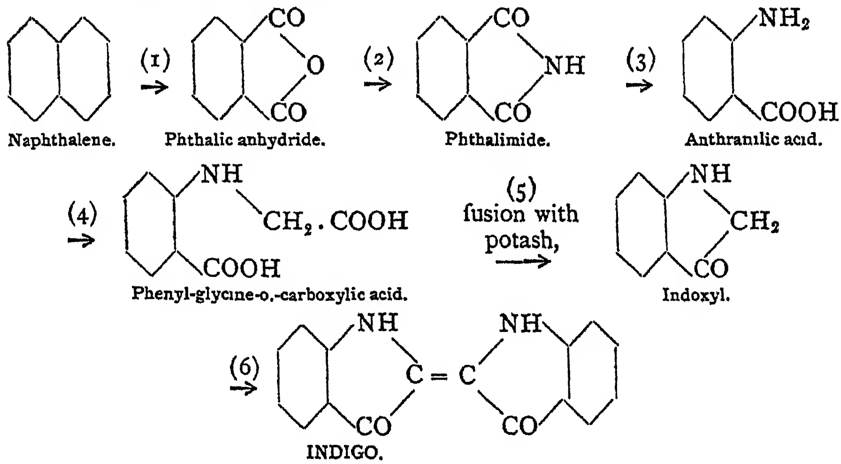
They acquired the patent from von Baeyer for his synthesis of indigo from toluene via ortho-nitro-benzaldehyde, but the method was found to be impracticable on a commercial scale although a small amount appears to have been made this way.

After a decade of unsatisfactory results, which would have discouraged any firms not possessed of a great degree of optimism founded on the highest chemical perception, they

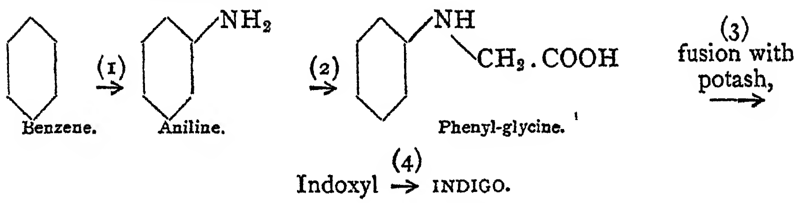
were once more encouraged by the appearance of two new syntheses of indigo discovered by Prof. Heumann, the one starting from naphthalene, the other from benzene.

Although somewhat complicated, it is worth while noting the lines taken by each method :

(1) *From Naphthalene.*



(2) *From Benzene.*



Unfortunately, however, both the syntheses had serious drawbacks. By the first method the yields obtained in all the processes, from phthalic anhydride on, were excellent, particularly in the stage (5), which is the most difficult to carry out, but there was no cheap method of obtaining phthalic anhydride except by oxidising naphthalene with fuming sulphuric acid, and here the yield was so poor that it was almost impracticable.

In the second method, from benzene via aniline, excellent yields were obtained in each stage of the operations until (3), the fusion with potash ; but here also the yield was so minute, only amounting to quite a few per cent. of the theoretical yield of indigo, that the process had to be abandoned.

Several years' further work was devoted to the problem, but finally it was decided that Heumann's method was of no technical use, and in 1897, after seventeen years' co-operative research and the expenditure of over half a million sterling, the agreement was dissolved and each firm decided to go its own way.

The Hoechst works nevertheless succeeded in producing some quantities of indigo from anthranilic acid obtained from ortho-nitro-toluene.

In this connection an interesting point arises: if toluene were taken as the starting-point for the syntheses of indigo, the former must, of course, be obtained from coal-tar, of which it forms only a small proportion, the chief components being benzene and naphthalene. Now the annual production of crude benzene amounted then to some 25,000 to 30,000 tons per annum, of which only some 5,000 to 6,000 tons were toluene, which was already in demand for the preparation of benzaldehyde for making malachite green and so on.

Now by the best methods 1 ton of indigo could not be made from less than 4 tons of toluene, and as the annual demand for indigo was not less than about 5,000 tons, it is obvious that the world's output of toluene could only cover a quarter of this, whilst if the output of toluene were increased to the necessary 20,000 tons, simultaneously there would be at least 80,000 tons more benzene to be disposed of besides the production of a colossal amount of coke for which there would be no use.

The introduction of benzol as a motor fuel, and the recovery of benzol and toluol from coke-oven gases, has to-day considerably altered the state of affairs, but twenty years ago the objections noted were sufficient to decide against the use of toluene as a starting-point. In spite of many difficulties, financial and chemical, the Badische Company produced a certain amount of synthetic indigo, but it was not until 1896 that the problem was really solved.

In this year, shortly before the dissolution of the agreement between the two firms, an accidental discovery settled the matter. Sapper, of the "Badische Anilin und Soda Fabrik," whilst researching on the preparation of phthalic anhydride by oxidising naphthalene with fuming sulphuric acid, by chance broke the thermometer registering the temperature of the melt.

On allowing the mass to cool it was found that instead of the usual poor yield of the product the naphthalene had been converted almost quantitatively into the anhydride owing to the catalytic influence of the mercury.

It was at once seen that the solution of the indigo problem was at hand.

The only remaining question now was the cheap production of fuming sulphuric acid as an oxidising agent.

This difficulty was soon overcome by the researches carried out at the Badische works under the able guidance of Knietsch, by extending and improving the contact process for the manufacture of the acid from sulphur dioxide and air.

The process of oxidation may be represented by a scheme thus:

$$\begin{tikzcd} (\text{Sulphur dioxide} + \textit{AIR}) \arrow[r] & (\text{Sulphur trioxide}) \arrow[d] \\ (\text{Sulphur dioxide} + \textit{PHTHALIC ANHYDRIDE}) \arrow[u] & (\text{Sulphur trioxide} + \textit{NAPHTHALENE}) \arrow[l] \end{tikzcd}$$

so that the whole process is really carried out at the expense of the cheapest of all oxidising agents, namely atmospheric oxygen. Since already by 1900 40,000 tons of sulphur dioxide were produced annually by this process and reconverted into the trioxide (the figures for recent years must, of course, be many times greater), it will be realised what a huge affair this process is.

At the same time it was necessary to have large quantities of chlorine and acetic acid to produce the necessary chloracetic acid. The acetic acid was produced by the distillation of wood, of which in 1900 over 100,000 cubic metres were distilled for this purpose; the chlorine was produced by the electrolysis of potassium chloride from the Stassfurt deposits, and was purified by liquefaction according to the method worked out by Knietsch some years previously. In its turn the enormous volumes of hydrogen so produced as a by-product may be utilised either for filling airships, or, as the result of the joint researches of Prof. Haber and of the Badische Company, are caused to unite with the waste nitrogen produced in Linde's process for manufacturing oxygen from liquid air, to give ammonium salts for fertilisers and for the production of nitrates needed to manufacture explosives.

These facts show once again the intensive cultivation of this field of industry by the Germans, and the overwhelming importance of continuous and painstaking research.

The successful production of cheap phthalic anhydride from naphthalene thus forged the one missing link in the chain of processes necessary for the commercial synthesis of indigo from coal-tar, and from that date—1896—the problem may be said to have been solved. As far as the Hoechst works are concerned, however, matters must have seemed somewhat serious, as they had invested enormous sums of money in research in indigo and had obtained practically no return, and now had the chagrin of seeing the entire industry passing apparently into the hands of their competitor and erstwhile partner.

In 1900, however, Pfleger, of the Deutsche Gold und Silber Scheide Anstalt in Frankfurt, found that if phenyl-glycine (obtained from aniline) were fused with sodamide instead of with caustic potash, a nearly quantitative yield of indigo was produced, so that after acquiring the patent the Hoechst works were now in a position to challenge the Badische Company for the world's indigo trade, resulting in a big drop in prices; but the two firms soon came to an agreement on the matter, and from that day forward the two great chemical factories have absolutely controlled the world's markets and incidentally have almost ruined the Indian indigo industry, as may be seen from these figures:

Value of Exports of Indigo from British East India

Year.	Value in £.
1896	3,569,670
1897	1,980,319
1902	1,234,319
1905	556,405
1908	424,849
1911	225,000
1913-14	65,000

whilst, of course, the acreage under cultivation has sunk *pari passu*:

Year.	Acres.
1894-5	552,000
1900-1	300,000
1904	223,000
1905	171,000

the almost complete replacement of the ordinary colours for most purposes.

The last decade has also seen the development of yet another class of dyes, the so-called "sulphur colours" produced by treating almost any organic substance with sulphur and sodium sulphide.

For the most part they are amorphous substances of indeterminate chemical character made by purely empirical methods, reminding one very much of the early days of dye chemistry.

The colours so produced are of all shades, but are somewhat drab and flat in tone and are used chiefly for dyeing the cheaper classes of goods, such as shoddy.

Such then is the position of the coal-tar colour industry at the present day.

Owing to the adoption of scientific methods, the encouragement of research, and the fact that the great German firms are in charge of chemists, as they should be, and not under the thumbs of business men, however able the latter may be, we find that Germany has established practically a world monopoly of dye-stuffs.

At the outbreak of war German firms provided about six-sevenths of the world's consumption of synthetic dye-stuffs, valued at £12,000,000 to £15,000,000, of which Great Britain imported £1,800,000 in 1912, her own production being only to the value of about £200,000.

Recent figures are not available, but for 1904 the proportion of the exports of German synthetic dyes going to various countries was officially given as :

Country.	Per cent. of total exports.
U.S.A.	20·4
Great Britain	15·5
Russia	10·1
Austria	10·1
China	5·2
India	4·9
Italy	4·9
Japan	4·5
France	4·4
Switzerland	3·4
Belgium	2·4
Holland	1·6
Other countries	12·0

Note on the Causes of German Supremacy in the Coal-tar Dye Industry

It would be difficult to assign within concise limits the causes of German supremacy in the industry ; two main causes must be taken into account :

(1) The attention devoted to research work.
(2) The manipulation of the Patent Laws for the benefit of the German firms.

As regards the first reason, it is probably too well known to need emphasising, but when it is remembered that the Hoechst works employ 350 research chemists, the Badische Company about the same number, and the Bayer Company nearly as many—in all about 1,000 university-trained chemists, many with the highest degrees—it will be realised that this great number of highly trained, scientific brains constantly engaged in discovering new dyes, evolving improved methods for making old ones, and so on, has exerted and will continue to exert a preponderating influence on the development of the German industry, and the fact that the two German firms at Ludwigshaven and at Hoechst were ready to spend nearly a million pounds sterling in *pure research* work connected with the synthesis of indigo shows to what an extent the industry is entrusted to the hands of research chemists ; and in the annual balance sheet of every German chemical firm a large proportion of the profits is set aside for research work as a matter of course, in precisely the same way as other portions are reserved for payment of interest, repayment of loans, and so on.

Perhaps a clearer idea of the position of the chemist in such a works may be gathered from the figures published by the Hoechst works :

Date.	Engineers, etc.	Clerks.	Foremen.	Workmen.	Chemists.	Ratio of chemists to workmen.
1863 . .	—	—	—	5	1	1 : 5
1875 . .	—	12	—	370	12	1 : 31
1880 . .	10	45	40	1,650	25	1 : 66
1888 . .	9	86	50	1,860	57	1 : 33
1912 . .	74	611	374	7,680	307	1 : 25
(1914.	Unofficial values *circa*		. .	10,000	350	1 : 29)

It may be noted also that, taking the various branches of industry as a whole, there is in Germany one university-trained

chemist to every fifty workmen, whilst in Great Britain the ratio is certainly not higher than 1 : 500.

(2) As regards the German use or misuse of the British Patent Laws the subject is far too large for full discussion in this place. It will suffice to mention, however, that the German firms have taken full advantage of the laxity of the British Patent Laws by filing numberless blocking patents to prevent competition, and even of dummy patents never intended to be worked, but deliberately calculated to mislead competitors.

The following table demonstrates the activity in this branch since 1865; it gives also an indication of the volume of original work produced by the German firms. (Since no official figures are issued by the Patent Office, the following table has been compiled from the official specification abridgment lists and may be taken as substantially accurate.)

TABLE GIVING NUMBER OF PATENTS TAKEN OUT IN GREAT BRITAIN BY BRITISH AND GERMAN INDIVIDUALS OR FIRMS FOR SYNTHETIC DYES AND INTERMEDIATE PRODUCTS. (In five-year periods.)

Period.	German.	British.
1856–60	8	20
1860–65	21	54
1866–70	17	23
1871–75	8	11
1876–80	47	13
1881–85	113	15
1886–90	201	39
1891–95	386	29
1896–1900	427	52
1901–05	447	38
1906–10	561	30
(1911–12	252	11)

Further comment is needless.

ESSAY-REVIEWS

A NEW AID TO MATHEMATICAL KNOWLEDGE, by C.: on **Catalogue of Current Mathematical Journals, etc.,** with the names of the Libraries in which they may be found. Compiled for the Mathematical Association. [Pp. 40.] (London: G. Bell & Sons, 1913. Price 2*s.* 6*d.*)

THE pamphlet which is here reviewed recalls to mind a piece of similar work done by Augustus de Morgan about fifty years ago, the well-known catalogue of Arithmetical Books. It may be doubted whether either Prof. de Morgan or Mr. W. J. Greenstreet, who from internal evidence appears to be the editor of this Catalogue, would have addressed themselves to their heavy and self-imposed tasks if they had not been trained by studies in which scientific co-ordination is a compelling force. The trained librarian achieves tasks which, it is true, are of greater magnitude, but his work is done under conditions which the amateur cannot imitate. There is another point of similarity between the catalogues; each represents a special piece of work which at the moment of its execution does not fall within the routine of a library. De Morgan's work was perhaps the first of a series of special catalogues which are now repeated in a hundred forms every year; indeed, it is the hall-mark of a bookseller of the first rank that he publishes such special catalogues devoted to one or other particular class of books. Mr. Greenstreet too has compiled an essentially new kind of catalogue, and one which must surely, before long, have many imitations in other subjects. It is with the object of drawing attention to the design of the Catalogue and in the hope that others may do for different branches of science the service which the editor has rendered to mathematics, that this article is written.

Mathematicians are well served with catalogues of the literature of their subject. The Royal Society issues its great International Catalogue of scientific papers at regular intervals, while the classified list of scientific papers up to the close of the nineteenth century is now complete, both for mathematics and

mechanics. Besides these invaluable works the *Jahrbuch über die Fortschritte der Mathematik* has been published yearly since 1871. This journal gives a short abstract of papers published in each year beginning with 1868, and is arranged in subjects and indexed. In addition the *Revue semestrielle des publications mathématiques* was commenced in 1893 by the mathematical society of Amsterdam. It is more up-to-date than the German periodical, being not three years, but only three months, behind the publications which it reviews: in it the titles of papers in the various serial publications is given, together with short abstracts, and it is, as its title indicates, published twice a year. But though from one or other of these works information is readily accessible as to new work which is published, yet it is not possible except in London, Oxford, or Cambridge to have access to the greater part of the seventy journals which constitute the main channels in which new work is communicated to the mathematical world.

In 1906 Sir Thomas Muir investigated the supply of mathematical journals available in the South of Scotland and published in the *Proceedings of the Royal Society of Edinburgh* a valuable paper on Library Aids to Mathematical Research. In this paper he takes sixty-seven important serials and tells us that just about half are to be found in the libraries of the South of Scotland, Edinburgh possessing thirty-one, and Glasgow twenty-three, and of these twenty-one are duplicated. Without pursuing the problem which Sir Thomas Muir discussed at length, it may be stated that in his paper he placed before Scottish mathematicians, or rather mathematicians residing in Scotland, most valuable information with regard to accessible periodical literature. Now the editor of this catalogue has elaborated and extended Sir Thomas Muir's pious labours. Here, in the brief compass of forty pages, we have most valuable information with regard to the mathematical periodicals which are to be found within the United Kingdom.

The Catalogue is divided into various sections: thus following the title page, preface, and list of abbreviations we have the section (22 pages) of current mathematical journals, which constitutes the kernel of the pamphlet; then follows a general alphabetical index of the journals, a society index, and a geographical index.

The list of journals is far more extensive in this catalogue

than that given by Sir Thomas Muir, as it contains no less than 182 entries : some of these, however, relate to the same periodical; the Berlin Academy, for example, occupies five sections numbered from 57 to 61 with the varying titles, French and German, under which it has been published. The list is arranged according to countries. It may be of interest to enumerate the countries and the number of entries under each country: Austria, 5 ; Belgium, 2 ; Denmark, 6 ; France, 28 ; Germany, 39 ; Holland, 7 ; Hungary, 3 ; India, 3 ; Italy, 25 ; Japan, 2 ; Norway, 3 ; Poland, 4 ; Portugal, 4 ; Roumania, 1 ; Russia, 11 ; Spain, 1 ; Sweden, 14; United Kingdom, 14; America, 6 ; and unclassified, 2. Information was obtained from 49 libraries concerning the series of these 182 journals in their possession. Each library is indicated by an appropriate abbreviation, thus L.L. is the London Library, Q.B. is Queen's College, Belfast, C.P.S. is the Cambridge Philosophical Society. Each journal is described briefly by format, place of publication, and a short general history, where necessary ; appended to each entry is a list of libraries which possess the journal and the exact holding of each, even in one instance a note is added ("willing to give away")—an offer which now probably may be omitted. Thus as a specimen we may take the entry which covers the great French mathematical journal :

FRANCE.

16 **Annales de Mathématiques pures et appliquées.** Recueil périodique, rédigé par **J. D. Gergonne** et J. E. Thomas-Lavernède. 1810–1831, vols. 1–22, No. 2. 4° Nismes and Paris.

Aber. 1810–1831. *Brist* 1810–1831. *Man. L. & P.* 1830–1831. *R.S.* 1810–1831. *U.C.L.* 1810–1831.

(*Continued as*)

17 **Journal de Mathématiques pures et appliquées,** ou recueil mensuel de mémoires sur les diverses parties des mathématiques. Publié par **J. Liouville** (H. Resal, C. Jordan). 1836→ 4° Paris.

Aber. 1836→ *Arm.* 1905→ (sér. i.–v. wanting). *Birm.* 1836→ *B.M.* 1836→ *Bod.* 1856→ *Brist.* 1836→ *Cardiff* 1836→ *C.U.* 1836→ *Liv.* 1836→ (5^e^ sér. vol. 9, iii–iv.; vol. 10, iii. 6^e^ sér. vol. 2, i. wanting). *Man.* 1875→ *Man. L. & P.* 1852, and tables of vols. 1–15. *Pet.* 2^e^ sér. 1, 1856–3^e^ sér. 9, 1883. *R.S.* 1836→ *R.S.E.* 1837→ *St. A.* 1836→ *S.K.* 1836→ *T.C.C.* 1836→ *T.C.D.* 1836→ *U.C.L.* 1836→

This extract shows better than any explanation the nature of this Catalogue. Any mathematician who wishes to consult a paper in *Liouville* will know at once the library in which he is sure to find it. If he resides in London, for instance, he will

not go to the British Museum for it, but will turn to the Royal Society or the University College. The importance of such a service as this will be readily appreciated. The more one dips into the Calalogue the more curious are the facts discovered concerning the libraries which have and the libraries which have not certain journals. Thus if the Catalogue is correct there are only eight libraries which contain sets of such an important journal as the *Transactions of the American Mathematical Society*; again, over twenty journals are only in a single library. A further point of interest is that the only Public City Library mentioned is Nottingham; it was with some disappointment that further search discovered that the journal owned by Nottingham is the *Journal of the Institute of Actuaries and Assurance Magazine* and that the library possessed only an incomplete set of this journal.

An object which the editor of the Catalogue had in view was the establishment of exchange between different libraries; certainly the Catalogue renders this feasible, as librarians can see at a glance the libraries which have defective sets of any particular journal. But whether it is likely that librarians will be ready to give up their incomplete sets is another matter; they may deem it a sounder policy to fill up in the open market the gaps which exist in their own sets.

Without doubt there are omissions in the Catalogue, and the editor probably knows them better than any one else; it would be a thankless and a difficult task to hunt for them. Even a superficial study convinces the reader of the general completeness of the Catalogue and of the marvellous care and labour which have gone to its compilation. The task is, however, one which is never complete; the Mathematical Association, under whose auspices it is undertaken, has in its continuation at regular intervals a field of useful service to the subject in whose interest it is founded. Further, the Catalogue is a record of the progress of peace; some of us know already how serious are the breaches in continuity of many of the journals which figure in this catalogue; it is a sad thought to reflect upon the additional labours of a future editor which will be caused by the terrible year 1915. When however peace is happily restored, leading scientific societies may well address themselves to the task of compiling similar catalogues in their special subjects.

THE INSECT AND THE HERMIT, by CHARLES H. O'DONOGHUE, D.Sc., F.Z.S.: on **Fabre, Poet of Science**, by Dr. C. V. LEGROS, translated by BERNARD MIALL. [Pp. 352, with a Frontispiece.] (London: T. Fisher Unwin, 1915. Price 10*s.* 6*d.* net.)

FROM the dawn of Biology insects have attracted attention and served as the examples, real and imaginary, of many virtues and pointed the moral of many fables. What boy away from the stunting confines of a town has passed through his school-days without at any rate a mild attack of bug-hunting? How many men have carried the after-effects of this into later life, and become collectors of some particular group of insects? Nor is this a matter for surprise when one considers the enormous number of species comprised within the class, many more indeed than the rest of the animal and vegetable kingdoms put together, their universal plenty, and the facility with which most of them can be handled. The variety of their forms, often quaint and bizarre, and the glory of their flashing colours cannot fail to strike the most superficial observer. The marvellous manner in which they are adapted to their many different modes of life invariably calls forth admiration. Perhaps they appeal to us most of all because we find among them animals living in large communities exhibiting a social organisation not unlike that of man himself, and in some cases better, for shirkers and wasters are not tolerated. They offer an inexhaustible field for research, but only yield their secrets to the indefatigable worker.

To France we owe much of our knowledge of insect life and structure. Réaumur, by his accurate observations, led the way in these studies, and was followed by the Hubers with their work on the bee and Léon Dufour. The last named, although he has done much good work, is likely to be remembered chiefly as the author who inspired Fabre. After Réaumur, in spite of the other authors, a gap remained and the study of insects was somewhat neglected. Let us turn to consider the man who was to take up the threads.

Jean Henri Fabre was born in December 1823 in Saint-Léons, a small village in the Haut Rouergue. His parents were poor farmers, and his early life was passed under wretched conditions. When seven years old he went to the village school kept by his godfather; thence five years later he passed to the school in the small town of Rodez. At nineteen we

find him with his diploma entering the college of Carpentras as a primary teacher, with the munificent salary of £28 per annum. These were the inauspicious circumstances in which he passed his early years, but already his inclinations had shown themselves, for he had learned to know and love the creatures of the countryside, and some of his earliest recollections are of discoveries in nature knowledge, such as the first time he heard the bell-ringer frog. His dogged perseverance and natural aptitude for study had also been manifested, and he shortly passed his *baccalauréats* and two licentiate examinations. Here for the first time we find indications of what almost amounts to a desire to avoid other people, for he writes to his brother, " When something embarrasses you, do not abuse the help of your colleagues ; with assistance the difficulty is only evaded ; with patience and reflection *it is overthrown* "; but at the same time his own method is revealed in the following words : " Try only for a few days this method of working, in which the whole energy, concentrated on one point, explodes like a mine and shatters obstacles ; try for a few days the force of patience, strength, and perseverance ; and you will see that nothing is impossible." When he missed two successive appointments he seems to have shown an unnecessary amount of indignation and resentment, but eventually he obtained a post in Corsica. The wonderful flora and fauna of this island were a revelation to him and enamoured him more and more of the pursuits of natural history, and he collected and observed with remarkable ardour. A year or so later he returned to the lycée of Avignon. Up to this time his studies had included mathematics, physical sciences, chemistry, and classics, besides his natural history, but he had not yet limited his attentions.

By chance he read a volume by Léon Dufour which awakened his curiosity, and he started to investigate some of the problems for himself. This decided his future career, and from this time forward he steadily devoted himself to biology and became world-renowned as one of the most remarkable of entomologists. All the time two sides of his nature were being developed. His was one of those remarkable characters that possessed the power of poetry to a high degree, indeed his *Souvenirs entomologiques* are coloured throughout by poetic imagery, and at the same time had also the scientific tem-

perament that enabled him to go on making observations and experiments in one case for forty years, until he had solved the problem he had embarked upon. How rarely do we find these two temperaments well developed in the one individual! More often is the absence of one so marked that the other is unbridled and leads to very lopsided results. In his early life he suffered a great deal of hard fortune, that seems to have alienated his sympathy from other people and strengthened his desire to be alone. Consequently we find he takes the first opportunity of retiring to the seclusion of Sérignan, a tiny hamlet in Provence. Here he lived almost as a hermit for many years, during which he devoted practically the whole of his time to his beloved insects.

The volumes of *Souvenirs entomologiques* contain his main work, and a very remarkable contribution to scientific literature they form, but in addition he also wrote many papers. The poet in him would not allow him to rest content with mere anatomical studies, and we find him constantly inquiring into the insect mind. Baffled here and there, he returns again and again to the quest with remarkable persistency. He very carefully tried to distinguish between the parts played by instinct and intelligence in insect behaviour, and came to the conclusion that the animal is guided almost entirely by the former and the application of intelligence is rare, and when tried only within very narrow limits. His discoveries in this field, and also of the remarkable relations between parasite and host, which he could not conceive as being the result of natural selection, led him to reject the current evolutionary views. What though we do not always follow his interpretations! what though he made some small errors! We cannot fail to be inspired by his devotion and charmed by the delightful language in which he relates his observations. Old tales are sifted and put to the test, new and more wonderful stories of life-histories take their places, and the books have become a perfect storehouse of wonders.

In *Fabre, Poet of Science*, Dr. Legros, one of his ardent disciples, seeks to lay before us an account of his master's life and the inspiration of his works. This he does successfully, though at the same time we cannot help feeling that it is an admirer who is writing, as Fabre himself warns us in a short preface. It is a thoroughly readable volume, and should do much to call

attention to Fabre's work, most, if not all, of which can be read with profit and enjoyment by layman and scientist alike. The chapters are annotated, but in some places the reference numbers seem to be wrongly placed in the text.

Why it is that Fabre and his works remained practically unknown is hard to understand, for he certainly deserves a wider circle of readers than he had a few years ago. Perhaps it is because of his retiring or even slightly resentful disposition and his absolute distaste for any sort of advertisement. Then, too, he was a very bad correspondent, only scanning most of the letters sent to him, and writing infrequently to but a few people. Also it must not be forgotten that he strongly opposed the Evolution Theory at the height of its popularity, and this, in our opinion, without thoroughly grasping it. Finally, he paid but little attention to the works of other scientists. Allowing all this, however, there still remains the wonderful mind, the unceasing labour, ingenious experimentation, and "infinite capacity for taking pains." When he writes it is as a poet writing of the things he loves, and his vivid personality is infused into his words. His inimitable writings will stand the test of time, and should prove an inspiration to many, for who before him has sung the glories and fascination of the insect like this hermit poet of Provence?

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

History.—A very useful summary of the contents of ancient Indian mathematical works is given by G. R. Kaye in a little book reviewed elsewhere in the present number. Prof. Florian Cajori has written a very detailed and valuable paper giving an account of the works of William Oughtred (*Monist*, 1915, **25**, 441). The most interesting points about Oughtred's works are the amount of symbolism used, and the fact that he was the first inventor of the slide rule, though not the first to publish anything about slide rules. Cajori (*Open Court*, 1915, **29**, 449) also gives a biography and portrait of Oughtred.

Logic and the Principles of Mathematics.—The number of the *Revue de Métaphysique et de Morale* for September 1914 only appeared in June 1915, and contains a very complete account of the work of Louis Couturat followed by a bibliography. The account is by André Lalande, and we can trace in it the development of Couturat's thought from *De l'Infini Mathématique* of 1896, through his publications on the logic of Leibniz (1901, 1903) which were partly based on original research at Hanover, through his study of the logical calculus (1905), up to his exposition of modern work (chiefly by Russell) of the principles of mathematics (1905 and later). Since 1901 Couturat became more and more occupied with the details of international languages and particularly " Ido " : this was also an effect of his study of Leibniz. His work on Leibniz is far the most permanent part of his reputation, and mathematical logicians will ever be grateful to him for it.

Dr. Charles A. Mercier (*Mind*, 1915, **24**, 386) contends in his usual amusing way that the syllogism is neither the only nor psychologically the most natural form of argument. Certainly any modern logician would agree with him on this point. Further, Mercier maintains that logicians merely assert that there is a universal in every argument, and that in fact this is not true.

As a result of Rignano's articles referred to in *Science Progress* of July 1915 (**10**, 116), Giuseppe Peano (*Scientia*, 1915, **18**, 165) has treated, in an interesting article, the function of symbols in mathematics. The part played by symbolism in arithmetic, algebra, and the geometry of vectors is first treated, and stress is laid upon the fact that symbolism provides a means both of shortening work and, what is much more important, of giving a new classification of ideas. In arithmetic this may be illustrated by the Hindu-Arabic system of numeration, and in the symbols of algebraic operations by the fact that the verbal equivalents of formulæ involve words which stand also for ideas not used in those formulæ. In the evolution of algebraic symbolism there are three stages: (1) Ordinary language; (2) the technical language of Euclid, in which there is a one-to-one correspondence between words and ideas; (3) the abbreviation of the words of this technical language, which began about the sixteenth century and was nearly complete in the system used by Newton. Mathematical logic or the algebra of logic was the last to appear, but is in no way inferior to the above symbolisms. In any mathematical reasoning there are specially mathematical and general logical ideas; mathematical logic represents the latter by symbols and finds that the ideas are subject to the rules of a calculus very like algebra. This was Leibniz's and Boole's discovery. Against those who consider mathematical logic as a science in itself, Rignano's criticisms are quite just, but not against those who consider it as an instrument for solving mathematical problems which resist the ordinary methods. It seems to the reviewer that this is rather an unnecessarily restricted view to take of mathematical logic, and that the proper reply to Rignano is that, until comparatively lately, symbolism in mathematics and logic had simply the end of helping reasoning by brevity or analogy or by showing the mechanical character of logical deductions (of course this has no psychological implications), but that a great part of what modern mathematical logic does is to increase our subtlety by emphasising *differences* in reasoning instead of *analogies*.

Prof. E. B. Van Vleck (*Bull. Amer. Math. Soc.* 1915, **21**, 321) gives a most interesting address summarising the part played by the theory of sets of points in geometry and the closely connected subject of dynamical trajectories. Up to

the present time the applications of the theory of aggregates to physics have been but few, though they are of great promise. It is curious to notice that our conception, formed after Dedekind and Cantor, of the continuum, is parallel to the recent progress of physics, which tends more and more to become a theory of discrete molecules, atoms, and electrons. " Even the existence of the matrix or ether in which these are imbedded has become problematical and open to suspicion." Beginning with the paper of 1902 in which Hilbert laid the foundation for geometrical motion, Van Vleck goes on to mention the application of ideas in the theory of sets made by Hadamard in 1898 to geodesics upon surfaces of negative curvature—almost the only application of the theory to differential geometry—the work of Bohl (1909) and Bernstein (1911) on the existence of a " mean motion " in the theory of secular variations, Borel's treatment of the theory of probability, and some work of Poincaré, Birkhoff, Brouwers, and others. It is remarkable that Peano's space-filling curve, which is usually considered to be a refinement of exclusively pure mathematics, actually has a practical bearing on some physical work of Boltzmann on the theory of gases and statistical mechanics.

Robert L. Moore (*Trans. Amer. Math. Soc.* 1915, **16**, 27) shows that any plane satisfying the axioms given by Veblen in 1904 contains a system of continuous curves such that, with reference to these curves regarded as straight lines, the plane is an ordinary Euclidean plane, or, in other words, is a " number-plane." Moore also (*Annals of Math.* 1915, **16**, 123) gives a set of eight postulates for the linear continuum in terms of *point* and *limit*. This set has a certain similarity with the set proposed by F. Riesz in 1908 ; and postulates for the linear continuum in terms of *point* and *order* have been given by Veblen in 1905 and by Huntington in the same year.

Arithmetic and Theory of Numbers.—The tercentenary of the publication of Napier's *Descriptio* of 1614 called forth a number of accounts of Napier's invention of logarithms. The earliest seems to be by Prof. G. A. Gibson (*Proc. Roy. Phil. Soc. Glasgow*, 1914, **3**), whose paper is reprinted in the *Napier Tercentenary Handbook* reviewed elsewhere in the present number, while the lecture by Prof. E. W. Hobson is also reviewed elsewhere in this number. Whereas Gibson gave a very full account of Napier's life, his relations with Briggs, and the

Descriptio, he did not do very much more than mention the *Constructio*, which was written long before the *Descriptio*, though it was only published posthumously in 1619. However, it seems that the *Constructio* gave an account from which the mode of genesis of Napier's great invention may be gathered, whereas the kinematical and almost fluxional considerations in the *Descriptio* seem to be of origin later than the invention of the logarithms. For this reason, the long paper by Prof. H. S. Carslaw (*Math. Gaz.* 1915, **8**, 77, 115), which gives, in modern notation, Napier's method as explained in his *Constructio*, is very welcome. An account of the contents of the *Memorial Volume* published to commemorate the Napier Tercentenary is given by Dr. C. G. Knott in the number of this Quarterly for October 1915 (**10**, 189).

W. D. MacMillan (*Proc. Nat. Acad. Sci.* Washington, D.C., 1915, **1**, No. 7) gives some theorems connected with irrational numbers, as such numbers play an important part in the convergence of some series occurring in celestial mechanics.

The number $d(N)$ of divisors of N varies with extreme irregularity as N tends to infinity, and a theorem of Dirichlet on the sum $d(1) + d(2) + d(3) + \ldots + d(N)$ has aroused a good deal of interest in recent years with Voronöi, Landau, and Hardy. S. Ramanujan (*Proc. Lond. Math. Soc.* 1915, **14**, 347), in a paper on highly composite numbers, proves a large number of results which add a good deal to our knowledge of the behaviour of $d(N)$. The same author (*Journ. Indian Math. Soc.* 1915, **7**, 131) gives an investigation on the best possible upper limit for $d(N)$, using only purely elementary reasoning.

G. Humbert (*Compt. Rend.* 1915, August 30) writes on the reduction of Hermite forms in an imaginary quadratic corpus.

During 1915 papers have been read to the Paris Academy of Science by G. Mittag-Leffler on a new theorem in the theory of Dirichlet's series, and by G. H. Hardy on the problem of divisors of Dirichlet. In the same year, G. H. Hardy and Dr. Marcel Riesz published a small work on the general theory of Dirichlet's series, which is reviewed elsewhere in the present number, and which is so up-to-date that it includes even a reference to the above paper by Mittag-Leffler (see pp. 8, 75).

Analysis.—Arnaud Denjoy, in a paper read to the Paris Academy in 1915, has studied the theory of the four derivatives of a function—a theory which, principally owing to the

work of Dini and Scheefer, occupies a very important place in the theory of functions of a real variable. Maurice Fréchet (*Compt. Rend.* 1915, June 28) gives an extension of the definition of an integral for any aggregate of numbers.

Paul Appell (*ibid.* Sept. 27) investigates a second form of theta functions of the fourth degree. W. B. Ford (*Proc. Nat. Acad. Sci.* 1915, **1**, No. 7) represents arbitrary functions as limits of definite integrals depending on a parameter when the parameter becomes infinite, or by a series of definite integrals.

In 1915 F. R. Moulton presented to the same Academy a paper on the solution of an infinite system of differential equations of the analytic type. It is interesting to notice in this connection that, under certain conditions, there is a rigorous solution of the problem of infinitely many bodies. To the same Academy, H. Blumberg presented a paper in which he applied the methods of what Prof. E. H. Moore called "general analysis" in the strikingly original book he published some years ago, to giving a uniform theory for factorisation of various types of expressions.

On the subject of differential equations, D. Pompieu (*Compt. Rend.* 1915, August 30) gives a double solution of Riccati's equation, and Prof. T. Levi-Civita (*Atti del R. Instituto Veneto*, 1915, **74**, 907) discusses the reduction of the differential equations of the problem of three bodies.

There is, of course, an infinity of functions whose values at a finite number of points are the same as those of a given function, but Prof. E. T. Whittaker (*Proc. Roy. Soc. Edinb.* 1915, **35**, 181) shows that a certain one of these functions is represented by a well-known expansion in the theory of interpolation, and that it can be constructed when any one function of the above infinity is known. The function thus representative of the set of functions mentioned is called the "cardinal function," and the end of Whittaker's paper contains a suggestion for the use of functions analogous to this cardinal function to simplify the theory of functions of a real variable.

Geometry.—The second volume of the *Opera Matematiche di Luigi Cremona* was published at Milan in 1915 and contains principally various papers of a historical nature on modern geometry, the prolegomena to a geometrical theory of surfaces written in 1866, and sundry papers on *gauche* curves and surfaces.

H. S. White (*Proc. Nat. Acad. Sci.* Washington, D.C., 1915, **1**, No. 8) shows that if seven points on a twisted cubic curve be joined, two and two, by twenty-one lines, then any seven planes that contain these lines will osculate a second cubic curve.

The general form of the pseudo-sphere has not yet been obtained, and there is therefore some interest in the determination of simple particular cases. Dr. J. R. Wilton (*Proc. Lond. Math. Soc.* 1915, **14**, 339) writes on a pseudo-sphere whose equation is expressible in terms of elliptic functions.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

Dynamical Astronomy—Memoirs of the R.A.S., vol. lx. pt. vi., contains an important paper by H. Jeffreys, in which are discussed various hypotheses as to the internal structure of the Earth and Moon. Some knowledge as to the internal density and rigidity of the Earth can be derived from (i) the value of the precessional constant, (ii) the Earth's superficial ellipticity, (iii) the period of latitude variation, (iv) the velocity of earthquake waves, and (v) the lunar deflection of gravity. Using the data which are provided in this way, and supposing the Earth to consist of a homogeneous metallic core surrounded by a rocky crust, it is found to be highly improbable that either the shell or the crust can be permanently elastic, and that owing to its plasticity, the Earth must have at present the hydrostatic form to a high degree of accuracy. The only possible distribution of density is then that originally given by Wiechert, there being a homogeneous core of density 8·2, and with a radius 0·78 of the outer radius, and a homogeneous crust of density 3·2. The large ellipticities of the Moon are then discussed, and are explained on the supposition that the Moon solidified when much nearer the Earth than at present, and when its period of rotation was only about six of our present days. It is also found that the fact that the Moon turns the same face always towards the Earth is not necessarily explicable as an effect of tidal friction, as is generally thought: if the Moon's libration at the time of solidification was small, it will have remained small whether or not tidal friction has been operative; if, however, the Moon possessed a considerable libration when it solidified, tidal friction must have since operated to reduce this to zero.

It is well known that when a comparison of the observations of the Moon is made with theory, there are certain deviations in longitude which remain unaccounted for. Prof. E. W. Brown in an address to the British Association in 1914 (*Brit. Assoc. Report*, 1914) pointed out the similarity between the deviations of the Moon and those of the Sun and Mercury. H. Glauert, in two papers entitled "The Rotation of the Earth" (*M.N., R.A.S.* vol. lxxv. pp. 489 and 685), discusses whether such deviations are explicable by a small change in the rate of rotation of the Earth. Since such a change would cause a corresponding change in the unit of time, it would make itself apparent as an error in position of any body moving fast enough to show the effect. The result of the discussion is that the errors in longitude of the Sun, Moon, Venus, and Mercury can be accounted for by a slight and rather irregular change in the Earth's rate of rotation, the increase in the length of the day being about 0·01 seconds in a third of a century: the corresponding change of momentum may be entirely or partially compensated by a change in the Moon's mean motion. Sir Joseph Larmor (*M.N., R.A.S.* vol. lxxv., p. 211) has examined the possible physical causes of such an irregularity in the Earth's rotation. He concludes that the phenomenon is of an order of magnitude which is not inconsistent with its being due, in large measure, to such terrestrial movements of matter as the melting of water off an elevated antarctic ice-cap or to a sudden or gradual local rise or fall of part of the ocean bottom.

Stellar Motions.—During the past few months several important papers have been published dealing with this subject. J. C. Kapteyn and W. S. Adams (*National Acad. of Sciences* (Washington) *Proceedings*, vol. i. p. 14) have discussed the relationship between the radial velocity of a star and its proper-motion. They find that for all stellar types, the average radial velocity increased very considerably with increase of proper-motion. Since, on the average, large proper-motions indicate nearness and low luminosity, whilst small proper-motions indicate remoteness and great luminosity, this result may mean that the average velocity of the stars decreases with increasing distance from us, or that it decreases with increasing luminosity, or that there is a correlation between the different components of motion of a star. This result

was also derived in the course of a determination of the systematic motions of the stars from their radial velocities by A. S. Eddington and W. E. Hartley (*M.N., R.A.S.* vol. lxxv. p. 521). In this paper also, the position of the vertex of the preferential motion of the stars was found to be R.A. 94°·6, Dec. + 12°·5, in very good agreement with the position obtained from the study of proper-motions. The relation between the radial velocity and *apparent* brightness of stars has been discussed by C. D. Perrine (*Astroph. Journ.* vol. xli. pp. 315, 395). He finds that, in general, there is a small but regular increase in speed with decreasing apparent brightness, which holds both for galactic and for non-galactic stars. This result, taken in connection with the preceding, indicates that part at least of the progression found in the above investigations must be due to one or both of the first two mentioned causes, and a little consideration shows that the only possible explanation is that the average speed of the stars increases with decreasing absolute luminosity. The same conclusion is found by W. S. Adams (*Astroph. Journ.* vol. xlii. p. 172) from a discussion of the radial velocities of 500 stars determined at Mt. Wilson Observatory; he concludes also that the magnitude of the effect is of greater range than the progression of speed with spectral type. The bearing of these results on the general question of stellar evolution has been discussed by A. S. Eddington (*Observatory*, vol. xxxviii. p. 392) who suggests that there is a correlation between the *speed* and *mass* of a star, from which both the progression of speed with type and also with absolute luminosity arises.

Stellar Evolution.—H. Shapley (*National Acad. of Sciences* (Washington) *Proceedings*, vol. i. p. 459, and *Astroph. Journ.* vol. xlii. p. 271) discusses the densities and dimensions of five eclipsing binary systems of late spectral type, and finds for the upper limit of the mean densities in the five cases the respective values 0·02, 0·0005, 0·0005, 0·00012 and 0·00005, the density of the Sun being the unit, and that the dimensions of the systems are very large compared with those of the Sun. This proof of the existence of stars of types G and K of very small density is of great importance from its bearing upon the question of the order of stellar evolution. The formerly accepted view was that, starting from nebulæ, stars gradually passed in succession through the spectral types B, A, F, G, K, M,

i.e. they passed from blue to red as they condensed. The alternative theory, developed by Hertzsprung and Russell, which is gradually gaining acceptance, supposes that in the early stages of a star's evolution, it is very diffuse, and has a spectrum of the red type; that, as the star condenses, it becomes brighter and bluer up to a certain limit, after which it commences to decrease in brightness, and again to become redder. The turning-point depends upon the star's mass. The existence of red stars of very great absolute luminosity, and the non-existence of blue stars of very small absolute luminosity was an important point in favour of this theory. The result of Shapley seems conclusive in its favour, for if the red stars could come only at a late stage in the evolution, their densities must necessarily be very considerable. Further systems are to be examined to strengthen the evidence.

Determination of Large Proper-motions.—The stereo-comparator with its blink microscope has been applied by R. T. A. Innes (*Circular No. 25 of the Union Observatory of S. Africa*) for the purpose of discovering proper-motions. For many purposes in astronomy it is desirable to know all the large proper-motions in a certain region of the sky. The advantage of the blink method is that it automatically separates the stars with large motion from those with small motion. With other methods this is not possible, and much unnecessary labour is involved in seeking for the large motions. The case is clearly put by Innes himself: "This blink method of determining proper-motions appears to be superior to all others in its simplicity, certainty, and inclusiveness. The meridian-circle method, with many costly observations and many years' interval, will yield a few proper-motions of the brighter stars; the great 'Carte-du-Ciel' Catalogue, when it is duplicated in the far distant future, will by prodigious calculations extend the list to perhaps the 12th-magnitude stars. But the blink method only requires plates separated by moderate intervals of time, and its work at once goes to the very limit of photographic magnitude. It ignores what neither the meridian instrument nor the 'Carte-du-Ciel' ignores, namely, the stars of no proper-motion; it rejects the chaff automatically. It will give in a few years a knowledge of the proper-motions of faint stars such as without its help seemed to be for ever beyond our reach." The method consists in viewing together

two plates taken at an interval of about twenty years, and rapidly cutting them alternately out of sight. When the plates are adjusted so that the background of stars appears at rest, the large proper-motion stars are evidenced by their motion. It is to be hoped that this method will become more widely used.

PHYSICS. By James Rice, M.A., University, Liverpool.

A very notable contribution to the theory of metallic conduction has been recently made by Sir J. J. Thomson. The paper is published in the *Proc. Phys. Soc. London* for August, and also in the *Phil. Mag.* for July.

It is now generally accepted that the conductivity of a substance both for electricity and heat is dependent on the properties of the electrons in it; but in order to relate the conductivity to these properties, some assumption must be made concerning the motion of the electrons. The assumption which has had the greatest vogue hitherto is originally due to Drude and J. J. Thomson, and is known as the "free electron" hypothesis. Intermolecular space in a metal is supposed to contain electrons which have escaped from molecules and are free for a time sufficient to allow them to acquire on the average the energy possessed by gas molecules on the average at the same temperature, an energy which is in fact proportional to that temperature on the absolute scale. The electrons are assumed to have their motions distributed about the average according to the usual Maxwell law which holds in the kinetic theory of gases, and, on account of their comparatively large mass and volume, the molecules play much the same rôle as the walls of the containing vessel in gas theory.

This electronic motion produces no motion of electrons as a whole in one direction in a metal unless a potential difference is established between two surfaces in it. In that case the electrons have combined with their ordinary free path motion between collisions a drift in the direction of the electric force exerted on them, which, as they are negatively charged, is opposite to the electric field, and this drift constitutes the electric current. The hypothesis leads directly to Ohm's Law, and determines the conductivity in terms of the charge, mass, density, and free path of the electrons and the temperature.

The well-known fact that good conductors of electricity are good conductors of heat also receives an explanation ; for just as gas molecules tend to diffuse from a region where their temperature, *i.e.* their kinetic energy, is high to a region where it is low and thus transmit heat, so the free electrons at the heated end of a metal rod tend to diffuse to the cool end. This does not imply a motion of electrons as a whole ; equal numbers of electrons diffuse in either direction, but those coming from the warm end have more energy than those from the cool end, and gradually impart some of their increment of energy to the molecules at the cool end, so that we have transport of heat (without transport of electricity) in one direction. Metals being rich in free electrons ought therefore to be good conductors of heat. Further, the well-known Weidemann-Franz law concerning the ratio of thermal to electric conductivity can be deduced. This ratio should be a constant for all metals and proportional to the absolute temperature. Now there is in fact for many metals a close approximation to such constancy and dependence on the temperature on the part of the ratio referred to.

This hypothesis, however, plausible as it seems, is not free from serious difficulties ; one of the most important concerns the relation between the electric conductivity and the temperature. In pure metals the conductivity varies almost inversely as the absolute temperature at ordinary temperatures ; in alloys there seems to be no simple relation ; yet the above theory leads to an inverse proportion between the conductivity and the square root of the temperature, unless further postulates are introduced concerning change of molecular structure with temperature. There are other objections which cannot be dealt with here. Suffice it to say that of late years the theory has received some very adverse criticism, and as far back as 1900 Thomson advanced an alternative hypothesis which can be found developed in his *Corpuscular Theory of Matter*. Indeed, indications of the hypothesis may be found in his earlier work, *Applications of Dynamics to Physics and Chemistry*. However, this hypothesis is not as widely known as the " free electron " theory, but recent work, it would appear, is likely to bring the former into considerable prominence.

Kammerlingh Onnes has recently made some remarkable investigations on the resistance of metals at the temperature

of liquid helium, and shown that some metals can exist in a state where their specific resistance is less than one hundred thousand millionth part of that at 0° C. Thus when a current was induced in a small ring of lead at 4° absolute, by bringing a magnet close to it, the current, instead of dying away when the motion of the magnet ceased, went on with practically undiminished intensity, and its rate of decay was so slow that Onnes estimated it would take four days to fall to half its initial value. The transition from the state in which the resistance is diminishing normally with the temperature to the condition in which a metal possesses this remarkable superconductivity is abrupt and takes place at a definite temperature, and the difference in electric properties above and below this temperature are as marked as the difference in elastic properties between the solid and fused state, or in magnetic properties above and below the temperature of recalescence. This superconductivity is another, and apparently fatal, objection to the free electron hypothesis, and in the paper referred to above Thomson puts forward his own special hypothesis once more, and shows how the effects discovered by Onnes are in accordance with it.

No permissible increase in the number of free electrons or in the mean free path is of any avail to explain superconductivity at low temperatures, but Thomson's hypothesis is one of electrically polarised atoms and has some points of resemblance with the usual theory of residual magnetisation in magnetism. According to it the atoms of some substances, including the metals, contain electric doublets, *i.e.* pairs of equal and opposite charges at a small distance apart. Under ordinary circumstances the axes of the doublets in a body are uniformly distributed in all directions, but under the influence of an electric force they tend to set themselves parallel to this force, just as a magnet tends to set its axis parallel to a magnetic force. However, this tendency is opposed by various causes, and so a compromise is effected between the condition of uniform distribution and the extreme condition in which the axes would all point one way. Foremost among these opposing causes is the heat motion of the molecules, which, in the case of gases, gives rise to collisions between the molecules, thus tending to knock the axes of the doublets out of line as fast as they are brought into it by the electric force. In the case of solids and

liquids the opposition to unidirectional pointing is to be sought in the gyroscopic resistance of the rapidly rotating molecules to change of direction, which thus endows them with a quasi-rigidity, making each molecule behave very much as if its axis of rotation were acted on by a restoring couple proportional to the angle through which it is displaced and proportional also to the kinetic energy of rotation. This sort of rigidity is well exemplified in the well-known experiment in which a smart blow sets quivering the inner ring of a gyroscope whose flywheel is turning rapidly.

The polarisation of the substance thus has two extremes; it is zero when the axes of the doublets are uniformly distributed; it has a maximum value when they all point in one direction. The greater the electric force the more highly polarised is the medium for a definite energy of rotation in the molecules, *i.e.* for a definite temperature; for a definite electric force the less polarised is the medium the greater the energy of rotation of the molecules, *i.e.* the higher the temperature. Thomson develops a formula connecting polarisation with electric force and molecular rotational energy. He expresses it by a graph in which polarisation is the ordinate and the ratio of electric force to the mean energy of the molecule is the abscissa. The graph rises from the origin asymptotically to the limiting value of the polarisation, and is practically straight in its initial sloping part, and then begins to bend over towards the axis of abscissæ, ultimately merging into a line parallel to this axis.

For a finite value of the polarisation we will have a number of chains of polarised molecules all pointing in the direction of the electric force, the remaining molecules being uniformly distributed. Now if we consider two adjacent molecules in a chain, presenting oppositely charged ends to one another, there will be a considerable attraction due to the positive end of one molecule, on the electrons in the negative end of the other, and Thomson's essential hypothesis at this point of the argument is that in the molecules of metals, in contradistinction to the molecules of insulators, there exist electrons (not probably those of the doublets) easily abstracted if the molecules are crowded together. So we suppose that under these "end" forces, electrons pass along a chain of molecules "like a company in single file passing over a series of stepping-stones."

On such a hypothesis the chief function of the impressed electric force is to polarise the medium, *i.e.* dispose a number of molecules into chains, while the actual current is largely due to the forces developed by the charged ends of the molecules themselves. Indeed (and this is the bearing of the hypothesis on Kammerlingh Onnes' work), if the temperature is sufficiently low, and thus the ratio of the electric force to rotational energy very large, the molecules once polarised will remain polarised under their own mutual end attractions, the heat motion being too feeble to knock them out of line. The original impressed electric force may be removed without disturbing the polarisation, and the dragging of electrons round the chains formed in the closed conductor will go on as before until the feeble heat motion slowly "depolarises" the medium. Thomson shows how his formula and curve lead to a definite temperature below which this state of affairs exists and above which it cannot exist. All the usual results connecting thermal and electric conductivity with temperature are obtained, and a further discussion shows how the properties of alloys as distinct from pure metals can be accounted for.

The older free electron theory has had such vogue in the lecture room, and has of late suffered such adverse criticism, that it would be well if every teacher of Physics read this very important paper, so that what is undoubtedly a very promising hypothesis may become much more widely known than it seems to be at present.

INORGANIC CHEMISTRY. By C. SCOTT GARRETT, B.Sc., University, Liverpool.

PROGRESS in inorganic chemistry has not been marked by any sweeping generalisations or revolutionary advances such as have been taking place recently in the sister science of physics. Nevertheless a steady advance along detailed lines is going on, and observations and data are being accumulated, which some day will enable a master mind to shed brilliant new light on the dark places of the science.

Some observations on the reaction between chlorine and sodium thiosulphate have been made by Self (*Pharm. J.* 1915, **95**, 133), and as they are of considerable importance at the present juncture, it may be of general interest to mention

them here. In order that the solution of thiosulphate may remain alkaline until the whole of the thiosulphate has been decomposed by chlorine, it is necessary to have present $Na_2CO_3 . 10H_2O$ in slight excess. A satisfactory formula for a respirator is 5 : 6 : 10 of thiosulphate, crystallised carbonate, and water respectively. Self points out that with the formula of the Paris Academy of Medicine, the solution becomes acid when only 35 per cent. of the thiosulphate has been decomposed.

Catalysts.—The search for substances which will act as specific catalysts is being prosecuted with increasing vigour, as the need for more economical processes in industrial work becomes greater. To this end, more and more attention is being directed to the rarer inorganic substances, such as the metals and compounds of the rare-earth and platinum groups. The Badische Anilin und Soda Fabrik have lately patented (D.R.P. 275518) the use of ruthenium, its oxides and salts to catalyse certain oxidation processes. With asbestos and potassium ruthenate, methyl alcohol in presence of air readily forms formaldehyde at 120° C., whilst a red heat is necessary in the cases of platinum, palladium, and iridium. With hydrocarbons, where air is unsuitable, ordinary oxidising agents such as chlorate can be utilised successfully when a trace of a ruthenium salt is present.

In the classical researches on ammonia, descriptions of which are now appearing (*Z. Electrochem.* 1915, **21**, 191), Haber and his co-workers have made very frequent use of catalysts, and have investigated their actions in a systematic manner. Iron and osmium are specific catalysts for the decomposition of ammonia, whilst uranium acts as a synthetic catalyst. Uranium carbide was used, but the active agent appears to be Ur_3N_4 which is formed gradually, best of all from the carbide, although other derivatives give the effect. High pressure and a temperature about 500° C. are the other requisite conditions for the optimum reaction of the mixed nitrogen and hydrogen gases.

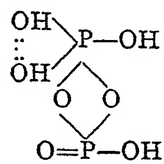
Analysis.—The well-known brown ring qualitative test for nitrates is liable to be vitiated when halide salts, more especially bromide and iodides, are present. Consequently the new test devised by Tingle (*J. Soc. Chem. Indust.* 1915, **34**, 393) of the same order of delicacy, but which is reliable in

the presence of halides, is of importance. The test depends on the production of a yellow or orange colour on rendering alkaline the nitro-salicylic acids formed by a nitrate and salicylic acid in presence of concentrated sulphuric acid. The reagent consists of a 7 per cent. sulphuric acid solution of salicylic acid, and a few drops of this are warmed with a small amount of the solution to be tested. On cooling and making alkaline, the colour will be produced. Another colorimetric test which may prove very useful in analysis has been discovered by Sacher (*Chem. Ztg.* 1915, **39**, 319). One part of manganese in 200,000 parts of solution may readily be detected, if the $Mn(OH)_2$ precipitated by alkali is allowed to oxidise partially in the air and then treated with drops of a N/2 oxalic acid solution. A sharp red colour results, which is stated as being due to the formation of a double salt. Presence of reducing agents, tannic acid, excess of oxalic acid, and high temperatures must be avoided, but as such conditions are required in the separation of manganese by the ordinary table, the test is valid to detect traces of the metal.

A further improvement in the method of analysis in Group III B has been effected by Atack's test for cobalt with the sodium salt of α-nitroso-β-naphthol (*J. Soc. Chem. Indust.* 1915, **34**, 641). In the presence of dilute sulphuric acid this reagent is capable of detecting 0·001 mg. Co per c.c. by the production of an orange-red coloration. The test can be applied in the presence of nickel, ferric, manganese, and zinc salts, and tartaric and citric acids, although it is first necessary to remove any large amounts of nickel which may be present.

A modified method of testing for strontium and calcium in Group IV has been suggested by Gilmour (*Chem. News*, 1915, **111**, 217), which surmounts the difficulty of having to separate strontium before the oxalate test for calcium can be applied. Barium is separated in the usual way by potassium chromate precipitation. Strontium and calcium are then reprecipitated as carbonates, dissolved in acetic acid and neutralised with ammonia. One portion of this solution is tested for strontium by means of calcium sulphate solution, the other for calcium by adding a saturated solution of potassium ferrocyanide, which gives a precipitate of calcium potassium ferrocyanide, whilst strontium gives no such precipitate.

Constitution.—From a consideration of the conditions of dehydration, of the reactions of $P_2O_3Cl_4$, of ethyl pyrophosphate, and of $NaAg_3P_2O_7$, as well as the reaction between PCl_5 and $Na_4P_2O_7$, Balareff (*Zeit. anorg. Chem.* 1914, **88**, 133) comes to the conclusion that pyrophosphoric acid must have a constitution represented by the following unsymmetrical formula :



The first two hydrogen ions, moreover, have practically identical ionisation constants, whilst the constants of the third and fourth are widely different from these, but near to one another. The unsymmetrical formula would account for these differences. Further, Rosenheim and Triataphyllides (*Ber.* 1915, **48**, 582), show that metallo-derivatives of this acid can be prepared of the general formula $R(R'P_2O_7)$, where R is a monovalent alkali metal (K, Na, Am) and R′ a trivalent metal (Mn, Cr, Mo), in which the grouping $R'P_2O_7$ acts as an acidic complex and does not give the ordinary reactions of the metal R′.

These complex metallo-derivatives are always polyhydrates.

Generally speaking, noticeably more attention is being paid to the structural arrangement of the water molecules in hydrates, and when a definite colour is associated with the hydrated state, one of the best methods of investigating the problem is to be found in measurements of absorption spectra. In some cases marked differences in the ease of dehydration of one or more molecules of the hydrate water is being taken as a criterion of a different structural position. $MgSO_4 . 7H_2O$, for example, loses one molecule of water on long desiccation over calcium chloride, and Johnsen (*Centr. Min.* 1915, 289), consequently, represents the constitution by the formula $[Mg(H_2O)_6] . SO_4,H_2O$ with the possibility of the six molecules being polymerised into $(H_4O_2)_3$ or $(H_6O_3)_2$. The problem of hydrates must, however, be taken in conjunction with general complex radicle formation, in which the water molecules

become substituted by other specific basic, acidic, or neutral radicles. The extended researches of Ephraim and his co-workers at present being carried out on the nature of subsidiary valencies, may be expected to advance our knowledge on this important subject. With regard to water itself further data on its polymerised state has been published by Oddo (*Gazzetta*, 1915, **45**, i. 319). Below 32° C. water vapour appears to be ionically dissociated, at 32° the vapour density indicates the mono-molecular formula, whilst above this temperature association sets in, and at 270°, over 40 per cent. is in the dimolecular form, H_4O_2.

Preparative.—Zinc has lately been definitely added to the list of metals forming peroxides. The new peroxide is obtained by adding hydrogen peroxide to ammoniacal zinc nitrate at − 5° C., and stirring the white precipitated solid with concentrated hydrogen peroxide solution. Some valuable preparative work has been carried out by Sander (*Zeit. angew. Chem.* 1915, **28**, 273) on polythionates. Tetrathionates hitherto have never been prepared in a state of purity, and Sander has been able to show that the presence of thiosulphates is a prime factor in their decomposition. If, then, the tetrathionate as soon as it is formed can be removed from the presence of the parent thiosulphate, a pure product ought to be obtained. This has now been accomplished by adding the thiosulphate solution to a cooled alcoholic solution of iodine. The tetrathionate being insoluble in the alcoholic solution, is precipitated out as soon as it is formed, and is obtained in a pure state.

A new allotropic modification of lead is, according to Heller (*Zeit. Phys. Chem.* 1915, **89**, 761), obtained by leaving pure lead immersed for several days in a solution of lead acetate acidified with nitric acid. The metal becomes so brittle as to be capable of being powdered, and the modification is designated "grey lead." In the light of Cohen's extended researches on metallic allotropes, this discovery is not surprising, and in fact we must now look upon absence of allotropic modifications in a metal as something quite abnormal. Not only so, but ordinary samples of a metal probably contain varying proportions of different allotropes, a contingency, as Cohen points out, which is highly significant as to the value of the hitherto determined physical constants of metals.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., St. Mary's Hospital Medical School.

IN a recent number of the *Comptes rendus* (1915, **161**, 150) H. D. Dakin describes the preparation of a hypochlorite solution suitable for the treatment of wounds; it is prepared by slaking 200 grams of bleaching powder with a solution of 140 grams of sodium carbonate in 10 litres of water, syphoning off the supernatant solution after half an hour and filtering through cotton wool; boric acid is then added until the solution is just neutral or faintly acid to phenolphthalein. Such a solution can be employed for the irrigation of wounds for several days without much irritation of the skin, since it contains no free alkali or chloric and it has the advantage of dissolving necrosed tissue as well as being a powerful antiseptic. The author has also prepared and tested a number of organic chloro-amides with a view to their possible employment for the same purpose, and has found that the sodium salts of benzene and toluene sulphonyl chloro-amides $C_6H_5SO_2NClNa$ and $CH_3C_6H_4SO_2NClNa$ are strong antiseptics, and being only slightly toxic they can be employed in stronger solution than hypochlorites, but they exert a powerful hæmolytic action and moreover do not dissolve necrosed tissue.

An interesting series of papers on the chemistry of the interaction between toxins and antitoxins is contributed to the *Journal of the Russian Physical and Chemical Society* (1915, **47**, 263, 301, 307 and 313) by I. I. Ostromisslenski. According to this author toxins are colloidal substances of high molecular weight and slightly basic character. Although toxic themselves, they form salts which are not poisonous, in which respect they differ from the alkaloids and other crystalline poisons whose toxic properties are not destroyed by salt formation.

Antitoxins are globulins whose specific properties are due solely to their physical condition such as the size, mass, surface or electrical condition of their colloidal particles. The interaction between a toxin and an antitoxin takes place in three stages: (1) the mutual physical adsorption of the two substances without loss of toxic properties, (2) the chemical neutralisation of the feebly basic toxin by the weak acid antitoxin with the formation of a non-toxic salt which can be decomposed by either acid or alkali, and (3) a further chemical change of an intramolecular nature or possibly brought about

by the surrounding medium resulting in the formation of a compound which cannot be resolved by acids or alkalies. A globulin such as is contained in horse-serum can, as a rule, be converted into an antitoxin by heating with a toxin in presence of sodium chloride to act as a catalyst. Thus if 3 c.c. of a toxin are heated with 3 c.c. of normal serum and 0·36 gram of sodium chloride for from three to thirty-six hours, the serum is converted into the antitoxin provided such a substance exists, and the product will be atoxic. To test this a quantity of the product considerably in excess of the lethal dose is injected into the aural vein of a rabbit or porpoise, and at the same time an equal amount of the toxin without serum is injected into another animal as a control experiment. If in the former case the animal survives while the other one dies, an antitoxin has been produced and immunisation of the animals may be effected, but if the animal dies there is no antitoxin for that particular toxin. In this way the author has been able to prove the formation of antitoxins for diphtheria, tetanus, and botulismus or meat poisoning, but not for the poisonous alkaloids strychnine and morphine.

The observation of Danysz that when a toxin is gradually added in portions at a time to an equivalent amount of antitoxin the former is not neutralised but remains poisonous, is explained by assuming that the toxin is able to adsorb physically more antitoxin than it can neutralise chemically. Thus for example, if m molecules of toxin adsorb $(n+p)$ molecules of antitoxin, a neutral substance $T_mA_n + pA$ is formed containing an excess of adsorbed antitoxin pA which however is unable to neutralise any further toxin, although the latter must of necessity be present, and the resulting system consequently remains toxic.

A good deal of confusion has existed hitherto with regard to the use of the term lecithin, and it is now shown by H. Maclean (*Biochem. J.*, 1915, **9**, 351) that the substance usually described as "lecithin" obtained by extracting tissues with alcohol is in reality a mixture of two phosphatides contaminated with a complex nitrogenous impurity which it is very difficult to remove, and contains substances of a purine nature; by the fractional crystallisation of the cadmium chloride compound of this "lecithin" from ether it can be separated into true lecithin of the formula

$$
\begin{array}{l}
CH_2\,.\,O\,.\,\text{Fatty acid radicle} \\
| \\
CH\,.\,O\,.\,\text{Fatty acid radicle} \\
|\qquad\quad O \\
|\qquad\quad \| \\
CH_2\,.\,O\,.\,P(OH)\,.\,OC_2H_4N(CH_3)_3OH
\end{array}
$$

and kephalin in which the choline complex of lecithin is replaced by aminoethyl alcohol $NH_2C_2H_4OH$.

In a paper entitled "Physicochemical Studies on the Synthesis of Chlorophyll," two authors, Albert and Alexandre Mary (*Moniteur scientifique*, 1915, [v], **5**, i. 121) propound the theory that chlorophyll is a product of the polymerisation of aniline. By adding nitrous acid drop by drop to a very concentrated alcoholic solution of aniline, a white deposit is formed which gradually becomes pink and finally green. At the same time a green or reddish brown deposit is left on the walls of the vessel. If this is extracted with benzene and the resulting fluorescent green solution is evaporated in the dark, minute dichroic crystals are obtained which, according to the authors, are synthetic chlorophyll, the substance resembling the natural product in solubilities, crystalline form, and spectrophotometric character, and also in giving Fremy's reaction. If when the reacting mixture becomes green a solution of sodium carbonate is rapidly added, the whole of the colouring matter can be extracted with benzene, but if the alkali is not added sufficiently rapidly, the colour changes to red or brown, similar to that of dead leaves. By reducing an acetic acid solution of chlorophyll obtained from *Hedera helix* with iron and subsequently removing the latter with potassium ferrocyanide, the authors obtained an amber-coloured solution of reduced chlorophyll which on treatment with nitrous acid was reoxidised, becoming green and finally reddish brown.

The author's views concerning the constitution of chlorophyll do not coincide with those of Willstätter. They do not regard magnesium as an essential constituent, and state that when a benzene solution of natural chlorophyll is repeatedly shaken with aqueous ammonia until no more magnesium hydroxide is precipitated, the spectrum of the benzene solution remains the same as it was before treatment.

No physical constants or analyses are given, and most of the evidence is based on colour reactions.

The question as to whether plants are able to assimilate free atmospheric nitrogen has been again revived in a paper by Mameli and Pollacci (*Atti R. Accad. Lincei,* 1915, [v], **24**, i. 966), who assert that their original contention (*loc. cit.* 1910, [v], **19**, i. 501, and 1911, [v], **20**, i. 680) concerning the ability of certain seeds to assimilate atmospheric nitrogen is correct. They further criticise and explain the contrary indications obtained by other authors such as Oes (*Zeitsch. Bot.* 1913, **5**, 145) and Molliard (*Comptes rend.* 1915, **160**, 310).

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

General and Stratigraphical Geology.—The origin of coral-reefs has been investigated anew in the field by Prof. W. M. Davis, who has applied to the problem those methods of modern physiography which have been largely perfected by himself. As a result of this study (*Amer. Jour. Sci.* 1915, **40**, 223) he gives the support of his great authority to Charles Darwin's original theory of subsidence. Prof. Davis points to the frequent embayment of the central islands and the unconformable contact of the reef-masses with their volcanic foundations as essential consequences of subsidence.

In consequence of field and petrographical study of the Carrara schists and marbles, Prof. T. G. Bonney and the Rev. W. H. Winwood (*Geol. Mag.* 1915 (6), **2**, 289) abandon the hypothesis of a Mesozoic, or even later Palæozoic age, for these rocks, and believe that they are most probably Archæan.

In an important paper on the correlation of the Dinantian of Belgium and the Avonian of south-west England (*Quart. Jour. Geol. Soc.* 1915, **71**, 1) Dr. A. Vaughan has set out the diagnostic and dominant faunas of the Belgian sequence, and has demonstrated a period of local emergence in Belgium during the time S_1, which corresponds with a period of renewed subsidence in Britain. Two "knoll" horizons are also shown to exist, just as in the Clitheroe-Craven area of Lancashire.

Mr. T. C. Nicholas has studied the geology of the little-known St. Tudwal's peninsula (Carnarvonshire) in great detail (*ibid.* 83). He demonstrates beyond reasonable doubt the Pre-Cambrian age of the strip of highly-altered schists, jaspers, and other rocks from Nevin to Bardsey Island, by the discovery of a great series of lithologically different and quite unaltered

Cambrian beds. These facts also confirm the widely accepted opinion of the Pre-Cambrian age of the "Mona complex" of Anglesey. There is a great unconformity at the base of the Arenig in this area, which supports Ramsay's original views as to the relation of the Arenig to the subjacent strata.

Glaciology.—A shelly clay, containing erratics solely of Scandinavian origin and with a fauna of Arctic affinities, has been discovered on the Durham coast by Mr. C. T. Trechmann (*Quart. Jour. Geol. Soc.* 1915, **71**, 53). This drift underlies the main Cheviot drift and appears to fill up hollows and fissures in the Pre-Glacial land surface. It records perhaps the earliest ice invasion of the English coast. In the discussion of the paper Mr. G. W. Lamplugh supported the correlation of this clay with the shelly Basement Boulder Clay of Yorkshire.

The complicated history of the moraines of the great ice-sheet in Indiana and Michigan, and the equally complicated and interrelated glacial history of the Great Lakes is the subject of an exhaustive work by F. Leverett and F. B. Taylor (*United States Geological Survey, Monograph* 53, 1915).

Petrology-Igneous Rocks.—A much-needed systematisation of the microscopical characters of volcanic tuffs is provided by Prof. L. V. Pirsson (*Amer. Jour. Sci.* 1915, **40**, 191). He distinguishes between vitric tuffs, crystal tuffs, and lithic tuffs, terms which explain themselves; but is careful to point out that there are all gradations between these groups. The weathering, consolidation, devitrification, and metamorphism of tuffs are also dealt with at length.

An important study of the crystallisation of the ternary system albite-anorthite-diopside has been carried out by N. L. Bowen from a petrological point of view (*Amer. Jour. Sci.* 1915, **40**, 161). These mixtures may be regarded as simple dioritic and basaltic magmas (haplodioritic, haplobasaltic). It is shown that there are no diorite or gabbro eutectics, and that the differentiation of such rocks is controlled entirely by crystallisation.

An exhaustive study of Hawaiian lavas, supported by numerous first-rate chemical analyses, has been made by Whitman Cross (*United States Geological Survey*, 1915, *Professional Paper*, No. 88). While many of the rocks are ordinary plagioclase-basalts, a decided alkaline cast is given to the assemblage by the presence of soda-trachytes, trachydolerites, with melilite-

and nepheline-basalts. Dr. Cross further destructively criticises the unhappy "Atlantic-Pacific" classification of igneous rocks, and the memoir contains much excellent petrological discussion of a general character.

A rare igneous type, bekinkinite, hitherto only known from the type-locality of Bekinkina, Madagascar, has been discovered at Barshaw, near Paisley, by officers of the Geological Survey of Scotland. A further discussion of the occurrence is given by the writer (*Geol. Mag.* 1915 (6), **2**, 304), who also records the presence of another rare type, lugarite, as veins in the predominant bekinkinite. Both rocks are rich in nepheline and analcite.

Petrology-Sedimentary Rocks.—An investigation of the chemical composition of the skeletons of present-day crinoids and echinoderms by F. W. Clarke and W. C. Wheeler (*United States Geological Survey*, 1915, *Professional Papers*, 90-D and 90–L) has disclosed the novel and important facts that the hard parts of these animals are comparatively rich in magnesium carbonate (6–14 per cent.); and that the proportion of this constituent varies with the temperature of the water in which the organisms thrive. On the other hand fossil crinoids are much poorer in magnesium carbonate (average 1½ per cent.). With further work it is probable that these facts will be found to have an important bearing on the origin of magnesian and other limestones. W. H. Twenhofel (*Amer. Jour. Sci.* 1915, 40, 272) describes a black shale at present forming in swamp waters on the Esthonian coast of the Baltic Sea. From this occurrence he concludes that black hydrocarbonaceous shale may form in water so shallow that it is but a step to land conditions. A review of theories of black shale formation leads him to the conclusion that marine and non-marine types may be differentiated and that this kind of deposit is by no means generally indicative of deep-water conditions.

In an account of geological exploration along the Canadian Pacific Railway from Golden to Kamloops, British Columbia (*Geological Survey of Canada*, 1915, Mem. 68), Prof. R. A. Daly describes interesting dune-sand quartzites and quartzite of loess-like character in the Beltian System (Pre-Cambrian) of that part of the North American Cordillera. The former consist of minute, perfectly rounded quartz grains; the latter are distinguished by their very fine-grained, homogeneous, thick-

bedded character, and by having a composition like that of a siliceous mud, similar to present-day loess.

Economic Geology.—As the greatest corundum deposits of the world occur in Ontario it is appropriate that an exhaustive treatment of the occurrence, origin, distribution, and economic uses of this mineral should appear in a Memoir (No. 57, by A. E. Barlow) of the *Canadian Geological Survey*. The corundum occurs as an original pyrogenetic constituent of certain nepheline-syenites and anorthosites belonging to the Archæan of the Canadian Shield.

BOTANY. By F. Cavers, D.Sc., A.R.C.Sc.

Plant Physiology.—A large amount of attention has recently been given by plant physiologists to antagonistic ion action and some related phenomena, and the results already obtained mark a great advance in the knowledge of the complex relations involved in the exchange of substances between the exterior and the interior of the cell. The modern conception of antagonism—that is, the mutual hindrance between salts, or more properly between ions, in their absorption in solution by cells—dates from Loeb's discovery in 1901 that certain animal eggs cannot form an embryo if placed immediately after fertilisation into a solution of pure NaCl of the same concentration as that of this salt in sea-water, but that the toxicity is reduced if a small quantity of the salt of a bivalent metal is added to the NaCl solution. Further work on both plants and animals has shown that antagonism is a phenomenon of widespread if not of universal occurrence in organic life, that it is with very few exceptions limited to kations, and that the effect is greatest between ions of different valency. Following up their useful resumé of the literature, published last year (*New Phyt.* **13**), Stiles and Jorgensen have now described (*Ann. Bot.* **29**) the results of experiments on the exosmosis of electrolytes as a criterion of antagonism, in which they attacked the problem of the relations between plant tissue and a solution surrounding it by examining the changes in electrical conductivity of the latter. They conclude that within limits the rate of exosmosis is a measure of toxicity, and that a decrease they have observed in this rate when certain ions are added to solutions containing poisonous ions may be due to the same cause that produces what other investigators have called antagonism. That is, the

phenomena classed under the heading of antagonistic ion action are apparently much more complex than has hitherto been assumed, and it is necessary to apply exact physico-chemical methods to the analysis of the various cases. True and Bartlett (*Amer. Journ. Bot.* **2**) have investigated in detail the exchange of ions between lupin roots and culture solutions containing respectively one and two nutrient salts; the chief points brought out are that when a mixture of corresponding salts or two or three different metals is present, the roots usually absorb more than from pure solutions of equal concentration, that in pure solutions one metallic ion may be more favourable to absorption than another in weak concentration, while in stronger concentration the reverse holds good, and that absorption and growth are more or less independent phenomena. Osterhout (*Bot. Gaz.* **59**), in continuing his studies of permeability, has found that there is a remarkable difference between monovalent and bivalent kations in their effects on permeability, none of the former being able to decrease permeability, while all the bivalent kations investigated did so to a marked degree; and that protoplasm is able to withstand very great changes of permeability, an increase or decrease of 30 per cent. or more being possible without rendering a return to normal permeability possible.

Pentose sugars, such as xylose and arabinose, are of general occurrence, in the form of their polysaccharides, such as xylan and araban, in the cell-walls of plants, and numerous investigations have been made as to their utilisation by both animals and plants, though the question whether organisms secrete enzymes capable of splitting the complex pentosans into their constituent sugars has usually hitherto been answered in the negative. Hawkins (*Amer. Journ. Bot.* **2**) has studied the action of a fungus, *Glomerella cingulata*, on these substances, and claims to have obtained definite evidence that in this case at any rate such an enzyme is present. The fungus was found capable of utilising either glucose, xylose, arabinose, araban or xylan as a sole source of carbon, the three sugars being most efficiently utilised, xylose on the whole best, and it thrived better on xylan than on araban. A filtered extract of the fungus can act on xylan under aseptic conditions with the formation of alcohol-soluble substance which reduces Fehling, forms furfurol when boiled with HCl, and possesses other properties of xylose,

the breaking-down of the xylan taking place gradually and the furfurol-yielding material increasing the longer the extract is allowed to act. The author therefore considers that the fungus contains an enzyme, xylanase, which hydrolyses xylan to xylose.

In the first of a series of papers on the physiology of parasitism, Brown (*Ann. Bot.* **29**) describes experiments made with an extract of the fungus *Botrytis cinerea*, finding that the action of the extract on plant tissue is twofold—action on cell-wall leading to disintegration of tissue and action on protoplast causing death. The extract can be deactivated by heating, by mechanical agitation, and by neutralisation with alkali, but though the extract is acid, if any special lethal substance is present it must be of colloidal nature; the only active substance present is apparently an enzyme which causes maceration mainly by solution of the middle lamella of the walls, and that it is responsible for the lethal action is suggested by the fact that death of the cells occurs either by direct action of the enzyme on the protoplasmatic membrane or indirectly through the action on the cell-walls. As an expected corollary, it is found that the ability of certain tissues to resist the action of the fungus extract depends upon the special properties of the walls.

Morphology.—Burlinghame (*Bot. Gaz.* **60**) has followed up his detailed study of the structure and development of the conifer genus *Araucaria* by an elaborate discussion of the origin and relationships of the araucarians, including some well-timed remarks on the study of phylogeny in general and that of the gymnosperms in particular. He points out that the science of phylogeny has fairly adequate and reasonably trustworthy rules of evidence; that the degree of relationship is most clearly indicated by a detailed and accurate comparison of all the structures of the plant in all stages of development, and is roughly proportional to the number and exactness of the resemblances; that direct comparisons should be checked when possible by the geological record, and may be supplemented by the less reliable evidence afforded by the presence of presumably vestigial structures in primitive regions and by recapitulationary phenomena. As to the gymnosperms as a whole, he considers the resemblances among these to be so strong as to preclude the probability of their being polyphyletic, and that since the

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Cycadophyta are almost certainly derived from a fern ancestry it follows that all are ultimately traceable to the same source. The conifers closely resemble Cordaitales and are probably derived from them, but the Araucarineæ resemble Cordaitales far more closely than do any other conifers and have evidently sprung direct from that group. The transitional conifers of the Mesozoic are either araucarians or cordaiteans well on their way towards Pinaceæ, while the Abietineæ are very old, and are derived either directly from the Cordaitales or from the most ancient members of the Araucarineæ.

ZOOLOGY. By C. H. O'Donoghue, D.Sc., F.Z.S., University College, London.

It seems fitting to record here the great loss British zoology has sustained by the death of three of its best known workers and to add our tribute to their memory. Prof. E. A. Minchin, F.R.S., was justly regarded as one of the foremost Protozoologists in the world, and his contributions to this branch of knowledge will be much missed. The other two were mainly occupied with Embryology, Dr. R. Assheton, F.R.S., and J. W. Jenkinson. The latter met his death on the battlefield in Gallipoli and one of his papers was published in the period now under review.

Protozoa.—Dobell and Jameson have studied "The Chromosome Cycle in Coccidia and Gregarines" (*Proc. Roy. Soc.* B. 610, August 1915) using as their main types *Aggregata eberthi* and *Diplocystis schneideri*. They point out that the formation and arrangement of the chromosomes are much more constant than is generally supposed. Trypanosomes do not appear to divide at all readily in the rat, but Coles has shown in his note "Multiplication Forms of *Trypanosoma lewisi* in the Body of the Rat" (*Parasitology*, September 1915) that equal and unequal binary fission does occur in young rats. "Experiments in Trypsofrol Treatment of Trypanosomiasis (*T. brucei*) in Guinea Pigs and of Piroplasmosis in Dogs" are recorded by Nuttall and Hindle (*Parasitology*, September 1915).

Invertebrata.—Haswell has added to his previous work on the Platyhelminthes a description of a new form in "Studies on the Turbellaria III *Didymorchis*" (*Quart. Jour. Micro. Sci.* vol. lxi pt. 2). In the same journal Helt writes "On a New Species of Pentastomid from a N. African Snake (*Zamenis ravigieri*)."

"A New Cestode of the Genus *Zschokkeela*" (*Ann. and Mag. Nat. Hist.* **91** July 1915) and "A Trematode from Protopterus" (*ibid.* **92** August) are dealt with by Baylis. Hunt shows that *Enchytracus albidus* regenerates when cut off between the eighth segment and the posterior end (*American Naturalist*, August 1915). The regeneration takes place in salt or fresh water, and more readily posteriorly than anteriorly.

Clark makes "A Study of Asymmetry as Developed in Genera and Families of Recent Crinoids" (*American Naturalist*, September 1915).

Certain new molluscan species are recorded: "Descriptions of a New *Modiola* from Ceylon and of a New *Tellina* from New Caledonia" (*Ann. and Mag. Nat. Hist.* August 1915); and "Descriptions of New Species of Mollusca from Various Localities" by Sowerby (*Ann. and Mag. Nat. Hist.* September 1915).

The Crustacea are treated in three papers. Cunnington describes the Entomostraca of the Albert Nyanza (*Ann. and Mag. Nat. Hist.* July 1915). In the September number of the same journal Collinge adds "Some Observations on the Isopod *Idotea hectica*," and in "On the Species of *Lucifers* and their Distribution" Borradaile helps to clear up the muddle into which this genus had fallen.

As before, the Insecta are well represented. Cockerell adds two more notes to his already long series on "Descriptions and Records of Bees" (*Ann. and Mag. Nat. Hist.* July and August 1915). "Notes on the British *Machilidæ* with Descriptions of Two New Species" are contributed by Reilly (*Ann. and Mag. Nat. Hist.* July 1915). Various Coleoptera figure in "On New Species of *Histeridæ* and Notices of Others" by Lewis (*Ann. and Mag. Nat. Hist.* July), "The *Xylophilidæ* of Ceylon" by Champion and "Note on the Nomenclature of Certain Species of Ruteline Coleoptera" by Arrow (both in *Ann. and Mag. Nat. Hist.* September 1915). Australian forms are dealt with by: Distant in "On Some Australian *Cicadidæ*," and Recardo in "Notes on the *Tabanidæ* of the Australian Region" (both in *Ann. and Mag. Nat. Hist.* July 1915), Campion in "A New Agrionine Dragonfly from Northern Australia" (*Ann. and Mag. Nat. Hist.* August 1915). Additions to the known species of Lepidoptera are to be found in "New Species of Indo-Malayan Lepidoptera" by Swinhoe, and in "Descriptions of New Species of Lepidoptera from Africa and the East," by Bethune-Baker

(both in *Ann. and Mag. Nat. Hist.* September 1915). F. W. Edwards writes "On *Elporia,* a New Genus of Blepharocerid Flies from South Africa" (*Ann. and Mag. Nat. Hist.* September 1915). Prof. T. H. Morgan has already rendered the fly Drosophila a classical form for zoologists, and is continuing his studies thereon. He has recently dealt with "The Rôle of Environment in the Realisation of Sex-linked Mendelian Characters in Drosophila" (*American Naturalist,* July). He concludes that the amount of water in the food determines the realisation of abnormal types. However effected, it is possible for the experimenter to determine at will the nature of the flies by controlling the food supplies. The abdomen is sometimes abnormal, possibly as the result of injury, but the evidence shows that such a condition in the parent has no effect on the offspring. In the same fly Short—"A Note on the Gonads of Gynandromorphs of *Drosophila ampelophila*" —makes the interesting discovery that in lateral gynandromorphs the gonads do not follow the separation of the somatic cells into male and female, but on the same side are always either male or female.

Vertebrata.—The "Origin of Bilaterality in Vertebrates" has been investigated by Enclesheimer (*American Naturalist,* August). The eggs of a number of Anurous and Urodele Amphibia were employed, and it was found that the bilaterality reveals itself through the early cleavage grooves. Several theories of the homologies of the ear bones in higher vertebrates have been put forward, and Goodrich has attacked the question of "The Chorda Tympani and Middle Ear in Reptiles, Birds, and Mammals" (*Quart. Jour. Micro. Sci.* vol. lxi. pt. 2). He finds it is remarkably uniform in its development, and is always a postrematic branch of the facial nerve, save in the chick, where it is pretrematic. The relations of the parts bear out the view that the proximal region of the columella corresponds to the stapes, the quadrate to the incus, and the articular to the malleus. Collinge has a "Note on an Interesting Abnormality in the Mandibular Arch of *Chimœra monstrosa*" (*Ann. and Mag. Nat. Hist.* August). "Descriptions of Two New Lizards from Australia" are furnished by Boulenger (*Ann. and Mag. Nat. Hist.* July), and Schmidt has written "On a New Flat-fish of the Genus *Arnoglossus* from the Black Sea."

The mammals receive a fair amount of attention in the

FIG 1

FIG 2.

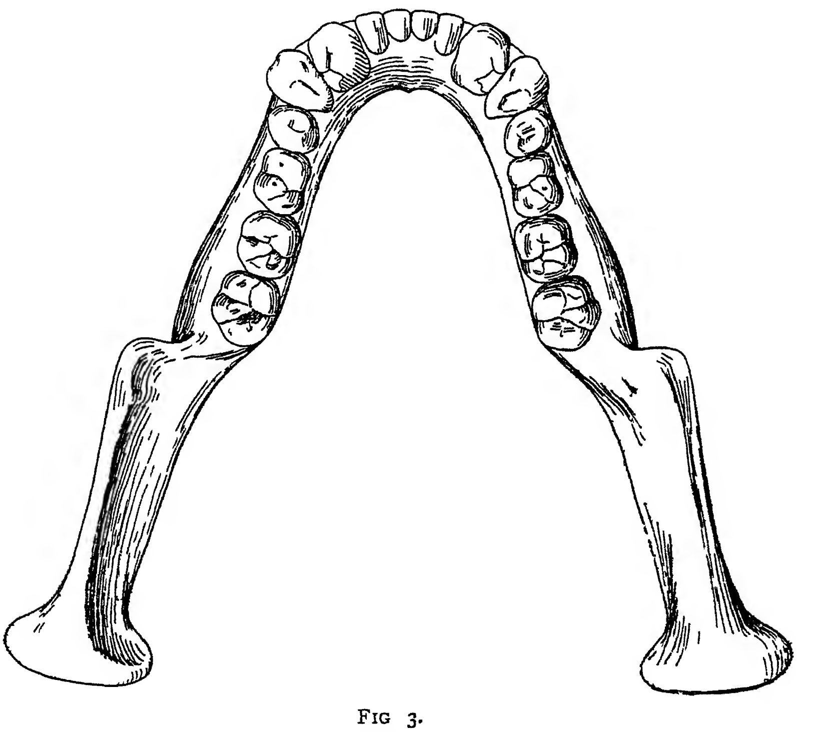

FIG 3.

Illustrations of *Sivapithecus indicus*

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Fig 1 = Portion of right side of mandible viewed from outside Natural size. Fig 2 = Fragment of left side of mandible viewed from outside Natural size Fig 3 = Restoration of mandible viewed from above About two thirds natural size

quarter under review. As noted previously, the late J. W. Jenkinson has a paper on " The Placenta of a Lemur " (*Quart. Jour. Micro. Sci.* vol. lxi. pt. 2). A description of the placenta is given, and it is pointed out that its structure conforms to that of other Lemuroids save Tarsius. A valuable contribution has been made to our knowledge of the ductless glands by two papers in *Proc. Roy. Soc.* B. 610 August. " The Development of the Thymus, Epithelial Bodies, and Thyroid in the Marsupialia " consists of two parts: Part I, *Trichosurus vulpecula*, Frazer and Hill, and Part II, *Phascolarctos, Phascolomys,* and *Perameles* by Frazer. They are abstracts of the papers to be published later in the *Transactions*, and will be best dealt with there. Thomas has three papers in the *Annals and Magazine of Natural History*, " Notes on the Asciatic Bamboo-Rats (*Rhizomys*, etc.) " and " On Bats of the Genus *Promops* " (both in July) and " New African Rodents and Insectivores " mostly collected by Dr. C. Christy for the Congo Museum (August). Two further parts of " On the African Shrews belonging to the Genus *Crocidura* " have been added by Dollman (Part I, *Ann. and Mag. Nat. Hist.* July; Part II, *ibid.* August). Pocock contributes papers " On the Species of the Mascarene Viverrid Galidictis, with the Description of a New Genus and a Note on *Galidia elegans* " (*Ann. and Mag. Nat. Hist.* August), and " On Some of the External Characters of the Palm-Civet (*Hemigalus derbyanus*) and its Allies " (*ibid.* September).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc., Public Museum, Gloucester.

In the October number of Science Progress I summarised in considerable detail an important paper by Dr. G. E. Pilgrim on *Sivapithecus indicus*, a remarkable Miocene animal, which Pilgrim believes is a primitive genus of the Hominidæ (see *Records of the Geological Survey of India*, vol. xlv. Pt. I, 1915). I am now able, with the author's kind permission, to reproduce three of his figures of this fossil. Fig. 1 shows the portion of the right side of the mandible containing the premolar and molar teeth, and fig. 2 shows the fragment of the left side of the mandible, wherein the canine and the symphysis may be seen. Fig. 3 is Pilgrim's restoration of the entire lower jaw, as viewed from above. The figures will repay close examination, for the fossil is one of the half-dozen most impor-

tant relics of a man-like creature ever discovered. The anatomical details were discussed in the last number, but attention may again be drawn to the character of the mandibular symphysis, which is short and resembles that of modern man, and has not the transverse flange which is so characteristic of the living apes and of the Pleistocene Piltdown skull.

The *American Anthropologist* for April—June has now been received, and is perhaps somewhat less interesting than usual. This number of the magazine deals almost exclusively with questions of social anthropology. A. A. Goldenweiser, of Columbia University, contributes a suggestive paper on "The Knowledge of Primitive Man." The author thinks that in the study of the mentality of savages, too much attention has been given to those departments of barbarian thought which differ most from our own, whereas insufficient attention has been paid to the positive knowledge of the savage, "his concrete experience, his familiarity with beings, things, relations, processes, actions." Thus a one-sided picture of the savage mind is often presented, and superstition probably dominates the lower races less than is commonly supposed. Among the other essays in this journal, special mention may be made of "Exogamy and the Classificatory Systems of Relationship," by R. H. Lowie, and of an article by F. G. Speck on "The Family Hunting Band as the basis of Algonkian Social Organisation."

Several important papers will be found in *Man* for the third quarter of 1915. In the July number, W. P. Pycraft, of the British Museum, writes on "A Plea for a Substitute for the Frankfort Base-line: With an Account of a new method of Drawing Skull Contours." This article is, of course, highly technical, but no craniologist should miss seeing it. Pycraft proposes to substitute for the well-known "Frankfort base-line" used in the comparison of human skulls another line "passing from the nasion backwards through the centre of the auditory meatus." The same number contains an article by E. J. Wayland on the "Occurrence of Stone Implements in the Province of Mozambique," the implements in question being of extremely rough, almost eolithic, workmanship. The September number contains a highly interesting contribution from Dr. Hugh Stannus on "Pre-Bantu Occupants of East Africa," in which the legends of gnomes prevalent in

Nyasaland are described, and are compared with the similar stories from the Kikuyu district, which were recorded in a previous number of *Man*, and which I mentioned in SCIENCE PROGRESS for July 1915. The author comes to the conclusion that the origin of these legends is to be found in the real existence of a dwarf race in these countries in early times, and there is much to be said for this thesis. Moreover, as Dr. Stannus says, the same explanation is applicable to the European traditions of strange wild dwarfs. Gnomes are not the product of our ancestors' imaginations; they actually existed.

Soon after the outbreak of the present war, certain persons took advantage of the passions raised to advocate in the popular press the heretical theory that the English are not a mainly Teutonic, but a chiefly Keltic (or Kelto-Iberian) nation, and are racially more akin to the French than to the Germans. In the September *Man* there is a paper (written in French) by F. Romanet du Caillaud, taking this view of the matter, and entitled "De l'identité des races qui ont formé les nationalités britannique et française." The argument is not convincing, and indeed the superficiality of the essay may be judged from the fact that there is no mention of the Iberian ingredient in either the French or the British population, and M. du Caillaud does not tell us what he means by the Keltic type. The races which have gone to form the two nationalities are, no doubt, almost "identical," but they have been mixed in very different proportions in the two countries. When J. R. Green, the historian, said of Anglo-Saxon England that "it was the one purely German nation that rose upon the wreck of Rome," he probably went too far, but he was more nearly right than the old school, now resuscitated, which began English history with the Ancient Britons. The English conquest of England and the Frankish conquest of France were quite unlike. The early English polity was purely Pagan Teutonic, and there is every reason to suppose that the population of the eastern three-fourths of England and of south-eastern Scotland was mainly Teutonic in blood also, though probably not to the extent that Green thought. Of course the Germanic infusion was small in Ireland, and smaller in Wales, but the bulk of the British population is in England. The truth is that in view of the great mixture of races in all the larger countries of Europe

it is only in a very qualified sense that this war can be given any racial meaning whatsoever.

Of the younger scientific societies of Great Britain, none is more active than the Prehistoric Society of East Anglia. In the first half of 1914 this society carried out some extensive excavations at Grime's Graves, Weeting, Norfolk, and the report on these researches is now published in a special volume of 250 pages (not in the Society's *Proceedings*), priced at 5*s*. The word "grave," in this connection, means not a burial-place, but any hollow or pit, and "Grime," or "Grim," is the old Anglo-Norse synonym for witch or watersprite. Our forefathers, not knowing who made the graves, attributed them to the work of preternatural beings. In point of fact, the Grime's Graves are ancient flint mines, and are almost certainly early Neolithic in date. Several excavations were made, and the results obtained are described in detail by various specialists. The general account is written by A. E. Peake, the human remains are described by Prof. Keith, the mammalian and other bones by Dr. C. W. Andrews, the Mollusca by B. B. Woodward and A. S. Kennard, and the flints and other artifacts by Reginald A. Smith, whilst shorter chapters are contributed by other scholars. So far as the human remains are concerned, there is nothing definitely to prove the date—they might be either Neolithic or late Paleolithic (though probably the former), but the age of the mines is proved by the mammifauna, which is exclusively Holocene. No trace of a Pleistocene mammal was found. This is confirmed by the molluscs, which are described in a masterly manner. Indeed, it appears to be possible to give a more exact date to the mines on the strength of the molluscan evidence, and to attribute them to Geikie's "Lower Turbarian" age—the cool epoch which succeeded the first warm phase of the Holocene. Pottery was also found. In view of all this, it is surprising to find Reginald Smith arguing for a Mid-Paleolithic (Mousterian-Aurignacian) date for the workings, albeit he eschews dogmatism. He relies upon the form of the flints, but the fact is that the specialists have overreached themselves on the question of flints, the types being more mixed up in the different ages than enthusiasts would have us believe. The evidence of the fauna is conclusive in favour of a Post-Pleistocene dating.

NOTES

The British Association

THE meeting of the British Association held last autumn in Manchester was notable for the curtailment of the accustomed social side of the proceedings, as a consequence of the war. Whilst this innovation doubtless affected adversely the numbers of members and their friends attending the open meetings and functions, it does not seem to have greatly lessened the attendance at the sectional meetings. Instead, the interest displayed in these sectional addresses and discussions was of the standard of former years. Naturally many of the younger generation of scientists were absent on patriotic duty; some, we regret to record, have lost their lives in the service of their country. The great lesson of the war was reflected in many of the addresses which were delivered at the meeting. Organisation of the nation supplied as a general theme the Organisation of Science.

In inaugurating the meeting, the President, Prof. Schuster, chose as the text of his address the common aims of science and humanity. His discourse was a closely reasoned analysis of the motives compelling the study of science and the pursuit of knowledge. The President called to his aid the attitudes of the outstanding men of philosophy in all countries and ages. Much of the current self-depreciation of our national lack of organisation is due to the confusion as to what are the characteristics of true organisation, that modification which casts aside discipline and substitutes co-operation. To say that a nation which has acquired and maintained an Empire and conducts a large trade in every part of the world is deficient in organising power is an absurdity. The utilitarian drum must, however, not be beaten too loudly and scientists must not lose sight of the idealistic side, even if events of the moment seem to drive us in the other direction. For a time material advantages may seem to monopolise the view, but the love of truth and intellectual freedom will inevitably reassert themselves in advancing civilisation.

To the delegates of Corresponding Societies, Sir Thomas Holland also had much to say on the subject of the organisation of science. Science has won for itself a place equal to that of literature in academic culture, but our success has been very limited in getting the professors of pure science to co-operate in unison with the captains of industry, who depend entirely, consciously or otherwise, on the application of scientific laws to industrial problems. Our universities as well as our scientific and technical societies suffer from a lack of co-ordination and from overlapping and conflicting interests. They are, moreover, largely dependent on private charity and a satisfactory state can only be obtained when the Government, recognising the national value of their work, takes in hand their reorganisation and financial support.

In the address to the Section of Mathematics and Physics (A), Sir F. W. Dyson lifted his hearers out of the realm of human warfare into the mysteries of the "island universe." The whole field of astronomy was passed in review and the rapid progress which is being made in the collection, classification, and explanation of our knowledge of stellar data was admirably stated. The president pointed out objections to some recent conclusions and indicated the scope of the work at present engaging the attention of the observatories of the world. Our present and prospective knowledge of the stellar system, if supplemented by constant attempts at interpretation, will enable us to proceed to the dynamical and physical study of the history and evolution of the universe.

Prof. Bone delivered a highly important address to the Chemical Section (B) on gaseous combustion and the economical use of fuel. The president outlined the recent advances in our knowledge of ignition phenomena and the negative results found for Sir J. J. Thomson's suggestion as to the potency of electronic ionisation in such phenomena. The valuable work of Prof. Dixon at Manchester and of Dr. Wheeler at Easkmeals was reviewed and a plea for wider academic training in gas analysis and research on coal and combustion was preferred. The scientific utilisation of fuel has not received the attention which it thoroughly merits, if only from the economic standpoint. The wasteful beehive coke ovens are disappearing, but the Government ought to fix a time-limit. Enlightened chemical control of gas works and metallurgical fuel-consuming works has led

to results which are sufficient to warrant the setting up by the Government of a central controlling organisation.

Prof. Cole's address to the Geological Section (C) dealt with the thermal weakening of the foundations of the earth-crust and the consequences which would be involved from such subterranean changes; the dragging movements of a mobile Untergrund; possible catastrophic breaks in the slow continuity of earth-movements as evidenced in the mountain-building periods and regional subsidence; abrupt geographical changes such as river capture. Behind all our orderly observations of to-day is the earth itself, quiescent it may be, but by no means in the sleep of death. Some day, in its due season, the earth will once more be active, with an activity with which human ingenuity will be powerless to cope.

Prof. Minchin's discourse to the Section on Zoology (D), unfortunately not delivered by himself through illness which has since proved fatal, was concerned with reconstructing the steps in the evolution of the cell, and for this purpose the study of the Protista was recommended. Cell evolution has probably taken place amongst the Protista. The fundamental importance of the chromatinic constituents of the cell in the evolutionary scheme was emphasised, for they alone of all cell elements persist throughout the life-cycles of organisms universally. Further, they possess specific individualisation and physiological predominance, especially in constructive metabolism.

Major Lyons made a strong appeal to the members of the Geographical Section (E) for more scientific and quantitative research by individuals in the many branches of the subject. The need of this throughout was made very clear, and the president pointed out how geographers can promote a real advance of their science if they will only prosecute seriously research in any of the many subjects which are readily accessible to all. Most of the data which has been collected is merely descriptive and lacks proper scientific accuracy, and there is a tendency to make broad generalisations of doubtful exactitude.

Dr. Hele-Shaw was highly critical in his address to the Engineering Section (G). The urgent needs of the present situation were uppermost in his mind throughout, calling for immediate and determined co-operation with the authorities. Education, standardisation, the metric system, exhibitions and museums, patents and patent laws, and co-operative organisation

were the chief subjects of his far-reaching criticisms and practical suggestions.

Anthropology (H) was enriched by the address of Prof. Seligmann on the prehistory of the Sudan region, more especially the Anglo-Egyptian portion. The nature and extent of the influences of Egyptian civilisation on the Ethiopian races of the Nile valley as evidenced in prehistoric remains and in the characteristics of the modern prototypes were critically reviewed. A southward drift of such influences along the Nile course and across the Congo divide is traceable, which would seem to be prior to the better substantiated westward drift which is traceable as far as the Senegal and Niger rivers.

In addressing the Section on Physiology (I) Prof. Bayliss dealt lucidly with the special set of conditions encountered in the regions of contact of phases in any heterogeneous system. Colloids, adsorption, and enzyme catalysis were discussed in this connection and the phenomena of the phase boundaries of cells, cell contents, and cellular structure generally were applied to explain many physiological actions.

Addressing the Botanical Section (K) Prof. Lang called for a change in the view adopted by botanists towards morphological problems. The causal aim in such work has been overshadowed by the Darwinian principle of natural selection. A hopeful sign is apparent in that the difficulties are being more intensely realised and a greater adoption of the causal attitude will lead to broader inductive study without producing antagonism to the phyletic aspect.

In her presidential address to the Educational Section (L) Mrs. H. Sidgwick, speaking as the first woman to occupy the chair, quite appropriately reviewed the development for women of educational opportunities equal to those enjoyed by men. The administrative side of education was presented in broad terms, and the president expressed the view that since the advent of compulsory primary education parents have been inclined to shift the whole responsibility for the education of their children on to the shoulders of the compulsory system.

Members of the Agricultural Section (M) listened to an optimistic address by the president, Mr. R. H. Rew, who contrasted the state of British agriculture during the Napoleonic wars with the state experienced to-day. On the whole the year was good agriculturally, and despite many difficulties farmers

have patriotically and intelligently contributed their share to the national effort. With the Navy predominant and strict economy a matter of common prudence, all reasonable needs in food supply will be forthcoming.

The sectional meetings were productive of several interesting and some highly important papers and discussions, from amongst which it is only possible to allude to the following:

Chemical Section:—Smoke prevention; Scientific gas-fire construction; Homogeneous catalysis.

Economics Section:—Industrial harmony.

Botanical Section:—Application of science to the cotton industry.

Educational Section:—Industry and education; Military education.

C. S. G.

Government and Chemistry

Not the least valuable result of the present devastating war is the realisation by our Government of the urgency of the co-ordination of science and industry on national lines. The need for the proper recognition of the fundamental importance in the life of the nation of the scientific worker and researcher has long been apparent even to moderate intellects, but it has taken the calamity of a world war with scientifically endowed enemies to open the eyes of our authorities to our backward position in this respect. Happily, too, the Press is beginning to realise the importance of the scientific profession, and if such enlightenment continues, we hope that the days will not be far distant when this long-slighted profession will be elevated to its proper place in the national life alongside its already honoured relation, the medical profession.

Some progress towards such a happy dénouement has been made as the result of the memorials presented to the Prime Minister and the Government on March 1 of last year by the Royal Society and the Chemical Society co-jointly.

The memorial of the Royal Society to the Prime Minister pointed out the supreme national importance of our chemical industries, and the comparatively backward state of certain of these. Speaking as the chief representative of science in this country, the Society alluded to the following as the chief causes of this deplorable state of affairs:

(1) Failure to realise that modern industry, to be successful, must be based on scientific research;
(2) Want of more intimate association between the manufacturers and the workers in science.

The urgency of a permanent as opposed to a temporary controlling committee was strongly advocated. The memorial then alluded to the Advisory Committee which had already been appointed by the Board of Trade and expressed the opinion that this Committee contained the nucleus of a permanent organisation such as was essential for the future maintenance of our chemical industries in the face of foreign competition. It was suggested that from this nucleus a larger standing committee of the nature of an Intelligence Department serving the chemical industries in the way that the Commercial Intelligence Department serves merchants and traders, might be established. An officially supported Chemical Intelligence Committee, under the auspices, say, of the Board of Trade, would secure the confidence of manufacturers to an extent that has hitherto been unrealisable. With increased facilities of communication between manufacturers and expert advisers it might confidently be anticipated that such advice would be more and more sought by those concerned, and our chemical industries benefited to a corresponding extent.

The memorial presented by the Chemical Society to the Government revealed the same dissatisfaction as regards the condition of our chemical industries. This unhappy state was to be credited mainly to the following factors:

1. The defects of our educational system, and particularly the lack of recognition of the importance of research as an essential part of the training of the student of science.

2. The want of scientific knowledge on the part of the community at large, especially of manufacturers, and the non-appreciation of the true value of scientific research.

3. The lack of organisation amongst various chemical and allied industries.

4. The almost total want of sympathy and co-operation between manufacturers and workers in pure science.

In suggesting the founding of a Committee such as that cited by the Royal Society, the members were of opinion that the duties of such a Committee should be:

(1) To investigate systematically the chemical industries of the country and to inquire into the connection of these industries with each other, and with those closely allied, such as the textile industries;
(2) to inquire into the sources of raw material required by these industries;
(3) to ensure that such sources shall not be monopolised by foreign manufacturers, and that the chemical products essential to our great industries shall be manufactured in sufficient quantity in this country;
(4) to provide what may be termed national researches in connection with such raw materials and industries;
(5) to inquire into the means of promoting the manufacture of (*a*) fine chemicals for the purposes of teaching and research; (*b*) glass for optical and other purposes; and to consider the question of the wider use of duty-free alcohol and allied solvents in chemical industry;
(6) generally to advise the Government on the most feasible means of making the Empire chemically self-supporting.

As a result of these memorials, on May 6 the Presidents of the Boards of Trade and of Education along with other members of the Government received a joint deputation of the Societies, comprising amongst other prominent members the Presidents of both Societies, the Society of Chemical Industry, the Society of Public Analysts, and the Institute of Chemistry.

Weighty and telling arguments in support of the memorials were advanced by individual members of the deputation. Prof. Perkin urged that in order to procure an adequate supply of research chemists, generous grants should be provided by the Government in aid of Chemical Departments of universities, especially of those universities that are willing to specialise in training chemists for an industrial career, and that closer co-operation between manufacturers and universities must be attained. Sir William Tilden went to the root-evil of the position, in pointing out the ignorance of the public in regard to the work of scientific chemists arising from the lack of organisation.

In those professions which have been efficiently organised,

we have not only a recognition of their utility and national importance on the part of the public, but we have a large number of officials whose emoluments are commensurate with the dignity of their position. The prizes offered by such professions as the Church and the Law are of a nature to attract into the ranks of those professions a large proportion of the able men of the country; consequently men are withdrawn from other professions they would probably prefer to enter—such as the profession of chemistry or applied science—with the result that there is a large waste of national intellect. In medicine, recently, eight physicians have been retained at a fee of £5,000 per annum each, but no such fee has ever been accorded to any scientific man.

The effect of this is that the public understands that the State must have great lawyers, a great body of clergy, and efficient doctors, who must be paid well; but a man who pursues science, however brilliantly, and makes the most important discoveries, receives, practically speaking, no recognition, because his profession is not recognised. Were scientific men employed more frequently and paid more reasonably, the public would gradually get to know that such work is important. As it is, not only are the salaries offered, even in Government establishments like the Arsenal, totally inadequate, but as the prospects of advancement are so meagre, a large proportion of the able young men who might otherwise take up chemistry as a pursuit are led into other professions.

The deputation was altogether favourably received, and in reply the President of the Board of Trade (Mr. Runciman) referred to our shortage of young chemists. He admitted that this was due to the fact that the career of chemist in this country is most precarious on account of the smallness of the remunerations offered to chemists upon whom the success of many of the industries of the country is dependent. This, he said, must be altered, and the career of the chemist must be better assured if greater numbers of young men are to be attracted to the profession. Mr. Runciman referred also to the difficulty of obtaining the co-operation of manufacturers whose businesses involved secret processes, lest in such co-operation those precious secrets might become known to competitors.

The President of the Board of Education (Mr. Pease)

recognised that the training of research workers must come within the province of the Board. He referred to the £4,000 grant already given by the Government for research, as well as the £7,000 grant to the National Physical Laboratory. He fully recognised the present unsatisfactory state, but by aiding the universities in such ways as improving their equipment, aiding the staff, and by State contributions much could be done within a few years to stimulate scientific workers.[1]

The matter was further ventilated in Parliament by a debate which took place on May 20, and shortly afterwards concrete action by the Government was outlined in a Memorandum[2] issued by the new President of the Board of Education (Mr. Henderson). This Memorandum intimated that an Advisory Council of Industrial Research would be appointed, and that the estimates for the current year would include a sum of £30,000 for the purpose of aiding research work. This Advisory Council has now been appointed, and it has been made responsible to a small Committee of the Privy Council in whose hands lie the control of the expenditure of the Parliamentary grant mentioned above.

Time alone will show the efficacy or otherwise of this scheme, and in the meantime the Council must receive the goodwill and co-operation of all concerned—scientists, manufacturers, and general public—in the arduous tasks which lie before it. To a nation spending five millions a day, however, the grant of £30,000 per annum for research so fundamental to its prosperity seems ridiculously small, although a considerable advance on the former endowment. Still, a start has been made in earnest, and the Council must press for larger grants as the organisation expands.

The demand for organisation has by no means been limited to the two Societies already mentioned. Organisation has been the theme of the presidential addresses at Manchester last year of both the Society of Chemical Industry and the British Association. Prof. Henderson made a sterling appeal for academic and industrial co-operation by the establishment of some such scheme as that inaugurated successfully

[1] A full abstract of the proceedings of the deputation to the Government, as well as the text of the memorials, will be found in the *Trans. Chem. Soc.* 1915, **107**, 974.

[2] White Paper (Cd. 8005, price ½*d.*).

in the United States by Prof. Duncan in connection with the Universities of Kansas and Pittsburg. Prof. Armstrong, in pessimistic vein, regarded the vocation of the academic chemist as completely atrophied under the present conditions of remuneration, and pressed for a development of purely technical training centres. Sir Thomas Holland, in his presidential address to the Conference of Delegates of Corresponding Societies of the British Association, affirmed that Government support is urgently needed, remarking that in this country scientific organisations, like the universities, are largely dependent on private charity. And again, at the Annual Meeting of the British Science Guild, Sir William Ramsay outlined a scheme of scientific organisation, making the Royal Society the basis of a general advisory committee in conjunction with which there would be sub-committees of the various other scientific societies in the country.

In July a scheme very similar to the above was adopted by the Chemical Society to render immediate service to overworked Government Departments during the present crisis. The Council of this Society has constituted itself a consultative body to consider, organise, and utilise all suggestions from its Fellows, reporting on the same to the proper authorities. Special committees of the following societies are co-operating in the scheme and to each committee are to be referred the suggestions with which it is the most competent to deal :

Royal Society of Agriculture
Biochemical Society
Society of Chemical Industry
Society of Dyers and Colourists
Faraday Society
Institute of Metals
Institution of Mining and Metallurgy
Pharmaceutical Society
Physical Society
Society of Public Analysts

This emergency scheme now stands constituted, and if it should prove of real service to the nation in this crisis, it is not too much to expect that it should be linked up to the National Advisory Council and placed on a firm financial basis.

C. Scott Garrett.

Government Grants for Scientific Research

In April last, the Chancellor of the Exchequer, replying to some remarks in Parliament, intimated that the Estimates for

the present year made increased provision for scientific research and for promoting certain special manufactures which this country had neglected in the past. On May 6 a deputation from the Royal Society and the Chemical Society waited on the Presidents of the Boards of Trade and Education. After calling attention to the want of application of scientific knowledge to manufactures and to the lack of organisation among the chemical and associated industries, the deputation urged the Government to give increased help to research for industrial purposes. The President of the Board of Education said that the matter had been under consideration by his Board for some time and that a scheme for the administration of Government grants for scientific research had been approved. At the end of July, the Board of Education issued a Memorandum entitled *Scheme for the Organisation and Development of Scientific and Industrial Research.*

The following is a brief summary of its provisions :—

1. The scheme is designed to establish a permanent organisation.

2. It is to operate over the United Kingdom as a whole, so that there may be " a single fund for the assistance of research, under a single responsible body."

3. It provides for the establishment of—

(*a*) a Committee of the Privy Council responsible for the expenditure of any new moneys provided by Parliament for scientific and industrial research ;

(*b*) an Advisory Council responsible to the Committee of the Privy Council and composed mainly of eminent scientific men and of men engaged in industries dependent on research.

4. The Committee of (the Privy) Council will consist of the Lord President, the Chancellor of the Exchequer, the Secretary for Scotland, the Presidents of the Boards of Trade and Education, the Chief Secretary for Ireland, and other Ministers and Members, of whom the first will be Lord Haldane, the Right Hon. A. H. D. Acland, and the Right Hon. J. A. Pease, M.P.

5. The Advisory Council will consist, to begin with, of the following members : Lord Rayleigh, Dr. G. T. Beilby, Mr. W. Duddell, Professors B. Hopkinson, J. A. M'Clelland and R.

Meldola, and Mr. R. Threlfall, with Sir W. S. M'Cormick as administrative Chairman.

It will be noted that with the exception of the Chairman all are Fellows of the Royal Society.

The duties of the Advisory Council will be to advise the Committee of Council on—

(i) proposals for instituting specific researches;
(ii) proposals for establishing special institutions or developing existing institutions for the scientific study of problems affecting particular industries;
(iii) the granting of Research Studentships and Fellowships.

It is recognised that it will probably be necessary in due course to strengthen the Advisory Council by appointing additional members.

6. It is proposed to ask the Royal Society and other leading scientific societies and associations to initiate proposals for the consideration of the Advisory Council, and to establish a regular procedure for inviting and collecting proposals.

7. The Advisory Council will frame a scheme for their own guidance in recommending proposals for research and for the guidance of the Committee of Council in allocating funds. One of their chief functions will be the prevention of overlapping between institutions or individuals engaged in research.

The Memorandum states that difficulties in the way of the scheme owing to the war have not been overlooked or underestimated, but "we cannot hope to improvise an effective system at the moment when hostilities cease, and unless during the present period we are able to make a substantial advance we shall certainly be unable to do what is necessary in the equally difficult period of reconstruction which will follow the war."

This statement may be commended to the notice of those who are urging that all efforts to advance education and research should cease until the war is over.

The Memorandum also points out that a great part of all research will necessarily be done in State-aided universities and colleges and that the supply and training of competent research-workers can only be secured through the public system of education.

Unfortunately, our universities and colleges have to face

a serious situation. The national call for recruits has largely depleted the number of their students and younger graduates, their income from fees is correspondingly diminished, and the Treasury Commissioners are now advising a reduction in the State grants for university education, the closing down of departments, and the disbanding of teaching staffs, wherever possible. Of course the need for national economy is urgent, but education, especially in its scientific and technical aspects, should be one of the last things to be stinted. To run the risk of disorganising university education and research for the sake of saving a comparatively small sum of money would surely prove a false economy in the end. Before the war, the United Kingdom had only 23,000 whole-time students in universities and technical institutes as compared with 90,000 students of collegiate grade in the United States, and 71,000 matriculated students in German Universities.[1] These figures may be taken as a rough index of the value that each nation places on higher education and scientific training. Allowing for differences in total population, it means that the United States and Germany each had between two and three times as many university students as the United Kingdom.

The nation is awakening to the tremendous part that applied science is playing in the war. Does it yet realise that in the industrial and commercial struggle that must inevitably follow the war, science will play an equally important part? If we are adequately to meet the needs of the future, we must educate in natural science a far larger proportion of the youth of the nation than we have done hitherto. This is essential in order to make good our deficiencies in the past and to replace those who fall in the war.

It is to be hoped, then, that the Government, which has recognised, in this new scheme, the importance to the nation of scientific and industrial research, will do all that may be possible during the continuance of the war to increase science teaching in our schools and to assist our universities and colleges through a difficult time, so that they may be ready, when peace returns, for all developments of scientific education and research that may be required to meet the needs of our great industries.

[1] Prof. R. A. Gregory, Ninth Annual Report of the British Science Guild: Appendix F.

With regard to grants for medical research, it may be recalled that under the terms of the National Health Insurance Act a sum of about £56,000 is expected to be available annually for purposes of research and investigation. A start was made early last year (1914), when a Medical Research Committee was formed with Lord Moulton as Chairman and Dr. W. M. Fletcher, F.R.S., as Secretary. The first annual Report of this Committee has recently been presented to Parliament. It is not possible here to give an adequate account of the large amount of research work inaugurated by the Committee, but attention may be directed to one important development. On the outbreak of war, the Committee at once modified its original plans and laid itself out to give assistance to Government Departments, especially to the Army Medical Department. The report shows, for instance, that the Bacteriological Section, under the direction of Sir Almroth Wright, has given the whole of its time and services to the investigation of problems arising directly out of the war. As an example may be mentioned the very important researches into the pathology and treatment of infected wounds. Other departments of research have also conducted special inquiries into matters more or less connected with the war, such as poison gases, the synthesis of certain drugs, statistics of sick and wounded, typhoid and allied infections, and cerebro-spinal fever. From this work, in one short year valuable results have accrued which have already saved many lives, greatly lessened the sufferings of the wounded, and prevented the spread of epidemic disease. The war, indeed, has given to the Medical Research Committee a unique opportunity of demonstrating to the Government and the nation in a way that can be readily appreciated by the lay mind that money given for purposes of research is one of the soundest forms of national investment.

H. S. W.

Medicinal Plants in Queensland

There is a shortage in certain lines of medicinal plants in Queensland, of which Germany was the main source of supply, and the question of local production is being seriously considered in the Commonwealth. Definite action has been taken, and a board has been appointed to inquire into the whole matter, and if possible, to establish the cultivation of medicinal plants on

practical commercial lines. The board will not be treated meanly in regard to funds, as the matter is of great urgency, and by working in conjunction with the hospital, it will be kept on practical lines. At first it is proposed to import seeds and plants, and allocate them to the high schools, to be tested in the school gardens. The different plants will be sent to those districts where, in the opinion of the board, the soil and climate are suitable for producing in the highest degree the principle which gives each plant its value. It is known, for instance, that while peppermint thrives luxuriantly in the cool, moist districts, it contains more of the essential oil when grown in a warm, dry climate. The produce from the school gardens will be tested at the hospital laboratory, and also at the laboratory of the Department of Agriculture, by which means it is expected to obtain the necessary data to allocate the various plants to the districts best suited to them. The results will be made public, and persons desirous of embarking in the industry will be invited to communicate with the board, which will give them the necessary instructions. As some of the plants—opium poppy, for instance—must not be grown indiscriminately, it is probable that the board will have the power to issue licences, and at the same time the growers will be guaranteed a market and a definite price for their produce. The scheme seems to be a feasible one, and has the merit of providing the growers with a market in advance.

The Medical Research Committee

The first annual Report of this Committee (1914–1915) has just been issued by Messrs. Harrison & Sons, but too late for detailed mention in this number of SCIENCE PROGRESS.[1] The activities of the Committee cover a very wide and important field, and the first section of the Report describes its administration. All medical research workers should obtain the publication, as the Committee is evidently going to make a great reform in medical investigations in Britain.

Sir Edwin Durning-Lawrence: a Patron of Science

The following note on some scientific investigations originated by the late Sir Edwin Durning-Lawrence some years ago and placed for execution under my direction, may be of interest

[1] See page 478.

to men of science. I do not know which to admire more, the fertility of idea which Sir Edwin showed, or the generosity with which he placed funds at our disposal for the purpose of such researches; and I certainly think that a short note on the subject should be placed upon record. About ten years ago Sir Edwin Durning-Lawrence seems to have become acquainted with the late Sir Rubert Boyce, Professor of Pathology at the University of Liverpool. Boyce was an Irishman of the very best type, with all the vigour and enthusiasm of his race, and directed these qualities especially to the promotion of medical investigation. I do not know exactly what passed between them then, but at that time Boyce was Dean of the Liverpool School of Tropical Medicine (of which I was Professor), and he was always keenly interested in yellow fever. Now Sir Edwin had long considered a hypothesis, which seems first to have seen the light in America, that yellow fever can be easily cured in the presence of cold, and Sir Edwin at this time appears to have offered, through Sir Rubert Boyce, a very considerable sum of money to the Liverpool School of Tropical Medicine, for the purpose of carrying out investigations on this subject. Unfortunately such investigations would have been extremely difficult, as they would have necessitated the construction of special freezing chambers in some very unhealthy part of America, and it was always doubtful whether any results would accrue. Apparently therefore, after due consideration, the School found itself unable to conduct the proposed researches.

In the year 1909, however, when I was Professor of Tropical Medicine at the University of Liverpool, we received a considerable sum of money from the Colonial Office for researches; and determined to employ this money, together with other funds which I might be able to raise, in prosecuting a very detailed investigation of certain parasitic maladies in England. Remembering Sir Edwin's generous offer, I wrote to him again, and asked him whether he would pay for a freezing chamber at the University of Liverpool, and also for a special research assistant to carry out investigations. He immediately responded with the sum of a thousand pounds, with promise of further sums when needed. My idea was that Sir Edwin's original conception would work in well with the detailed researches which I was now contemplating, and I proposed to employ his freezing chamber, not for the study of yellow fever, which of

course was impossible in Britain, but for the examination of the general question as to the effect of cold upon parasitic diseases.

At Sir Edwin's wish, the freezing chamber was made by Sir Alfred Haslam, the head of the great refrigerating works at Derby. It was 12 ft. long by 7 ft. wide and 7 ft. high, and was quite commodious enough for a patient to sit inside it, or even to move about in it. The temperature could be kept many degrees below freezing point, and yet without an unpleasant draught of cold wind being blown into the interior of the chamber. It was completed on September 10, 1909. Major C. L. Williams, Indian Medical Service, retired (formerly Professor at the University of Madras), was engaged to carry out the investigations; he remained in charge only for about six months, and was then succeeded by John Gordon Thomson, M.A., M.B., Ch.B.

Our preliminary experiments were directed to the investigation of the question as to whether various trypanosomes, parasites of rats, guinea-pigs, and mice, would be affected in number by these small animals being kept in a very cold air. Consequently infected animals with controls were placed inside the chamber in cages, were well fed and well bedded.

It was observed that they required much more fat in their food, and this was provided them; and it was already known, from the experience of the large freezing chambers employed for the preservation of meat, that rats do well in such, grow fatter, and acquire more hair. The diseases experimented with were various infections with trypanosomes, bovine tubercle, cancer in mice, tetanus, and spirochætes. The result of the experiments was that Major Williams thought the cold did retard the progress of infection to some extent.

When he was succeeded by Dr. John Thomson, the latter continued these experiments, but was also very largely employed for the purpose of studying infections outside the cold chamber in collaboration with his brother, Dr. David Thomson, Dr. Simpson, Dr. Fantham, and other workers of the School and myself. The results have been published in the appended list, and it is not possible to summarise them briefly here.

So far as the effect of cold is concerned, we generally felt that it might have a considerable influence on treatment. A case of human sleeping sickness who was often subjected to the cold in the chamber always declared that it greatly invigorated

him. It was curious that he felt drowsy and seemed inclined to drop off to sleep when in the chamber.

We ourselves tried the chamber on several occasions, and certainly experienced the same effect. After one of these experiments, the coldest air of British winter felt quite warm and balmy when we issued into it. Up to the middle of the year 1912 we had made many experiments in the cold chamber, and concluded that more of any such experiments on animals were scarcely required, and that what was wanted was the subjection of human patients suffering from various diseases to the dry cold of the chamber. Unfortunately, though we issued circulars to the medical men, none of them seemed inclined to make the experiment, especially as the cold chamber was not actually built in association with the hospitals. Hence the final test which I had hoped for when the chamber was first built never came to be properly applied. At the end of 1912 I left Liverpool to reside in London, and was appointed Honorary Physician for Tropical Diseases to King's College Hospital. We then thought of removing the chamber to that hospital, but as the latter was not completed, I doubted whether it would be advisable to ask Sir Edwin at that time for the money required for that removal. One thing and another prevented further elaboration of this idea, and the old freezing chamber in Liverpool was finally closed. Then, just as I was thinking of starting one in London, Sir Edwin died.

Nevertheless I think that the results obtained were quite worth the money and believe that the day will come when physicians will depend much more for treatment upon such things as temperature, light, and dry air than they do now. I had always hoped to commence this kind of treatment, but the difficulty of conducting protracted and out-of-the-way researches in Britain has finally dissuaded me from making the attempt. We shall continue to have drugs poured down our throats for many years to come. I consider, however, that Sir Edwin Durning-Lawrence, with whom the idea started, was pioneer in this subject.

The services of his research assistant, Dr. John Thomson, were invaluable in the other investigations which I referred to, and will be found described in the subjoined list of papers.

I should mention three more ideas started by Sir Edwin and the researches on which were paid for by him. The first

was to find a really effective hair-wash to destroy the vermin which we know are very common on children even in Britain. Researches on this subject were subsidised by Sir Edwin, and were made at the Runcorn Laboratories at the Liverpool School of Tropical Medicine. Another research was to discover the physiological effect of eggshell chalk. It was Sir Edwin's idea that chalk taken direct from eggshells would be much more beneficial to rickety children than chemical chalks. A certain number of researches were commenced, but never reached an effective result. Thirdly, he once wrote to me asking whether I knew any one who would experiment on the best form of explosive bullet for bringing down airships and aeroplanes. This was before the war ; and I note Sir Edwin's prescience with admiration. He gave a hundred pounds for the work, and I recommended an investigator, who however has never told me the result.

No one had more ideas, and often more useful ideas, than Sir Edwin. But it is not easy to get other people to take the trouble to bring one's conceptions to fruition. Work of this kind pays the worker so badly that he often takes little or no interest in it, unless it springs in his own mind. As we become older, we see how grateful humanity should be to the few persons who have ideas, and also to the very few persons who attempt to work them out.

In October 1913 Sir Edwin very kindly gave me a fund for the small expenses connected with the Beck Laboratory of the Royal Society of Medicine, of which I am Honorary Director, and he and Lady Durning-Lawrence have continued this subscription ever since. This has enabled the laboratory to be used. The work done in it as been of very considerable interest and was published by Dr. David Thomson in the Marcus Beck Laboratory Report in the *Proceedings of the Royal Society of Medicine*, July 1914. This Report contains a series of five good papers. In addition to the work of Dr. David Thomson, his brother Dr. John Thomson also commenced studies in the laboratory during 1914, but at the commencement of war both brothers left, one for military service, and the other for employment with the London School of Tropical Medicine. The laboratory is, however, still used by Fellows of the Royal Society of Medicine. As I told Sir Edwin when he was living, but for men like himself science would be in a bad way in Britain.

The State paid very little money for current researches, and none at all for encouraging investigations among private individuals. The result was that less and less investigation work was being done by Britain; and probably none at all would have been done for the last century except for the munificence of men like Sir Edwin. He was one of those rare persons of whom it may be said that he not only had ideas of his own, but loved to encourage the fruition of those of others.

Sir Edwin was born on February 2, 1837, and died on April 21, 1914.

RONALD ROSS.

February 1, 1915.

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Mr. Man-in-the-Mass

"So," said Mr. Man-in-the-Mass, "I have at last banished that fell magician, and am master in my own house. I will therefore take his staff, ascend the mountain, and exult in the prosperity of my island. Come with me, my dear friends, Field-Marshal Militarius and Baron Politicus, and I will show you something; but first bring with you a large bottle of that stuff which I like so much." So saying, he toiled with his friends to the top of the mountain, where a wonderful spectacle met his eyes. Beautiful ships rose and fell in the air, or clove the waters of the surrounding sea, or even dived into its depths. Swift creatures like great birds or dragonflies darted hither and thither in the sunny air, and white-winged yachts sailed on the waters. In the plain below there were numbers of beautiful cities and villages, full of people who toiled and rejoiced, and on the flanks of the mountain there were groups who discoursed

on wise matters or listened to wonderful songs and music ; and as far as the eye could reach, this spectacle could be seen.

"There," said Mr. Man-in-the-Mass, "see what I have made. That fell magician (he always liked the word *fell* because he thought it to be a fine one, though it contained only one syllable) pretends that he did this ; but it was really I who made it, though he kept me at it working like anything for thousands of years. I will therefore seat myself on this rock, and will have you, my friends, crown me Emperor. First I will give you these magnificent orders—not that you have done anything for me as yet, but because you tell me that you will do something some day." So he sat himself down on a rock on the top of the mountain, and his friends, Field-Marshal Militarius and Baron Politicus, crowned him with thistles and pinned the new orders on their breasts, which were already covered with decorations. "So," exclaimed Mr. Man-in-the-Mass, "I am Emperor at last, and my name is Alexander-Pompey-Cæsar-Autocrat-Plutocrat-Democrat-Journalist-General-Admiral-Secretary-of-State-Hans-Jean-Bull-Smith-Jones-Robinson-the-Great, M.P. It is I who have done all this. Now I will make a world ten thousand times more beautiful in a second. But first let us have a drink out of that bottle."

After taking the drink, he waved the magician's wand. But in a second the sun was darkened by great clouds, and the most terrible din resounded everywhere. The people on the flanks of the mountain sprang to their feet and ran fighting and tearing at each other. The airships flew at each other and began to drop bombs on the beautiful cities and villages. Out from the depths of the sea rose great iron monsters which torpedoed the ships that were sailing peacefully on the waters. Simultaneously the air was rent with the shriek of innumerable bullets and shells, and some of these began to burst round Mr. Man-in-the-Mass and his two friends. They stood aghast, and then jumped behind the rocks for shelter. The clouds grew more and more black, and the noise became more and more terrible. Where there was nothing but prosperity and beauty, now could be seen innumerable dead and dying, the ruins of cities, and human bodies scattered by the bursting of shells, while women and children ran shrieking amid the turmoil. Mr. Man-in-the-Mass and his two friends all blamed each other as they ran, and soon began to fight amongst themselves. The Field-

Marshal and the Baron tore away each other's orders, and both fell to kicking their master.

Suddenly, however, there appeared the figure of a tall old man with a long white beard, who was dressed in an old robe figured with quaint mathematical designs; and by his side there stood a beautiful lady with tears running down her cheeks—wherever her tears fell there sprang up wonderful flowers. "So," said the Magician, "you see what you have done, wicked Caliban—you and your friends, Trinculo and Stephano. I cannot leave you for a moment, but that you attempt some foolish trick of this kind. How do you dare to wave my staff when you do not know how to use it? Down again for another hundred years. See if you cannot learn more wisdom this time." Everywhere in the air was heard the laughter of spiritual voices as Ariel and his sprites chased Caliban, Trinculo, and Stephano down from that wonderful mountain.

* * * * *

Men in the mass see the wonders round them—engines which move on the land and the sea and in the air, messages borne about the earth, great poems, works of art, sublime music, and the stored wisdom of ages—and imagine that they have made them. Therefore they conceive themselves to be the children of God, the inheritors of immortality, and, indeed, nothing but gods themselves and above the order of nature. But it is not they who have done these things. The wonders were made not by them but by beings of another order who toil, plan, and think incessantly and who perish because of it. But see the masses of men. Do they not toil only for their own ends, to collect wealth or to earn small pleasures? After their school-days, what do they do to improve themselves or to benefit humanity? See them, for example, in hours of leisure (as upon board ship): nearly all of them do nothing but sleep, eat, play silly games, and look at silly books containing silly pictures—and so their life is spent. Not only have they not made the wonders referred to, but they are not capable of making them; not one in a thousand of them ever has a new idea; and not one in a thousand of these has the capacity to bring a new idea to maturity. They have learnt a few tricks which they call professions; but outside these domains they know nothing

and care for nothing. Their beliefs and opinions are merely those which have been taught to them by self-schemers and by the idle stuff which they read in worthless papers. Not for them the passionate desire for truth; and too often their religions are but superstitions and their ideals base. And they get for themselves rulers who are of the same kind: the hereditary impostors who pretend to possess the mandate of God—who would really be, if he gave them such a mandate, a demon; and the political adventurers and the demagogues who rule the world, not in the interests of the Great and the Good, but of themselves; and men in the mass look upon these quacks as being heroes and prophets. Thus it happens that suddenly at times the labours of those who have really made the wonderful things which have raised men from the brute are dashed to pieces, and men in the mass become the victims of themselves, and destroy themselves; just as the glutton dies from gluttony and the drinker from drinking. They fall upon each other and tear each other to pieces by millions; regardless of Reason they become the victims of Unreason; regardless of Truth they are massacred by Untruth; worshipping fools they are led into enormous slaughterings by the puerile ambition or stupidity of their heroes. The sun is obscured and it seems as if the world is tumbling to pieces.

The Great Poet when he came near the end of his life saw the truth of all this and constructed his last and most beautiful allegory. He saw the exile of the gentle and wise Bringer of Prosperity with his daughter the Wonderful; and he saw also the qualities of the mass, the average of the multitudinous variations of nature; the creature with the infinite crassitude of the intellect of the mob—Caliban.

R. R.

The Cash Value of Scientific Research

Such is the striking title given by Prof. T. Brailsford Robertson to a pamphlet [1] written in trenchant and arresting language. He opens his paper by drawing an amusing, yet accurate, picture of the state of mind of the non-scientific mortal. "The average man in the street or man of affairs," he says, "has no very clear conception of what manner of man a 'scientist' may be. No especial significance attaches in his mind to the term. No

[1] Reprinted from the *Scientific Monthly*, November 1915.

picture of a personality or his work arises in the imagination when the word 'scientist' is pronounced. More or less indefinitely, I suppose, it is conceded by all that a scientist is a man of vast erudition (an impression, by the way, which is often strikingly incorrect) who leads a dreary life with his head buried in a book or his eye glued to telescope or microscope, or perfumed with those disagreeable odours which, as everybody knows, are inseparably associated with chemicals." And he proceeds to show up this "average man" as taking cognisance of science only at rare intervals when some unusual discovery forces itself on his notice, and as possessing no idea at all of the vast scope of science and the important part it plays in the life of every individual. The chief point, however, that Prof. Robertson brings out very clearly is the fact that science is denied the business basis of commercial pursuits. In business concerns it is an elementary rule that part of the profits shall flow back into the business to be devoted to the production of future output; and yet science, which gives enormous sums of money to the world, is supposed to maintain itself as best it can on practically nothing. "Of all the inexhaustible wealth which Faraday poured into the lap of the world, not one millionth, not a discernible fraction, has ever been returned to science for the furtherance of its aims and its achievements, for the *continuance* of research." It is impossible within the compass of so short a note to quote the figures he brings forward to prove this fact, but a perusal of them will well repay the reader for his trouble. These justify his statement that "there is no regular machinery for securing the permanent endowment of research, and it is always and everywhere a barely tolerated intruder. In the universities it crouches under the shadow of pedagogy, and snatches its time and its materials from the fragments which are left over when the all-important business of teaching the young what others have accomplished has been done. In commercial institutions it occasionally pursues a stunted career, subject to all the caprices of momentary commercial advantage and the cramped outlook of the 'practical man.'" Only one sentence calls perhaps for criticism, when he tells us that "Faraday died a poor man . . . because . . . he found it necessary to choose between the pursuit of wealth and the pursuit of science, and he deliberately chose the latter. This is not a bad thing. It is perhaps as it should be, and as it has been in

the vast majority of cases." Why, we ask, because a man happens to be endowed with genius, should he therefore lack the means of subsistence? Is genius a sin for which poverty is a fitting punishment? Surely the old saying is in nowise disproved that the labourer is worthy of his hire. Unlike most writers who are apt in searching out evils and modestly retire when asked to provide a remedy, Mr. Robertson is ready with a panacea. He shows us that " as members of the body politic, we can assist the development of science in two ways. Firstly, by doing each our individual part towards ensuring that endowment for the university must provide not only for ' teaching adolescents the rudiments of Greek and Latin ' and erecting imposing buildings, but also for the furtherance of scientific research," and secondly by resorting to the expedient of the " collection of a tax upon the profits accruing from inventions (which are all ultimately if indirectly results of scientific advances) and the devotion of the proceeds from this tax to the furtherance of research." This, he concludes, " would not only be a policy of wisdom in the most material sense, but it would also be a policy of bare justice."

Economy

Every individual knows how difficult it is to alter a wrong train of thought after it has once crystallised into habit, and that the only way to eradicate such an evil is to cast it out before the stage of petrifaction. And yet how seldom is this applied by bodies of men, in Committees, Departments of State, and the nation at large. There has lately been a most striking example of this in the treatment meted out to Prof. Bottomley by the Board of Agriculture. Mr. W. B. Bottomley, Professor of Botany and Vegetable Biology, King's College, has, by his discovery of bacterised peat,[1] made it possible to double the production of our food supply, and one would have thought that the refusal of the Board of Agriculture to avail itself of such a well-timed boon would have been impossible. That this discovery will be utilised in England is due entirely to Prof. Bottomley's patriotism in refusing all offers of high emolument from German sources. In his lecture delivered at the Royal Botanic Society, October 18, he narrated how a German took the trouble to visit him and try to obtain the result of his genius

[1] See Prof. Keeble's review on page 520.

for his own country; and we do not doubt that in times of peace, when ideas of patriotism were not so much to the fore, this valuable discovery would have quietly slipped away across the seas after so many of its fellows. Of course much has been done lately by the English nation in the way of reform, yet the old habit of saying no to the new idea, just because it is easier to say no and avoid immediate trouble, still remains. The nation is to-day shrieking for economy, economy of money; but there is an economy of thought that is equally valuable and just as equally far-reaching. The lazy "no" results in loss of trade; then comes the awakening to that fact, the laborious investigation of the reason for its disappearance, and the still more laborious efforts for its reinstatement. Could there be greater waste of thought and time and money? True economy lies in the alertness of thought that perceives at once the value of any new idea when it is *first* presented.

CORRESPONDENCE

To the Editor of "Science Progress"

LOGIC: A REJOINDER TO A REJOINDER

Sir,—If your columns are open to correspondence I should like to take up the cudgels on behalf of Miss Stebbing, who is assailed by Dr. Mercier in your last issue.

The doctor adduces four concrete examples to fortify his arguments, and as the concrete is more easily handled than the abstract, I will refer to them one by one.

1. If The bed contains nothing but geraniums and violas,
then It contains no asters.

This Dr. Mercier propounds as a specimen argument which is not a syllogism. Miss Stebbing proceeds to show that it is a syllogism of the most ordinary kind, by putting the premiss in the form

All the flowers in the bed are geraniums and violas.

Whereupon her critic with an unnecessary amount of ironical banter charges her with making a statement unsupported by proof. Now, waiving the minor point that the conjunction *or* should have been used instead of *and*, the two premisses are identical. The word *If* with which Dr. Mercier introduces his argument is the equivalent of the old English *gif*, or give, in the sense of grant, and is understood before every logical premiss. Logic takes no cognisance of the truth or falsehood of a premiss. That is a question for other sciences or other departments of thought. Logic merely affirms that given certain premisses such and such a conclusion does or does not follow. Dr. Mercier's argument in syllogistic form would then read:

Given that everything growing in the bed is a geranium or viola,
Given that no geranium or viola is an aster,
then No aster grows in the bed.

2. As an example of an argument in which the middle term is undistributed, Dr. Mercier gives—

If Hannibal crossed the Alps,
and The part of the Alps that he crossed is impassable for elephants ;
then He took no elephants across with him.

In her reply Miss Stebbing makes a slip of which her opponent takes prompt advantage, as he is perfectly entitled to do. She says the middle term is distributed in both premisses, whereas in the first it is the predicate of an affirmative proposition, and is, of course, undistributed. However, to counterbalance Miss Stebbing's one mistake, Dr. Mercier makes two. First he says—The middle term is the Alps. It is nothing of the kind. The middle term is the part of the Alps crossed by Hannibal. Surely he does not imagine that Hannibal was of such transcendent size as to cover the whole Alpine range in his passage. Then he goes on to say, " In the second premiss part of the Alps only is referred to, and part of a class is an undistributed term "—quite ignoring the fact that he has added a qualification, the part of the Alps *that he crossed.* The qualifying adjective makes all the difference, and turns the undistributed into a distributed term. " Part of the population " is an undistributed term, but say " The healthy part of the population," and you distribute the term at once.

3. If Some of the crew manned the jolly boat,
and Others manned the long boat ;
then The whole of the crew were enough to man both these boats.

This Dr. Mercier puts forward as an argument in which a term is distributed in the conclusion, although it is not distributed in either premiss, and here I admit Miss Stebbing's reply is not quite satisfactory. However, there is a perfectly valid answer, viz. that the conclusion which Dr. Mercier draws is not the proper conclusion from the premisses. The correct conclusion is, *Some* of the crew were enough, etc. In order to arrive at Dr. Mercier's conclusion, a further chain of reasoning is necessary, as follows :

Two companies of the crew were enough to man the two boats.
The whole crew consisted of the two companies or more.
Therefore the whole crew were enough or more than enough to man the two boats.

4. Dr. Mercier controverts the principle that all reasoning consists in the application of general rules to particular cases. He seems to forget that this principle applies only to reasoning by argument, and not to what logicians call immediate inference, as when from Some German subjects are Poles, we infer that Some Poles are German subjects. He offers Miss Stebbing a year to find the application of a general rule to a particular case in the following :

If There are more little pigs than there are teats,
then One little pig must go without.

He might as well have given her twenty years. There is no reasoning here whatever. It is simply stating the same thing twice over in different language. No form of words is an argument unless it contains a conclusion which is not specifically implied in a premiss. If you tell us that A is greater than B, you are not giving us any fresh information by saying that B is less than A.

In my opinion Miss Stebbing has scored all four points and may justly claim the game.

Yours, etc.,
W. H. Winter.

" SYMBIOGENESIS "

Sir,—Kindly allow me to reply to the criticism contained in a review of my book on *Symbiogenesis: The Universal Law of Progressive Evolution,* which appears in the October issue of Science Progress. The point at issue briefly is : Do the facts of inter-relatedness and of Symbiosis warrant my theory together with its " bio-economic " and " bio-moral " implications ?

For the sake of clearness my reply has taken the dialogue form : *I.* is to stand for inquirer, and *W.* for writer.

I. What do you mean by Symbiogenesis ?

W. I mean the direction given to evolution by the long-continued operation of Symbiosis in the production of higher

forms of life and in the more complete development of beneficial relations between them.

I. What is Symbiosis?

W. It is usually defined as a physiological partnership between organisms of different species.

I. Is this "partnership" productive of economic, genetic, and social gains in a real and permanent sense?

W. It is primarily economic. When the association becomes sufficiently systematic and intimate, it conduces to pronounced physiological effects affecting sex, structure, status, and biological correlations.

I. Can you give an instance?

W. The lichen presents a symbiotic association between an alga and a fungus, a union calculated to meet by mutual effort the economic problem of existence where by single efforts it could not so well be met. Systematic economic co-operation in this case of "attached" Symbiosis has led to a high degree of reciprocal adaptation and reciprocal differentiation in the physiological economy of the organisms concerned. Indeed the physiological reciprocity has here become so intimate that it required years of painstaking research and of controversy to establish the fact of the compound and dual nature of the lichen. It has since been shown that the reproductive organs of the lichen are of a typically fungal character, *i.e.* the reproductive function could be deputed more specially to one of the associated organisms, whilst the other specialised in other important directions (photosynthesis with carbohydrate manufacture). Symbiosis here is responsible for new and improved economic and new and improved genetic values—important also so far as the correlated world of life is concerned. The lichen is a veritable pioneer of "organic civilisation"!

The radius of symbiotic action and reaction, however, has in course of evolution become very much wider than in such "attached" partnerships. The highly specialised partners eventually separated, betaking themselves to wider fields of action though maintaining "non-attached" Symbiosis. Here the effects of mutual stimulations upon the development of sex are no less important. I have demonstrated how in these cases we must look for *some of the faetors of inheritance to the symbiotic environment and that they cannot operate unless the symbiotic co-operation is fully carried out.*

This is of the utmost importance, not only so far as heredity is concerned, but also as indicating how symbiotic momenta generally are the mainstay of health, of "endowments," and of progress, whilst failure duly to maintain these momenta causes the organism to drift from Symbiogenesis into Pathogenesis.

The most important case of Symbiosis in the organic world is constituted by the complementary interaction between plant and animal which takes place on a grand scale in Nature.

Recurrent friction notwithstanding, this plant-animal symbiosis has established itself successfully and effectively in course of evolution. Obviously it must have involved, on the part of those mainly engaged, definite "symbiotic" activities, definite "symbiotic" relations and definite "symbiotic" duties. That reciprocity between the "kingdoms" really constitutes a case of Symbiosis has of course been recognised. For some reason or other, however, only "attached" cases have been labelled Symbiosis, which is regrettable, for it caused it to be overlooked that Symbiosis involves the very principle evolutionists were in search of: the definite and persistent principle, capable, as Samuel Butler would say, of acting as a rudder and compass to the accumulation of variations, and of making them keep steadily on one course for every species, until eventually many havens far remote from one another are safely reached.

It also caused it to be far too little realised to what an extent organisms are, in Robert Louis Stevenson's words, "condemned to some nobility." In the *Encyclopædia Britannica* it is stated "that such terms as symbiosis, commensalism and mutualism cannot be sharply marked off from each other *or from true parasitism* and must be taken as *descriptive terms* rather than as definite categories into which each particular association between organisms can be fitted." [Italics mine.]

Symbiosis is there actually treated under the head of Parasitism. Surely the time has now come to mark off the reciprocal from its opposite, the non-reciprocal. Surely the definition of Symbiosis is narrowed down too much if it is to apply only to those doubtful cases which hover near the borderland of parasitism.

The evidence of how much in evolution is due to and dependent upon beneficial reciprocal differentiation is enough to show that the chain of symbiotic relations is the medium not

only of the perennial kinship and the perennial plasticity of life, but that it is also the medium of maintaining in some profound manner the path of what one might call physiological and biological "legitimacy." Goethe's:

> Ein guter Mensch in seinem dunkeln Drange
> Ist sich des rechten Weges wohl bewusst,

contains a universal biological truth; and this in so far as organisms are involved in Symbiosis.

I. You would make all systematic physiological reciprocity synonymous with Symbiosis? You seem to regard evolution mainly as the development of a symbiotic principle with a kind of bio-moral sanction behind it. What of competition?

W. The principle of the interdependence of organisms is already vaguely conceded. It is only a question of what nexus it is that binds a particular set of organisms together. Given an insufficiency of symbiotic nexus, pronounced friction must sooner or later arise. I do not deny the importance of competition *as an instrument of co-operation*; but I hold with Joubert: "C'est la force et le droit qui règlent toutes choses dans le monde; *la force en attendant le droit.*" I am merely consistent in seeking to establish the continuity of economic and moral principles.

I. But are you not going too far in identifying the natural system with the "laws" of economics and in fixing upon man the duty of coming into line with "symbiotic" Nature even in the matter of food?

W. The contrast between man and Nature, as Prof. A. Dendy says, is purely arbitrary. The "Science of Life" cannot do without a chapter of Bio-Economics—the knowledge of what makes for real economy in the world.

Remember that the terms "partnership" and "division of labour" were borrowed in full consciousness from Political Economy. Darwin was much struck by the importance of "division of labour." He did a not inconsiderable amount of work to show how organisms have in course of time become interlocked and inter-evolved. It only remained to obtain a clearer conception of the underlying economic sequence. Take the case of "capitalisation" as an instance. Nature's methods of "capitalisation" are not far different from man's, though they are much more thorough and far-reaching. Nature

employs the most ideally balanced media for "currency" and for "exchange" in the shape of surplus food substances; establishes the most perfect "tools" in the shape of enzymes and ferments; stores up wealth not only as capacities but also as valuable correlations in virtue of which counter-services are ever at the command of the beneficent organism.

In nature again, just as in human society, all currency has to have a redemptive basis (in work) and definite standards of useful morality have to be obeyed. The bee, so long as it remains a legitimate "trader," has a long "banking-account" with the "Flowers Bank Ltd.," and the plant, so long as it remains a manufacturer, working for "social" service rather than for mere profit, has a valuable "account" with the "Bee's Bank Ltd." All of which is at the same time typical of the true relation between plant and animal and shows the story of the animal as inevitably bearing the "brand of Cain" to be a pure myth. No partner in Symbiosis is really the poorer for what it is made to surrender legitimately. Darwin showed that a whole circle of calamities must ensue if the bee, for instance, gets its food feloniously. Organisms are not "villains on necessity."

Nunquam aliud natura, aliud sapientia dicit.

I. How is it that all this has not been perceived before?

W. Partly because of difficulties in connection with the "slippery basis of metabolism," partly because of Darwin's deliberate statement that we are as yet profoundly ignorant of the mutual relations of the inhabitants of the world, and partly because of dissatisfaction with and distrust of Political Economy.

I would only add that the question as regards the justification of the sub-title of my book may well remain open. Meanwhile my theory will prove useful, so I venture to think, to all those who are in quest of "causes" in Biology. Thanking you for the insertion of this communication, I have the honour to be, Sir,

Yours faithfully,

HERMANN REINHEIMER.

SURBITON,
November 4, 1915.

REVIEWS

MATHEMATICS

A Treatise on the Analytic Geometry of Three Dimensions. By GEORGE SALMON, D.D., D.C.L., LL.D., F.R.S., late Provost of Trinity College, Dublin. Revised by REGINALD A. P. ROGERS, Fellow of Trinity College, Dublin. [Pp. xxiv + 470, Sixth Edition, Vol. I.] (London : Longmans, Green & Co., 1914. Price 9*s*.)

A Treatise on the Analytic Geometry of Three Dimensions. By GEORGE SALMON, D.D., D.C.L., LL.D., F.R.S., late Provost of Trinity College, Dublin. Edited by REGINALD A. P. ROGERS, Fellow of Trinity College, Dublin. [Pp. xvi + 334, Fifth Edition, Vol. II.] (London: Longmans, Green & Co., 1915. Price 7*s*. 6*d*.)

THE last edition of this work which appeared in one volume under the name of George Salmon alone was the fourth. Mr. Cathcart, indeed, took the work of revision almost entirely off Salmon's hands, so that the third edition was practically the last edition at which Salmon worked. Mr. Rogers undertook the task of so adding to Salmon's classical treatise as to make it a concise and comprehensive survey of algebraic and differential Euclidean geometry of three dimensions. In order to avoid delay it was thought advisable to publish the new edition in two volumes, and accordingly the first volume of the fifth edition appeared in 1912. A new edition of this volume became necessary in 1914, and the second volume of the fifth edition appeared in 1915. It is these two volumes which are reviewed here.

The sixth edition of the first volume only differs from the fifth by a few corrections, and it may be convenient to summarise here the additions to both volumes of the fifth edition. It is, of course, superfluous to dwell on the merits of Salmon's book; and, as regards Mr. Rogers's additions, which are enclosed in square brackets as all additions ought to be, this summary, which is taken from the two prefaces, is all that is needed. The additions include illustrations of models of most of the different species of quadrics, with generators or lines of curvature, and notices on the analytical classification of real quadrics, on projection and Fiedler's projective co-ordinates, on the non-Euclidean theory of distance and angle, and on the expression of twisted cubics and quartics by rational or elliptic parameters. In differential geometry Mr. Rogers's aim has been to form a closer connecting-link between Salmon's book and the more extensive and more purely analytical methods used by Bianchi, Darboux, and others; and he has therefore added articles on the Frenet-Serret formulæ, on the intrinsic equations of a twisted curve, on Bertrand curves, and on the application of Gauss's parametric method to conformal representation, geodesic curvature, and geodesic torsion. To the portion dealing with the differential geometry of curves on quadrics, Mr. Rogers has added Staude's thread-construction for ellipsoids, which is the three-dimensional analogue of Graves's theorem; and his definitions of confocal quadrics, which are the analogues of the ordinary definitions of conics by means of focal radii. The rest of the new matter in the first volume is of the nature of commentaries. The

second volume begins with Chapter XIII., on partial differential equations of families of surfaces. This chapter has been divided into three chapters, and the last two of these chapters deal respectively with rectilinear complexes, rectilinear congruences, and ruled surfaces; and with triply orthogonal systems of surfaces and normal congruences of curves. These chapters contain much new matter. There is also much new matter added to the subsequent chapters dealing with cubic and quartic surfaces. The Chapter XVII. of the fourth edition was on the general theory of surfaces, and this has been subdivided into two chapters, and much of the chapters has been rewritten or added to. In this second volume Mr. Rogers has had the very active co-operation of Mr. G. R. Webb, Miss Hilda P. Hudson, and Mr. Robert Russell. Mr. Rogers has adopted the convenient plan—rare in books printed in the English language—of providing two indexes, one for subjects and the other for authors.

PHILIP E. B. JOURDAIN.

The Theory of Proportion. By M. J. M. HILL, M.A., LL.D., Sc.D., F.R.S., Astor Professor of Mathematics in the University of London. [Pp. xx + 108.] (London: Constable & Co., Ltd., 1914. Price 8*s.* 6*d.* net.)

ALMOST all teachers agree that Euclid's treatment of proportion in the fifth book of his *Elements* is so difficult, both in form and matter, that it is quite unsuitable for purposes of teaching. Prof. Hill completely assents to this, but maintains that a treatment of the theory of proportion, which is valid when the magnitudes concerned are incommensurable, should be included in the mathematical curriculum. It must be remembered that Prof. Hill has a very large experience of teaching and is also author of an admirable *Contents of the Fifth and Sixth Books of Euclid's Elements* and three important papers on the Fifth Book in the *Cambridge Philosophical Transactions*. The third of these papers is noticed in this number in "Recent Advances in Science: Mathematics," but the other two were published some years ago. Prof. Hill arrived at the conclusion that, in addition to the difficulties arising out of Euclid's notation and out of the fact that Euclid did not sufficiently define ratio, two reasons could be assigned for the great difficulty of his argument. In the first place, the only two of the many definitions in the Fifth Book which effectively count are the test for deciding when two ratios are equal (fifth definition), and the test for distinguishing between unequal ratios (seventh definition). Further, Euclid takes the unnecessary course of deducing some of the properties of equal ratios from the seventh definition. In this book Prof. Hill only uses the fifth for this purpose. In the second place, Prof. Hill thinks that it is very probable that the two assumptions: If $A\,r\,B$, then $(A : C)\,r\,(B : C)$, where the relation r may be either $=$ or $>$, form the real bed-rock of Euclid's ideas, and that he first of all deduced his fifth and seventh definitions from these two fundamental assumptions (pp. vii–viii). In any case, the appearance of the above definitions at the beginning of Euclid's argument and without explanation presents grave difficulties to the student, which are avoided in this work. This work is a modification of Euclid's method, which requires for its understanding a knowledge of elementary algebra.

Prof. Hill remarks (pp. x–xi) that Chapters I.–IX. contain an elementary course and Chapters X. and XI. contain an advanced course. However, for those who are more interested in what the book contains than in what parts of it are suitable to be taught to whom, the following summary may be useful. The first three chapters introduce "magnitudes of the same kind" as undefined entities with which examples make us more or less familiar and of which characteristics

are given in essentials after Stolz, to whose *Allgemeine Arithmetik* Prof. Hill's work is greatly indebted. Then some propositions on their positive integral multiples are given; and the "ratio" of two such multiples of the same magnitude is (p. 12) *defined* as an integer or fraction. Chapter IV.: By a proof probably due to Pythagoras himself, it is shown that there are magnitudes of the same kind (the side and diagonal of a square) which are not multiples of the same magnitude. In such cases, can the magnitudes have a *ratio* to one another? (p. 19): "if so there must be numbers which are not rational" (p. 21). Prof. Hill is evidently more interested in the process of discovery of mathematical objects than in their logical nature. Logically, Prof. Hill's method is like first defining "foreigners" as Frenchmen and then asking if there are any non-French foreigners. But we all know how easily one can make fun of the "extension of the idea of number," which is described much as usual in Chapter V., and what important heuristic methods badly expressed have been used in this "extension." We can all, with a little sympathy, appreciate the value of the vague question: "Does anything exist which is not a *rational* number, which is nevertheless entitled to be ranked as a number?" (p. 24). And most of us will agree that "an argument which does not follow the course of discovery is frequently very difficult to follow" (p. viii).

Chapter VI.: Construction of a new theory of the ratio of commensurable or incommensurable magnitudes which satisfies the conditions: If $A\,r\,B$, then $(A:C)\,r\,(B:C)$, where the relation r is either $>$, $<$, or $=$. "Two ratios are said to be equal when no rational number lies between them" (p. 33); and the test for equal ratios is that, if $(A:B)\,r\,(p/q)$ then $(C:D)\,r\,(p/q)$, where r has the above meaning and p and q are unrestrictedly variable integers (p. 34). "Stolz's theorem" (p. 37) is that the condition obtained when $=$ is put for r is superfluous; and applications of this theorem in the method of exhaustions are dealt with in Chapter X. Chapters VII.–IX. give properties of equal ratios; Chapter XI. contains further remarks on irrational numbers, and includes a statement of the Cantor-Dedekind axiom; and Chapter XII. is a commentary on the Fifth Book of Euclid.

On p. x it is said that Dedekind acknowledged that he drew his inspiration especially from Euclid's fifth definition (of the equality of ratios) of his Fifth Book. This does not seem to be the case. In the passage referred to by Prof. Hill (Beman's translation of Dedekind's *Essays*, Open Court Co., p. 40), Dedekind merely remarks that the conviction that an irrational number is defined by the specification of all rationals that are less and all that are greater than it was, put in another way of course, at the bottom of Euclid's definition, and was the source of Bertrand's and others' considerations and of his own theory.

This is a book which should stimulate an intelligent student to research.

PHILIP E. B. JOURDAIN.

The General Theory of Dirichlet's Series. By G. H. HARDY, M.A., F.R.S., Fellow and Lecturer of Trinity College, and Cayley Lecturer in Mathematics in the University of Cambridge, and MARCEL RIESZ, Dr. Phil. (Budapest), Docent in Mathematics in the University of Stockholm. [Pp. vi + 78. No. 18 of the "Cambridge Tracts in Mathematics and Mathematical Physics."] (Cambridge: University Press, 1915. Price 3*s*. 6*d*. net.)

IT was primarily with a view to applications in the theory of numbers that Dirichlet first introduced into analysis the series called after him. The series are infinite sums in which each term consists of a coefficient multiplied by e raised to

the power of $(-\lambda_n s)$, thus defining functions of s. A number of important theorems on such series were proved by Dedekind, but both Dirichlet and Dedekind considered only real values of s. The first attempts to found a more general theory of such series were made by Jensen in 1884 and 1888 and Cahen in 1894. In this general theory, complex values of s were of course considered. The essential difference which distinguishes the general theory of Dirichlet's series from the simpler theory of power series is that, whereas the region of convergence of a power series is determined simply by the disposition of the singular points of the function which it represents, no such simple relation holds in the general case of Dirichlet's series. Such a series, convergent in a portion only of the complex plane, may represent a function regular all over the plane or in a region wider than the region of convergence. In fact, roughly speaking, many of the peculiar difficulties which attend the study of power series on the circle of convergence are extended, in the case of Dirichlet's series, to wide regions of the plane or even to the whole of it (pp. 9–10).

This extremely able tract is, owing to the war, issued without Dr. Riesz's help in the final correction of the proofs. This has given Mr. Hardy the opportunity of mentioning in the preface the value of Dr. Riesz's contributions to the book and the whole theory in general. Further, Mr. Hardy refers to his great debt to the writings and personal encouragement of Dr. Edmond Landau, and this partly explains the very touching dedication of this little volume. The subject of Dirichlet's series and their applications to the theory of numbers up to 1909 has been very fully dealt with by Landau in his *Handbuch der Lehre von der Verteilung der Primzahlen* of the above date. The volume consists of eight chapters and a very excellent bibliography of memoirs. This bibliography, though not professing to be exhaustive, seems to be so as concerns recent literature on the subject. After a short introduction (Chapter I.) we have a chapter (II.) on the elementary theory of the convergence of Dirichlet's series. The region of convergence is a half-plane, and the question of the convergence of the series on the line of convergence remains open in general. The series also has a half-plane of absolute convergence (pp 4, 8). The third chapter is on the formula for the sum of the coefficients of a Dirichlet's series, and the order of the function represented by the series. Up to this point only *convergent* Dirichlet's series have been considered; such a series defines an analytic function which may or may not exist outside the domain of convergence of the series. In analogy with the methods of summation used of oscillating series which have been used in the modern developments of the theory of power series, we have in Chapter IV. an account of the summation of Dirichlet's series by Riesz's "typical means," and in Chapter V. some general arithmetical theorems concerning typical means. In Chapter VI. we have a treatment of "Abelian and Tauberian theorems," and in Chapter VII. some further developments of the theory of functions represented by Dirichlet's series. Finally, Chapter VIII. is on the multiplication of Dirichlet's series.

PHILIP E. B. JOURDAIN.

John Napier and the Invention of Logarithms, 1614. A lecture by E. W. HOBSON, Sc.D., LL.D., F.R.S., Sadleirian Professor of Pure Mathematics, Fellow of Christ's College, Cambridge. [Pp. 48.] (Cambridge: at the University Press, 1914. Price 1*s*. 6*d*. net.)

THIS lecture was delivered some time before the celebration at Edinburgh in 1914 of the tercentenary of the publication of John Napier's (1550–1617) *Descriptio*; and this little book has a frontispiece reproduced from the steel engraving of

a painting of Napier given in Mark Napier's *Memoirs of John Napier of Merchiston* (1834), and contains a reproduction of one of the pages of tables in the *Descriptio*. There is not much that is new in this lecture, but it is pleasantly written and of some real scientific interest. Its object is to give an account, as concise as may be, of the conception of a logarithm in the mind of Napier, and of the methods by which he actually constructed his table of logarithms (p. 6). "Napier's conception of a logarithm involved a perfectly clear apprehension of the nature and consequences of a certain functional relationship, at a time when no general conception of such a relationship had been formulated, or existed in the minds of mathematicians, and before the intuitional aspect of that relationship had been clarified by means of the great invention of co-ordinate geometry made later in the century by René Descartes" (p. 7). "On the theoretical side, Napier's representation [of numbers and their logarithms] by continuously moving points involved the conception of a functional relationship between two continuous variables, where Stifel and others had merely considered the relationship between two discrete sets of numbers. This was in itself a step of the greatest importance in the development of mathematical analysis" (p. 45).

A short biography of that very extraordinary man John Napier is given, and his deep interest in theology as well as his interest in warlike inventions, agriculture, and magical practices are referred to. Also there is a reference (pp. 11–12) to his early mathematical work which was only published in 1839 by Mark Napier. Prof. Hobson supposes (p. 12) that Napier put aside his early mathematical work in order to devote himself to the discovery of means of diminishing the labour involved in numerical computations. Napier was "led probably by the circumstances of the time," for the second half of the sixteenth century was a time in which Continental mathematicians devoted a great deal of attention to the calculation of trigonometrical tables. There is strong evidence that Napier had fairly begun the great invention of logarithms in 1594 (p. 13). About the time of the publication of the *Descriptio* in 1614, Napier devised several mechanical aids for the performance of multiplications, divisions, and the extraction of square and cube roots, "for the sake of those who may prefer to work with the natural numbers." Napier published an account of these inventions in 1617. The *Descriptio* did not contain an account of the methods by which the Canon of logarithms was constructed; this was contained in the *Constructio*, published posthumously but written several years before 1614 (p. 15), and a short account of the contents of the *Descriptio* and *Constructio* is given on pp. 18–21. Coming closer to the nature of Napier's invention, the definition of a logarithm by the help of points moving on straight lines, as given in the *Descriptio*, is set forth on pp. 23–6. and Napier's method of calculating logarithms is described on pp. 27–8. The steps in the construction of the Canon are described, from the *Constructio*, on pp. 28–40. There are also interesting sections on the introduction of the decimal point by Napier (pp. 21–3), on the reception of the logarithms (pp. 16–18), on other tables of logarithms (pp. 40–3), and on Napier's predecessors and his one rival in the invention of logarithms, Jobst Burgi (pp. 43–7).

One point in all the expositions of Napier's work which the present reviewer has seen seems rather remarkable. It is the prominence given to Napier's kinematical definition of a logarithm. We know that Napier did not come upon his invention by kinematical means, and we can only suppose that he took to defining his logarithms in such a way for the sake of generality. It seems

that Napier was also influenced by the greater appeal to intuition afforded by the kinematical definition, and we must remember that fluxional ideas were not uncommon among the Scholastic philosophers and were not wholly due to Galileo, Roberval, Gregory of St. Vincent, Barrow, and Newton.

PHILIP E. B. JOURDAIN.

Indian Mathematics. By G. R. KAYE. [Pp. iv + 73.] (Calcutta and Simla: Thacker, Spink & Co., 1915. Price 2*s*. 6*d*.)

THIS is an excellent summary of what is known of ancient Indian mathematics. The orientalists of about a century ago tended to antedate Indian discoveries (pp. 1, 34). The history of Indian mathematics may be divided into three periods (p. 3): (1) The *Sulvasūtra* period with upper limit *c.* A.D. 200; (2) the astronomical period, *c.* A.D. 400–600; (3) the Hindu mathematical period proper, A.D. 600–1200. The first period (pp. 3–8, 46–7) is known by a name which means "the rules of the cord," and the rules treat of the construction of sacrificial altars. Their date has been variously fixed from 800 B.C. to A.D. 200; but as a matter of fact it is quite unknown. They have not a mathematical but a religious aim, no proofs of them are given, and there is in the presentation nothing mathematical beyond the bare facts. They relate to the construction of squares and rectangles, the relation of the diagonal to the sides, equivalent rectangles and squares, and equivalent circles and squares; and in them the Pythagorean theorem is stated quite generally and illustrated by a number of examples. It must of course be remembered that the Egyptians and Chinese were acquainted with the theorem much earlier even than 800 B.C. (p. 6). Later Indian mathematicians completely ignored the mathematical contents of the *Sulvasūtras*. The second period (pp. 9–13, 47) begins with the introduction of Western astronomical ideas, and the chief names associated with this period are Varāha Mihira and Āryabhata. Āryabhata's work contains one of the earliest records known to us of an attempt at a general solution of indeterminate equations of the first degree by the continued fraction process, and it may be considered as forming an introduction to the later somewhat marvellous development of this branch of mathematics in India (p. 12). The third period (pp. 14–26, 47–50) is characterised by the names of Brahmagupta, Mahāvira, Srīdhara, and Bhāskara, by the advanced treatment of indeterminate equations, the problem of finding rational right-angled triangles, and the perfunctory treatment of pure geometry. The question of our Hindu-Arabic place-value arithmetical notation (pp. 27–32) here appears. It would seem that the modern place-value system was probably not introduced earlier than about the ninth century. It seems to the reviewer that two facts are not sufficiently considered by most historians of mathematics: the Babylonians had place-value in their sexagesimal system; and surely the almost universal use of the abacus must have suggested the place-value in script. There is no evidence of the use of the abacus in India until quite modern times, and there is evidence that the notation was introduced into India by a right-to-left script (pp. 31–2). There seems to be evidence of an intimate connection between Indian and Chinese mathematics (pp. 38–41). Only later did the Hindus outstrip the Chinese in the development of indeterminate analysis. The mathematics of the Arabs seems, contrary to the usual opinion, to be practically independent of Indian influence, and shows a great advance on Indian work in all branches, except perhaps indeterminate analysis (pp. 41–3). The Arabs based their work almost wholly on Greek knowledge, and the Greeks may have influenced the Indians by way of China or Persia (pp. 44–5). There is an

excellent comparative chronology (p. 67, cf. pp. 32–3), a bibliography (pp. 68–70), some notes on Indian mathematicians (pp. 35–7), a collection of extracts from texts (pp. 46–50), and notable examples from the texts (pp. 51–66). The amusing example 40 on p. 59 should be compared with that on pp. 148–9 of the fourth edition of Mr. Rouse Ball's *Short History of Mathematics*.

PHILIP E. B. JOURDAIN.

Modern Instruments and Methods of Calculation: a Handbook of the Napier Tercentenary Exhibition. Edited by E. M. HORSBURGH, M.A., B.Sc., Assoc. M. Inst. C.E., Lecturer in Technical Mathematics in the University of Edinburgh. With the co-operation of the following committee: Herbert Bell, M.A., B.Sc., G. A. Carse, M.A., D.Sc., David Gibb, M.A., B.Sc., J. R. Milne, D.S. Convener: Prof. E. T. Whittaker, Sc.D., F.R.S. Honorary Secretary: Cargill G. Knott, D.Sc. [Pp. viii + 343.] (London: G. Bell & Sons, Ltd., and The Royal Society of Edinburgh, n.d. 6*s*. net.)

AS Dr. Knott has explained in his article in this quarterly for last October, this *Handbook* was prepared for the use of those who attended the Napier Tercentenary celebrations at Edinburgh in July 1914. It is only necessary here to give an account of the contents of this splendidly printed volume, which is of permanent value to the mathematician even from other points of view than that of historical interest. "The aim of the Exhibition," we are told in the Preface, "is to do honour to one whose influence on science has been singularly profound; partly by a display of relics, partly by indicating the scope of his work, but more particularly by tracing what may be considered as the development of his great achievement. The modern mathematical laboratory may look upon Napier as its parent." The first article in the volume is a reprint (pp. 1–16) of Prof. G. A. Gibson's article "Napier and the Invention of Logarithms" from the *Proceedings* of the Royal Philosophical Society of Glasgow, and is illustrated by portraits of Napier and some admirable views of Merchiston Castle. Other portraits of Napier, Babbage, and Sang are also contained in this volume. The other contents, in order, are as follows, where notes of more general interest are mentioned. First there is a catalogue of a loan collection of objects of antiquarian interest in connection with Napier's life and works, including some early portable sundials (there is a note by John R. Findlay on "Portable Sundials" on pp. 20–2), calculating machines, and various tables. Dr. Knott (pp. 38–47) gives a paper, reprinted from the *Proc. Roy. Soc. Edinburgh*, on Sang's tables; Herbert Bell and Dr. J. R. Milne give "A Working List of Mathematical Tables" (pp. 47–60); Dr. W. G. Smith writes a learned and interesting "Note on the Special Development of Calculating Ability" (pp. 60–8); F. J. W. Whipple writes an introduction to the exhibits of calculating machines (pp. 69–75); P. E. Ludgate writes on "Automatic Calculating Machines" (pp. 124–7); and T. C. Hudson writes on "H.M. Nautical Almanac Office Anti-Differencing Machine (pp. 127–31). Dr. Knott gives an abridged reprint of his well-known article on the Abacus from the *Transactions of the Asiatic Society of Japan* of 1886 (pp. 136–54); Dr. G. D. C. Stokes gives a useful summary about "The Slide Rule" (pp. 155–65), and this paper is followed by descriptions of various exhibits. The subjects of and exhibits of apparatus for integrating, differentiating, harmonic analysis, tide prediction, equation solving, plotting, and so on is dealt with in papers by Charles Tweedie, Dr. G. A. Carse and J. Urquhart, Dr. A. M. Robb, Dr. J. Erskine Murray, Edward Roberts, D. Gibb, and Dr. R. F. Muirhead (pp. 181–277). The Section on ruled papers

and Nomograms contains articles by E. M. Horsburgh, Schleicher and Schüll, and Prof. M. d'Ocagne (pp. 278-301). The Section on mathematical models (pp. 302-27) contains a short introduction by Prof. Crum Brown, and an article by Col. R. L. Hippisley on closed linkages. The other Sections contain lists of exhibited portraits and medals, and miscellaneous and late exhibits.

PHILIP E. B. JOURDAIN.

Descriptive Geometry for Students in Engineering Science and Architecture. By HENRY F. ARMSTRONG, Associate Professor of Descriptive Geometry and Drawing, McGill University. [Pp. iii + 125, with 114 figures. First edition.] (London: Chapman & Hall, 1915. Price 8*s*. 6*d*. net.)

THE arrangement, excellent figures and explanations in this book bear out the author's introduction to his preface—"The writer offers the contents of this text-book as the result of over twenty years' constant teaching of descriptive geometry . . ."—as it is apparent therefrom that the author is fully aware of the initial difficulties and the subsequent pitfalls of the student.

There is very grave danger that the scientific and fundamental side of the engineer's training will suffer very materially from courses and text-books specially designed "for engineers."

Mathematics, physics, chemistry, descriptive geometry, and even applied mechanics have all undergone the "potted" process for the engineering student, much to the detriment of his mental digestive powers.

In the present volume there is very little scope, outside the title, for criticism on this point. The subject itself is a vast one, and the author has simply selected those sections which are of direct value to the engineer and architect. For instance, "perspective projection" is included as being a section of great importance to the types of students named, but its treatment is broad and fundamental, and quite suitable for *anyone* wishing to study perspective. The same may be said of all the other sections, and even in the chapters on inter-penetrations of solids the author has not descended to the level of connecting-rod ends, but has confined himself to cones, cylinders, and spheres—a point of distinct merit.

In a book which has so much to recommend it, it is to be regretted that the price—8*s*. 6*d*. net—is such that the ordinary teacher will hesitate before selecting it as a text-book for his class, more particularly as it does not contain anything (examples excepted) beyond what is usually taken by the teacher himself.

There is one further point. The author has been teaching for twenty years. While this is a distinct asset when writing a text-book, it is no reason why the volume should not be up-to-date.

The opening chapter should have contained an introduction to projection in general. Projection, as such, should have been defined and illustrated, and the different kinds in *practical use* described in outline with particular reference to fundamental projection. Part I., Chapter I., is on "The Projection Planes," and describes orthographical projection. In no place in this opening section, however, is the student told that he is studying *one phase* of projection only and that phase orthographical. Other forms of projection are duly treated, but no idea is given that the various forms are closely related.

This point should certainly be corrected in subsequent editions, and the student given a broader outlook at the very beginning in the study of the "language of engineers," as drawing is sometimes termed.

J. WEMYSS ANDERSON.

ASTRONOMY

Stars of the Southern Skies. By M. A. ORR (MRS. JOHN EVERSHED). [Pp. xii + 92, with 5 illustrations.] (London: Longmans, Green & Co., 1915. Price 2*s*. net.)

THIS book has been written for those who know little about, but yet are interested in astronomy, and have no instrument for examining the stars except their own eyes with perhaps the addition of a pair of binoculars; and also in the hope that it may be of use to parents and teachers in answering some of the questions which children are always asking. It is a volume possessing all the interest for which we always look from Mrs. Evershed's pen (and here one is tempted to ask why it has been published under the author's maiden name). It is surprising how much information is contained within its slender compass. After descriptions of the forms of, and the brightest stars in the chief constellations, specimens are chosen from the stars of the southern sky in illustration of the generally accepted course of stellar evolution, of the relative distance and motion of the stars, of double and multiple stars, of new stars, of variable stars both regular and irregular, of star clusters of various kinds, of nebulæ and of the relation of the Milky Way to the stellar system. The southern sky possessing, as the author remarks, "the most beautiful part of the Milky Way, the two brightest stars in the sky, the finest coloured star cluster, the largest globular cluster, the brightest double star, the nearest of the stars, and the brightest of the large gaseous nebulæ," lends itself admirably to this mode of treatment. She might have added that, in Eta Argus, it possesses also the brightest of the "new stars" of the last century. To this truly astonishing star a special chapter is devoted, but no mention is made of the fact that it was discovered last year by Mr. Innes at the Union Observatory, Johannesburg, to be a double star. It is interesting to note that since this volume went to press, Mr. Innes has found yet another companion. In discussing also the short-period variables, and the cause of their light variations, no mention is made of the theory which accounts for these by means of pulsations in the atmosphere of a single star, a theory which avoids many of the difficulties of the various theories which presuppose a binary system, and which, on the whole, seems the most probable. Nevertheless, the whole field of descriptive astronomy is briefly surveyed, and many of the results arrived at by modern research are presented. The volume should stimulate the interest of many in astronomy, and inasmuch as the southern skies have been less studied than the northern, and so present an even wider field for amateur workers, some may be induced to commence work in one or other of the directions which Mrs. Evershed points out as being particularly useful and convenient for amateurs with but small instruments.

H. S. J.

The Stars and their Mysteries. By CHARLES R. GIBSON, F.R.S.E. [Pp. 248, with 19 illustrations and diagrams.] (The Science for Children Library. London: Seeley, Service & Co., Ltd., 1916. Price 3*s*. 6*d*.)

MR. GIBSON has a unique and well-deserved reputation for presenting to children the results of scientific research in a manner that is not merely interesting, but even attractive and fascinating. The present volume, although not so good as the best of his previous ones, yet fully maintains his reputation. No subject should appeal so strongly to the child's imagination as astronomy, if rightly presented, and for children of from nine to fifteen years of age we know of no book on astronomy more likely to achieve this aim and to awaken the child's interest.

The book is written simply, clearly, and lucidly, yet with never an inexact or slipshod phrase. The many striking analogies used in illustration are all true to fact, and, as was remarked of one of Mr. Gibson's earlier books, "facts have not been surrendered to make the book interesting, but the interest has been built out of the facts." The volume has been made as interesting as many a book of adventure by an imaginary trip which the author and his readers take together in a wonderful flying machine (reminiscent of Jules Verne) to see the moon and the various planets and what they are like. If we have any criticism to make of the treatment adopted, it is chiefly that it shows a slight want of balance, too large a portion of the book being devoted to our own solar system in comparison with that devoted to the stars in general, to nebulæ and to the other wonders of the heavens. Apart from this, the subject is well surveyed. We must confess, however, that we find the illustrations poor and disappointing. Astronomy is a subject which lends itself particularly well to numerous and good illustrations, and authors will find astronomers ready to assist them in this matter. Mr. Gibson's book would have made an even greater appeal to children had more care been taken in this respect. Some plans also of the best known constellations would have been valuable, for children's interest greatly increases when they are able to pick out different stars or constellations in the sky. In perusing the book, we have noticed a few slips which have entered, in spite of its general accuracy. On page 147 Jupiter is said to have nine moons, one of which has a retrograde motion, whilst on page 237 we are told eight; the correct number is nine, but two (including the recently discovered ninth satellite) have retrograde motion. The temperature of the sun is considerably understated, and the paragraph on the number of stars is poor. We mention these in no carping spirit, for Mr. Gibson has evidently been at some pains to avoid errors, and they in no way affect the general excellence of the book. We commend it to all parents and teachers and hope that it will be widely circulated amongst young people.

H. S. J.

PHYSICS

Surface-Tension and Surface-Energy and their Influence on Chemical Phenomena. By R. S. WILLOWS, M.A., D.Sc., and E. HATSCHEK. Reprinted from "The Chemical World." [Pp. viii + 80, with 17 illustrations.] (London: J. & A. Churchill, 1915. Price 2*s*. 6*d*. net.)

THE chemistry of colloids has, especially within recent years, assumed so great importance not only from a theoretical but also from a technological standpoint that more and more time and attention are being devoted to its study. There is more than one book available at the present time which deals with the subject of colloid-chemistry from what we may call the experimental scientific side, and there are several publications dealing exclusively with the technical applications of the subject, *e.g.* in reference to bleaching, dyeing, pigments, filtration, rubber, cement, soils, etc. The present volume has another purpose in view, namely to give as clear an account as may be of the fundamental physical concepts upon which the subject rests. Without such a groundwork, it is hardly likely that the research chemist will really grasp the significance of the phenomena with which he deals. It is obviously essential that with such a purpose in view the writing must be that of a physicist. The authors responsible for the work are eminently qualified to carry it out successfully. The only regret one has is that it has been deemed necessary to make the treatment so brief and in securing simplicity of presentation

to have made an almost too scanty use of simple mathematics. It is to be hoped that if the opportunity arises, a second edition may deal with the subject in a considerably expanded form. Thus one would have welcomed, for example, the simple deduction (by means of a thermodynamical cycle) of the fundamental relation of Gibbs which has been worked out by one of the authors but is not included in the present volume.

The subject-matter dealt with may be briefly summarised as follows. Chap. I.: Demonstration of the existence of surface-tension. Chap. II.: Relation between surface-tension and other physical properties. Chap. III.: Relation between surface-tension and certain chemical constants. Chap. IV.: Adsorption phenomena. Chap. V.: Electrical effects at surfaces and their influence upon capillarity.

The book can be recommended very warmly.

W. C. McC. Lewis.

CHEMISTRY

The Chemists' Year Book, 1915. Edited by F. W. Atack, M.Sc., School of Technology, Manchester University. [Two volumes, diary and pp. 914.] (London: Sherratt & Jones, 1915. Price 10s. 6d. net.)

Mr. Atack must be warmly congratulated on producing so excellent a substitute in English for the *Chemiker Kalender*, which up to the war has enjoyed, without opposition, the support of English-speaking chemists. The book is modelled on the German publication, and in the variety and scope of its tables and data it is no whit inferior to that much sought after Kalender. In fact, it contains several new features and tables of technological data more expressly suitable for the requirements of the English works chemist, and altogether there will be no excuse in future for the importation of a single volume of the publication from the home of Kultur.

The editor regrets that the initial difficulties experienced in setting up the numerous tables have made it impossible to complete, for publication this year, certain sections which it had been hoped to include. These, however, will appear in future issues, and all care is to be taken to revise the tables and other data in order to keep them up to date and as reliable as possible.

The book is issued in two pocket-sized volumes, of which Volume I., which contains the diary pages, is provided with a guard-flap. We might make the suggestion that the diary part be bound up separately from the rest of the volume and inserted by an elastic band, or placed in a suitable pocket in the cover. New diaries could then be issued yearly, so that the expense of purchasing a complete new book each year would be obviated.

The present price (10s. 6d.) is by no means too dear for the amount of information which is contained in the volumes, but many chemists will probably find it too expensive to purchase new copies each year, nor will the data become out of date so rapidly as to necessitate such a yearly renewal. With this suggestion, we heartily commend the new Year-Book to the support of all British chemists.

C. S. G.

Alcoholometric Tables. By Sir Edward Thorpe, C.B., LL.D., F.R.S. [Pp. xiv + 91.] (London: Longmans, Green & Co., 1915. Price 3s. 6d. net.)

This little volume consists of three tables whose completeness supersedes anything of a similar nature hitherto published. The scope and range of the data contained

in the tables is best shown by giving the headings and the extremes of the readings.

TABLE I.

Specific gravity in air at 60° F./60° F. or 15·6° C./15·6° C.	Percentage of ethyl alcohol.		Percentage of fiscal proof spirit.
	By weight.	By volume at 60° F. or 15·6° C.	
0·79359	100·00	100·00	175·35
0·9224	48·07	55·87	97·84
0·9998	0·10	0·13	0·23

TABLE II.

Indication of Sikes' hydrometer at 60° F. or 15·6 C.	Percentage of				
	British proof spirit.	American proof spirit at 60° F. or 15·6° C.	Alcohol by weight. (Germany.)	Alcohol by volume.	
				At 15° C. (France.)	At 15·6° C. (Tralles.)
	Over proof.				
A 0·0	73·5	198·2	98·2	98·9	99·1
13·0	55·7	177·9	84·2	88·8	88·9
	Under proof.				
64·8	9·3	103·5	44·2	51·5	51·7
99·8	99·6	0·5	0·2	0·2	0·2

TABLE III.

Indication of Sikes' hydrometer at 60° F. or 15·6° C.	Percentage of British proof spirit.	Indications of hydrometer of			
		Russia.	Holland.	Spain. (Cartier.)	Switzerland. (Beck.)
	Over proof.				
A 0	73·5	—	25·3	43·5	42·6
28	40·3	70·9	15·9	31·1	26·8
100	100·0	0	0	10·1	0·1

By means of Tables II. and III. it is possible to compare British fiscal spirit strengths with those of other countries. Cartier's aerometer is still used in South America, whilst official Italy and Austria-Hungary make use of Tralles' alcoholometer.

In a clearly written introduction the author surveys the various investigations which have been made from time to time, both for Governments and by private individuals, which form the basis of the data now published. The history and mysteries of proof, under-proof and over-proof British spirit are laid bare, and the various expedients of alcoholometry adopted by the Excise departments of other Governments are fully explained.

The book is indispensable to all persons connected with Excise, and from the scientific point of view it well deserves a place in every scientific library.

C. S. G.

The World's Supply of Potash. [Pp. ii + 47.] (London: The Imperial Institute, 1915. Price 1*s.* net.)

THIS timely pamphlet contains an account of all the more important sources of "potash," using the word as a general term for soluble potassium salts. It includes a description of the Stassfurt deposits in Germany, which before the war had an almost complete monopoly of the world's supply. Potash is pre-eminently important throughout the world as an artificial manure, and in Great Britain it is also employed to a lesser extent in glass, soap, and numerous other industries. In the form of chloride or sulphate, for the most part, about 90 per cent. of the total output is employed for fertilising purposes.

The shutting down of the German source of supply has made it imperative to seek out new sources or to develop those which in the face of German competition have heretofore had to take a backward place.

The pamphlet gives a very good descriptive resumé of all sources, animal, vegetable and mineral, at all likely to prove feasible. These comprise the recently discovered Tertiary deposits in Catalonia, Spain, which give promise when fully explored of rivalling the Stassfurt deposits; a small deposit in the Punjab, India; another at Atacama, Chile; old salt-lake beds and brines in the United States and India; sea-water; kelp from Scotland, Ireland, Norway, Japan, and the Pacific coast of the United States; ashes of wood, hedge-clippings and trimmings of sunflower stalks; residues of beet-sugar; wool-washings; nitre-earths from old village sites in India, Egypt and other countries; insoluble potash minerals (alunite, leucite, felspars, micas, etc.).

The most promising prospect of economic development seems to lie with the Spanish deposits and the kelp industry, more especially that of the Pacific Coast, where giant algæ occur which have been found to contain as much as 30 per cent of potash (K_2O). It is possible that similar algæ occur on the Canadian Pacific Coast, but as yet no steps have been taken to investigate. The increased production of potash in this country from kelp and other vegetable sources is now under serious consideration. Considerable attention appears to have been given by American companies to the production of manurial potash from the insoluble mineral sources. Brief outlines of the methods of extraction and working up are given where there has been any commercial production, and several references will be found to fuller sources of information.

C. S. G.

The British Coal-Tar Industry. Its Origin, Development, and Decline. Edited by WALTER M. GARDNER, M.Sc., F.I.C. [Pp. ix + 437.] With illustrations. (London: Williams & Norgate, 1915. Price 10*s.* 6*d.* net.)

THE test of war inevitably detects the weak spots in a nation's armour, and nowhere has this been more forcibly shown than in the case of the shortage of synthetic dyestuffs, drugs, photographic materials and the thousand and one other necessities of civilised life with which we have hitherto allowed Germany to provide us. There is perhaps no more romantic chapter in the history of civilisation than the rise and development of the organic chemical industries, where chemists have achieved victories over Nature that would with certainty have brought them to the stake for witchcraft a century ago.

Fascinating though this story of an industrial revolution has been, it has hitherto had no historian to recount it in this country, and those whose interests lay in that direction have had to delve into ponderous scientific works and in more or less inaccessible journals for their information.

Prof. Gardner therefore is to be warmly congratulated upon having collected together the present volume by combining a large number of important papers bearing upon the subject of the coal-tar industry from its inception in 1856 to the present day, so that all who are interested in the subject may have the facts ready to hand.

It is possibly more than a coincidence that the first and last papers in the book bear the honoured name of Perkin; the former is a lecture delivered by Sir W. H. Perkin in 1868 before the Royal Society of Arts on "The Aniline or Coal-Tar Colours," and gives a most interesting account of the beginnings, both scientific and technical, of the coal-tar industry.

One line in this paper cannot be too often quoted: "In fact the coal-tar colour industry is entirely the fruit of theoretical chemistry."

Again in the next paper, on "The History of Alizarin," Perkin remarks: "(the industry) is the fruit of scientific researches in organic chemistry, conducted mostly from a scientific point of view; and while the industry has made such great progress, it has in its turn acted as the handmaid to chemical science, by placing at the disposal of chemists products which otherwise could not have been obtained."

The papers which follow, reprinted from the Journals of the Royal Society of Arts, the Chemical Society, the Society of Dyers and Colourists and elsewhere, are arranged in chronological order, and afford a complete survey of the development of the industry abroad and its neglect at home.

From Prof. Meldola's paper on p. 133 we learn that as far back as 1885 British dyers were using almost exclusively German-made dyes. Throughout the whole series of papers one can see the same story reiterated again and again: the attention paid to scientific education and research work abroad, and their comprehensive and thorough-going neglect in Great Britain.

A very illuminating remark may be quoted from Prof. Green's paper on p. 201: "The English manufacturer has considered that a knowledge of the benzol market was of greater importance than a knowledge of the Benzol theory, and after the early but brilliant days in the infancy of the industry when . . . commercial progress and scientific investigation went hand in hand, but little encouragement has been given here to chemical investigators and discoverers. The control of the industry unfortunately soon passed into the hands of men who had no knowledge and absolutely no appreciation of the science upon which their business rested and, concerned only with getting the ultimate amount of present profit, discouraged all scientific investigations as a waste of time and money."

Other papers deal with the Patent Laws in relation to the industry; and last we find Prof. W. H. Perkin's Presidential address to the Chemical Society this year on "The Position of the Organic Chemical Industry." Here again the better utilisation of Science in our industries is insisted on, and in addition the Universities of Oxford and Cambridge receive a well-merited reprimand for their neglect of organic chemistry in the past.

If a word of criticism may be allowed, one might venture to suggest that a few more illustrations, such as that of the Greenford Green factory omitted from Sir W. H. Perkin's address on the history of Alizarin, and perhaps a picture or two of a German and a British dye factory, would considerably enhance the interest of the book, particularly for the non-technical reader.

One hopes also that it may later be possible to produce a somewhat cheaper edition, within the range of the pocket of a student, for the book is without doubt

one that should be read by every student of chemistry as well as by business-men, scientists, and politicians.

FREDERICK A. MASON.

Practical Organic and Bio-chemistry. By R. H. A. PLIMMER. [Pp. xii + 635, with coloured plate and other illustrations in the text.] (London: Longmans, Green & Co., 1915. Price 12*s*. 6*d*. net.)

THE present volume is in reality a new and enlarged edition of the author's "Practical Physiological Chemistry" which was intended primarily for medical students and dealt with organic and physiological chemistry from the point of view of animal physiology. The author's experience has, however, led him to believe that the book would be made more useful if its scope were extended, and to this end he has included new sections on organic chemistry and organic substances found in plants, together with methods used in more advanced work. In order to emphasise this change the title of the book has been altered. Although ostensibly a new edition, practically the whole of the text has been rewritten, for in its original form the book gave the impression of being written more especially for the use of the author's own students and all experimental details were in the imperative; the didactic tone has, however, been eliminated and the book is addressed to all and sundry, and may profitably be consulted by both student and teacher for study or reference.

No pains have been spared to make the book as complete as possible, and by a judicious rearrangement of much of the subject-matter, as well as by an alteration in the type, the book has gained greatly in clearness. In few sections is the complete change of treatment with resulting improvement more marked than in those dealing with enzymes and proteins. Many entirely new and valuable sections have been added, such as those dealing with the chemistry of leaf pigments, hæmin and related blood pigments, the functions of hæmoglobin and the analysis of blood gases, while shorter sections have been written on betaines, pyrimidines, iminazole derivatives, nucleic acids and hydroaromatic compounds, anthoxanthins and anthocyans; nearly all of these sections contain experimental details for the isolation and detection of representative members of the class of substance they refer to. The last chapter dealing with the analysis of tissues will prove especially useful to animal physiologists, who will find there compiled together the methods of estimating most of the substances likely to be of interest to them. A very excellent feature of the book is the thoroughly up-to-date character of the information it imparts; a great many of the methods described are not to be found in any other books and have been taken straight from the original papers. Altogether the author is to be congratulated upon writing a most interesting and valuable book which cannot fail to be received with acclamation by chemists in general and biochemists in particular.

P. H.

GEOLOGY

On Certain Channels attributed to Overflow Streams from Ice-dammed Lakes. By T. G. BONNEY, Sc.D., F.R.S., etc. [Pp. 44.] (Cambridge: Bowes & Bowes, 1915. Price 1*s*. net.)

PROF. BONNEY, with his life-long and intimate knowledge of the Alps, is well known as a critic of those who ascribe profound modifications of the earth's surface to the action of glacier-ice. As the leader of a group of geologists in

south-eastern England, to whom the features of the Chaix Hills of Alaska or the inland ice of Greenland still make small appeal, he is unable to believe that the British Isles have suffered from a serious ice-invasion. If such an invasion has occurred, the explanation given by Kendall, Lamplugh, Dwerryhouse, and others of certain dry channels cut in or across spurs of Britannic hills seems simple, logical, and conclusive. The melting of large bodies of ice must have produced, as in other countries, large effects, which present anomalous features if ascribed to ordinary river-drainage. If, however, the ice-invasion was not relative in importance to that which, in the same epoch, reached Central Saxony from Scandinavia, some other cause must be sought for these channels in our glaciated areas. This, we think, is a fair statement of Prof. Bonney's position, and he has, with admirable and characteristic thoroughness, examined the field-evidence over a large extent of country. The channels that are regarded by the supporters of the glacial view as among the youngest features of our scenery are held by Prof. Bonney to be truncated remnants of older drainage-systems, like the dry gaps of a folded country, beheaded by the recession of subsequent streams along the strike. It seems difficult to maintain this view in face of the freshness of many of the rock-walls down to the valley-floor—those of the Scalp near Dublin, for instance, which is cut in granite, and not, as here stated, in limestone. The great scarps cited by the author as antique features—those, for instance, of the Wetterhorn and the Dolomites—are surely very modern as regards their present faces, and are not buried in their own taluses because of the transporting power of the ice which surrounded them down to the human epoch. The latest description of the Dublin features is due, by the by, to J. R. Kilroe, following Lamplugh, and not, as Prof. Bonney suggests, to the present reviewer, who is, however, among the willing converts.

GRENVILLE A. J. COLE.

ZOOLOGY

The Mutation Factor in Evolution, with Particular Reference to Œnothera. By R. RUGGLES GATES, Ph.D., F.L.S. [Pp. xvi + 353, with 114 illustrations.] (London: Macmillan & Co., 1915. Price 10*s*. net.)

THE title of this book leads one to form a wrong idea of its contents, for it is really a detailed account of the mutations of Œnothera, with very brief references to mutations in general, and a short discussion of the part played by them in the evolution of new species.

De Vries, by his numerous studies and in particular by his work *Die Mutationstheorie*, has made Œnothera a classical plant to the biologist. At first received with scepticism, the repetition of his experiments by other investigators led to a more general acceptance of his results, and it seemed as if there was now an opportunity of investigating species in the making.

Dr. Gates was among the first to recognise the importance of an investigation of the cytological phenomena accompanying this mutation. Since his first paper on this subject in 1907 much further work has been done by himself and other workers, and the time is ripe for the appearance of such a volume as the present, in which the various results are collated and reviewed. The book therefore is useful, but in our opinion there is still room for a work dealing with mutations in general.

A well illustrated and succinct account of the various species of Œnothera, their distribution and early cultural history, forms a useful introduction to the remaining chapters. The various mutants of *Œ. Lamarckiana* are fully dealt with, and one

cannot fail to be impressed with what, for want of a better term, may be called the instability of the species. Races from different parts present considerable differences, and mutation constantly occurs in the general population. The chapter on the cytological basis of the mutants is very interesting and reveals the fact that various mutants have different numbers of chromosomes ranging from 14 to 28. It is noteworthy that the number 28 occurs in *Œ. gigas* and certain of its offspring, and it almost appears as if the large size of the plant is the result of the possession of the double number of chromosomes. A whole series of the mutants appears to result from an irregular meiotic distribution of the chromosomes. Indeed, in general the mutants in Œnothera appear to be very closely related to the number of chromosomes in their nuclei and often produced by irregularities in their distribution, such irregularities occurring during the reduction mitoses. This bears out what we meant by the instability of the species, for it seems as if the equilibrium of the nucleus can be upset fairly readily. The Mendelians have claimed that mutation is but another exemplification of Mendelism; but we think the author is justified in claiming that it is quite distinct. The two phenomena of hybridism and mutation are, of course, intimately related, and although the former sometimes seems to lead to an increase of the latter the two are nevertheless distinct.

After a brief account of the kinds of mutations the author concludes: "As regards the ultimate nature of mutations, we are therefore inclined to look upon them as the result of various types of change in the nucleus—(1) morphological changes (*a*) in number, (*b*) in shape and size of chromosomes, or in the arrangement of their substance; (2) chemical or functional changes in (*a*) whole chromosomes or (*b*) portions of particular chromosomes, by which a function may be modified or lost; (3) two simultaneous mutations may occur through mismating of the chromosomes in two pairs, so that each germ-cell receives both members of one pair; (4) changes may perhaps occur in the mysterious karyolymph or gel which forms the groundwork of the nucleus." The Mendelians come in for a certain amount of well-deserved criticism, for, as the author remarks apropos of their constant attempts to express almost everything in terms of the one hypothesis, "the truth is that Mendelism is a theory of inheritance, and as such is not adapted to deal with the question of origins at all."

It is, on the whole, a clearly written, interesting, and well-illustrated book.

C. H. O'D.

A History of British Mammals. By G. E. H. BARRETT-HAMILTON and M. A. C. HINTON. [Parts XV. and XVI., 1914, and Part XVII., 1915.] (London: Gurney & Jackson. In parts, 2*s.* 6*d.* net.)

THE previous part (XIV.) of *A History of British Mammals* contained a notice of the death of E. Wilson, the friend of the author and artist responsible for many of the useful plates that illustrate the work. He was with Captain Scott on his ill-fated expedition to the Antarctic regions. Sadly enough, the next part (XV.) contains a notice of the death of Major Barrett-Hamilton himself while on a scientific inquiry into the whale fisheries of the Southern seas. It was fortunate that Mr. M. A. C. Hinton of the British Museum had worked in close association with Barrett-Hamilton and was able to carry on the work so tragically interrupted. These three parts indicate clearly enough that the very high standard set by the previous parts will be maintained, and a more uniform result will be obtained than might otherwise have been expected. They deal with British mice; and although perhaps one may deplore the multiplication of subspecies and varieties,

it is impossible not to realise that this group of our mammals has never before been so exhaustively and conscientiously treated. Although dealing thoroughly with the scientific aspect of the various problems involved, there is also a great store of valuable information regarding the habits, habitat and bionomics of the various forms.

The series, when complete, will undoubtedly form the standard book of reference on the mammals of the British Isles, and one, moreover, that was needed and will not soon become obsolete. It is well illustrated and a book that reflects great credit on the perseverance and knowledge of its authors.

C. H. O'D.

Flies in Relation to Disease: Bloodsucking Flies. By EDWARD HINDLE, B.A., Ph.D. [Pp. xv + 398, with 88 illustrations.] (Cambridge Public Health Series. Cambridge: at the University Press, 1914. Price 12*s*. 6*d*. net.)

THIS work, as its title suggests, is a companion to that recently published by Dr. Graham Smith on the non-bloodsucking flies, and with it summarises our present knowledge of the subject specified by the general title "Flies in Relation to Disease."

After a short introduction, relating more especially to the modes of transmission of pathogenic organisms by biting flies, follow chapters on the structure, biology, and classification of the Diptera. These are accompanied by a list of those species known to transmit infection arranged systematically, and showing the disease or diseases transmitted, the general distribution of the carrier, and the authority responsible for the incrimination. Each family is then treated separately and accounts of its structural characters, bionomics, and classification are given; useful synoptic tables of the species comprising the more important genera are also included. The description of the diseases follows as far as possible that of the family concerned in their transmission, and at the end of each chapter a list of the more important references to the subject under discussion is provided. Owing to the great importance of mosquitoes and mosquito-borne diseases and the vast amount of work performed in this connection, it is only to be expected that the consideration of this part of the subject occupies much space. In view of the confusion existing in regard to the classification of the Anopheline mosquitoes, a systematic tabulation of the known species only is given. For this purpose the scheme proposed by Christophers is used and the tribe thereby divided into groups of species which correspond to certain of the older "genera." To arrange these insects in a comparatively simple and satisfactory manner is certainly a difficult matter, and although the method adopted is of much interest in displaying natural affinities, it is scarcely suitable for a book of this nature and possibly may confuse rather than enlighten the student. The bloodsucking members of the family Muscidæ and their connection with medicine are dealt with at some length, and that important group the Tsetse Flies (*Glossina*) receives considerable attention. A synopsis of the species of these flies is provided, but is unfortunately hardly up to date, as both *Glossina nigrofusca*, Newst., and *G. severini*, Newst., are omitted; of these the former was described in December 1910, the latter in June 1913. Moreover, *Glossina austeni*, Newst., is still retained in the *G. palpalis* group of Tsetses, whereas in spite of the hind tarsi being entirely dark the morphological characters of the male genitalia show clearly that it is related to *G. morsitans*, West. In general, however, errors are comparatively few, although a somewhat serious one occurs on p. 223, where mention is made of *Pulex irritans* as the cat

flea, and *Ctenocephalus canis*, the dog flea, is once again given the name *Pulex serraticeps*!

The volume is well illustrated and produced and should prove of value to the various classes of readers to whom it is intended to appeal. In the present work, however, it is perhaps unfortunate that the author has adhered so strictly to the limitations implied by the title, as in this way certain groups of voracious bloodsucking flies—not yet proven disease carriers—are excluded.

H. F. C.

Handbook of Medical Entomology. By W. A. Riley, Ph.D., and O. A. Johannsen, Ph.D. [Pp. ix + 348, with 174 illustrations.] (Ithaca, New York: The Comstock Publishing Company, 1915. Price $2.)

The contents of this volume are based upon a course of lectures delivered at Cornell University during the last six years; more specifically the work is an elaboration of the senior author's *Notes on the Relation of Insects to Disease*, published in 1912. Its object is to provide a general survey of the subject, and especially to acquaint medical and entomological students "with the discoveries and theories which underlie some of the most important work in preventive medicine." The subject-matter is arranged more from the medical than the zoological point of view, and is primarily treated according to the ways in which the various species of noxious arthropods affect man. Thus the contents may be divided into four parts: arthropods which are directly poisonous (Chapter II.), parasitic arthropods (Chapters III. and IV.), arthropods as disseminators of disease (Chapters V.–XI.), and, finally, the more important morphological characters of the various groups and species presented in a series of synoptic tables (Chapter XII.). In each part, excepting the last, short historical accounts, descriptions of the diseases, and eradicative or remedial measures are included, and with those forms capable of disseminating pathogenic organisms the various modes of transmission are discussed.

Among the arthropods of a directly poisonous nature which receive attention are spiders, scorpions, stinging insects, and the poisonous haired larvæ of certain Lepidoptera, appropriately termed nettling insects; blood-sucking forms also are dealt with in this connection, but the majority receive more detailed treatment in other parts of the work, in view of their more malignant functions as parasites or disease carriers. Two types of parasitic arthropods are distinguished: the true parasites, *e.g.* mites, ticks, lice, fleas, etc., and the accidental or facultative parasites, which normally feed on decaying substances, but which, when accidentally introduced into the system, may exist there for a varying period. Of the latter certain kinds of fly larvæ are pre-eminent, and those most frequently encountered are discussed and their main structural and differentiating characters shown in a useful key. The later and more important phases of the subject, arthropods as disseminators or transmitters of disease-producing organisms, are next inquired into and necessarily occupy considerable space. The various groups concerned in this capacity are here classed and discussed with special reference to the manner in which the transmission of the parasites is effected—the simple carriers, the direct inoculators, and the essential hosts of disease germs. As typifying those forms acting in the manner first mentioned, a well-developed summary of the case against the house-fly is given, and the second or mechanical method of transmission by blood-sucking arthropods is illustrated by a few examples in which experimental proof has been obtained, and by an interesting retrospective account of the rôle of fleas in the transmission of plague. Those

arthropods capable of acting as definitive or intermediate hosts of human parasites are discussed, as far as possible, in connection with the types of organisms transmitted. Accordingly we have our attention directed, *inter alia*, to insects and tapeworms, mosquitoes in their relations to filariasis, malaria and yellow fever, tsetse-flies and trypanosome diseases, ticks as carriers of Rocky Mountain spotted fever, ticks and lice as disseminators of the various spirochæte fevers, and lice as transmitters of typhus fever. Certain parasitic diseases of domestic animals and their carriers are also treated in this section, which is concluded by a chapter on "some possible but imperfectly established cases of arthropod transmission of disease," such as *Stomoxys* and infantile paralysis, *Simulium* and pellagra. Comparatively little information is given, in the body of the book, regarding the structural and diagnostic characters of the several groups and forms treated. These are, in the main, reserved for the previously mentioned synoptic tables, which, it is hoped, will also enable the reader to obtain a perspective of the relationships of the arthropods in general. This object will probably be attained, but whether the arrangement adopted will prove of real value in enabling the non-entomological student to identify his specimens seems doubtful. To use such tables with success, a certain knowledge of the external anatomy and of the terminology employed is indispensable; but the only sources of such information supplied are a series of figures depicting the more important taxonomic details of the main groups. A short appendix, which includes an extract from a paper published only a week or two before the appearance of the volume, and a bibliography extending to fourteen pages, conclude the work.

In view of the declared object of this handbook little criticism is necessary, but attention may perhaps be directed to a few points. In the table on page 303 only nine specific names of tsetse-flies are given, and of these one (*G. bocagei*) has long been recognised as invalid. Probably it is not essential that all the known species should be included, but a more judicious selection might have been made, as at least one important and easily determined species (*G. brevipalpis*) is entirely omitted. Again, on page 264 the rare tick *Rhipicentor bicornis* not only receives undue prominence, but is stated to occur in the United States—so far it has only been recorded from Africa. Misprints other than those mentioned in the errata list are comparatively few, but a distinctly important one, in view of the definitions preceding, occurs on page 175. It is stated that "a number of tapeworms are known to undergo their sexual stage . . ."—sexual should, of course, read asexual.

This volume should prove of decided value to students in the manner suggested by the authors, and as a summary of the more important advances made during recent years in the subject should be welcomed by those other classes of readers to whom it is intended to appeal.

H. F. C.

Report on Fishery Investigations in Bengal, Bihar, and Orissa, with recommendations for future work. By T. SOUTHWELL, A.R.C.Sc., Deputy Director of Fisheries, Bengal, Bihar and Orissa. Department of Fisheries, Bengal, Bihar, and Orissa, Bulletin No. 5. [Pp. iii + 87.] (Calcutta: The Bengal Secretariat Book Depôt, 1915. Price 6*d*.)

IT is doubtful whether the Indian fisheries are capable of improvement by the sudden introduction of European methods of fishing and fish culture. Although the Indian fisherman is generally more than semi-nude, one must not jump to the conclusion that he is a savage. To persons conversant with modern methods of steam fishing as practised in European waters his apparatus may appear primitive,

but it does not follow that it is not adapted to local requirements. It may be true, as a common Bengal proverb states, that "*Nikarir kane sona, Jaliar parane tena*" (A middleman wears gold earrings and a fisherman wears rags), but the fisherman of Bengal is not unique in being the subject of exploitation.

We do not underestimate the difficulties of fishery administration and research in Bengal, and we sympathise with Mr. Southwell in the problems he has encountered and still has to encounter. If we venture on criticism it is solely with a view of assisting the Fishery Department of Bengal, Bihar, and Orissa.

The improvement of the fish supply of the provinces, which is held out as one of the main objects of the Department, is rather to be sought for in the encouragement of the local fisherman than in the introduction of foreign methods of fishing. Steam trawling, to quote one instance, is a profitable method of fishing in European waters where the chief demersal fish are Gadoids and Pleuronectids. In the Bay of Bengal the former family is absent and the latter family poorly represented from a commercial standpoint. Other demersal fish are present, but it by no means follows that they are adapted to the requirements of the local markets. The native of Bengal has been accustomed to fresh-water or estuarine species, and "*Meta na hai*" (It is not sweet) is his comment on marine fish. The great problem to be solved for the improvement of the fish supply of the province is that of transport, and for this there are two essential factors: a cheap supply of ice and rapid land and water carriage.

The introduction of European or American methods of fish culture without previous survey of the spawning habits of indigenous fish is also to be deprecated. One is glad to notice that Mr. Southwell has grasped the main problem, which is the tiding over the period of enormous mortality ensuing between the deposit of the spawn and the fingerling stage and the distribution of the fry so saved. In all probability this will be possible without elaborate plant or buildings. Naturally there are considerable physical obstacles in a province which during the rains is one vast swamp from the hills to the sea, but we hope the Department will be successful in overcoming these obstacles, in adopting a rational system of saving the fry, and in distributing them to localities where their growth can be made with profit to the purchaser. Here again one of the great problems to be solved is that of transport, as once the railways are left the difficulties of carriage of such delicate objects as young fish are by no means to be underestimated.

It is impossible to touch on many of the interesting points raised by Mr. Southwell in his report. Reference, however, should not be omitted to the valuable scientific investigations into the fisheries which have been inaugurated largely, it must in fairness be stated, through the energy and enthusiasm displayed by Dr. Annandale, the Superintendent of the Indian Museum at Calcutta. Without a close co-operation between the Museum authorities and the Fishery Department further progress will be impossible, and one is glad to note that Mr. Southwell refers appreciatively to the assistance he has received from the Museum staff.

There is one matter about which we may be permitted to utter a word of warning, and that is "Fishery Legislation." The report states (p. 53) that "Up to the present there are no fishery laws operative in Bengal," though it is to be presumed that the (Indian) Fisheries Act of 1897, which forbids the use of explosives and poisons for the destruction of fish, can be enforced if necessary. Apart from this Act we feel convinced that no case has yet been made out for the enactment of any restrictions as to fishing in Bengal, Bihar, and Orissa, and it is to be hoped that years of research into the habits of the indigenous fish will precede any attempt of the kind, and the mistakes made at home be avoided.

In conclusion one feels justified in congratulating Mr. Southwell on an interesting résumé of the past history, present position, and future prospects of fishery investigation in the two provinces, and it is to be hoped that the Government will have the courage to vote sufficient funds to enable future work to be conducted in an efficient manner. At the same time it is to be feared that the real solution of the difficulties in the way of the improvement of the fisheries of India is the substitution of Imperial for provincial control.

J. T. JENKINS.

Thirty-third Annual Report of the Fishery Board for Scotland, being for the year 1914. [Pp. lxviii + 303.] (Edinburgh: H.M. Stationery Office, 1915. (Cd. 7976.) Price 1*s.* 8*d.*)

THE annual report of the Scottish Fishery Board is divisible into three parts: a general statement of the condition of the sea fisheries, a similar statement as to the salmon fisheries, and an account of the scientific investigations carried on with respect to the fisheries. The report under review contains a summary only of the scientific investigations; for the detailed reports reference should be made to Fisheries, Scotland, Scientific Investigations, 1914. The individual scientific papers are now published separately when ready. The annual report of the Scottish Fishery Board has long been recognised as the foremost in the world, and there is no falling off in the present volume. The method of summarising and presenting the statistical review of the fisheries is as near perfection as possible, and one notices many important statistical returns (*e.g.* the statistics of cured fish and the cured fish trade, and the valuation of the fishing fleet and gear) which are omitted from the annual reports of the Board of Agriculture and Fisheries. The present report is of greater interest than usual, since it shows the effect of the war on the fisheries of Scotland up to the end of 1914. It is, however, only proposed to deal here with the questions summarised under the heading of Part III., Scientific Investigations, which were carried on in 1914, as in previous years, under the supervision of Dr. T. Wemyss Fulton, Scientific Superintendent to the Scottish Fishery Board. The plaice-hatching operations were carried on as usual at the Marine Laboratory at the Bay of Nigg, Aberdeen, and as a result the number of fry liberated during 1914 amounted to 18⅔ million; the total from the year 1900 inclusive is now over 343 million. Considerable attention is now being devoted to the herring, and there are two papers on this important fish, one by Dr. Williamson entitled "A short Résumé of the Researches into the European Races of Herrings and the Method of Investigation," and an investigation into the herring fishery of Loch Fyne. This famous herring fishery is now at its lowest ebb, and it is to be hoped that the investigations will throw some light on the causes of the failure of this fishery in recent years, together with some suggestions for its improvement in the future. Other papers deal with fishery investigations in the North Sea, and these are summarised under various headings, *e.g.* Trawling Investigations, the Migrations and Growth of Fishes, and the Influence of Marine Currents.

There is also an interesting paper by Prof. D'Arcy Thompson on the mean level of the sea. This appears to us to be of more than passing interest, and since it is published separately (price 1*s.*), as indicated above, it should be obtained by all interested in hydrographic studies.

Finally, there is a summary of a paper by Dr. Bowman on the spawning of the plaice, and the distribution of the eggs in the northern parts of the North Sea. The whole of the scientific work done under the auspices of the Scottish Fishery

Board is, as usual, of a thoroughly sound and practical nature, and now, as ever, forms a model on which other work of the kind might well be based. So much of the work done with the aid of State grants for the study of fishery problems is devoted to subjects of remote interest to the fisheries, that one is glad to see the Scottish Fishery Board devote their energies to subjects of real importance.

J. T. JENKINS.

AGRICULTURE

The Spirit of the Soil; or, An Account of Nitrogen fixation in the Soil by Bacteria and of the Production of Auximones in Bacterised Peat. By GORDON D. KNOX, with a Foreword by PROF. W. B. BOTTOMLEY. [Pp. xiii + 242 with 17 illustrations.] (London: Constable & Co., 1915. Price 2*s*. 6*d*. net.)

THE *Spirit of the Soil* introduces us to a novel venture in scientific publication, for it presents the discoveries of the man of science as seen with the eye of the trained journalist. Mr. Knox has done his work well. He gives us a vivid account of Prof. Bottomley's discoveries. He describes in vivacious language the doings of the spirits of the soil—though why bacteria should be regarded as spirits is not patent. He champions with unfaltering staunchness the virtues of bacterised peat. In spite, however, of the excellence of Mr. Knox's "Boswell" to Bottomley's Johnson, we hope that this method of vicarious publication will not become fashionable among our men of science.

Readers of the daily press are already familiar with Prof. Bottomley's discoveries. Briefly, these discoveries may be stated as follow: Some kinds of peat when treated with certain bacteria give rise to large quantities of soluble humates. When the humating spirits are exorcised—by heat-sterilisation—the humated peat is used as a seed-bed for nitrogen-fixing bacteria, and after the latter have been at work for some time bacterised peat is produced. This preparation appears to have valuable manurial properties. So potent, indeed, are the effects of even small dressings of bacterised peat applied to plants that Prof. Bottomley was led to inquire how they are produced. He has reached the conclusion that the effects are due to the presence in the bacterised peat of "accessory food bodies," or, as he calls them, auximones. This discovery is of course of even greater importance than that relating to the manurial value of bacterised peat, for it necessitates the conclusion that modern theories of plant nutrition must be discarded or at least underpinned. The discovery of auximones and the fundamental part they play in the nutrition of plants will, if verified, invalidate the current view that plants may reach their full development on a diet of water, carbon-dioxide, and inorganic compounds of nitrogen, potassium, lime, phosphorus, and the like.

The discovery of auximones and their sovereign rôle in the nutrition of plants imposes on physiologists a pretty paradox. For, as is well known, the vitamines required by animals are derived from plants, whence it follows that the plant can supply this indispensable form of nourishment to the animal but cannot make supplies for itself.

When we survey the evidence on which these claims are based we are bound to admit that it offers strong testimony to the manurial value of bacterised peat. Many of the illustrations in Mr. Knox's volume show plants grown with and without bacterised peat, and the superior vigour of the former is most striking. We have ourselves seen not a few of the originals—grown at Kew under careful

observation—and can testify to the value of the evidence. But bacterised peat by no means always produces such marked effects, and indeed we have seen trials in which it has produced no favourable results at all. Wherefore, although there is positive evidence in favour of the high manurial value of bacterised peat, we cannot admit that the evidence is final and complete. Nor do we yet know what kinds of peat are susceptible of use for the production of bacterised peat. We believe that Prof. Bottomley has hitherto used peat moss litter as his raw material; but on this point we are without certain knowledge.

Any plant physiologist who has ever coaxed a minute seed, sown in pure sand and watered only with a mineral culture solution, to grow into a large flowering and fruiting plant, will be excused if he hesitates to accept the theory of auximones—even on the evidence presented by Mr. Knox, striking though that evidence may be. The truth is that existing biological methods of testing such claims as these are clumsy and treacherous, as all who have experience of experimental plots know to their cost. Many repetitions of the original experiments will require to be made before the claims of Prof. Bottomley are regarded by experts as established. In the meantime he would indeed be a hypercritical curmudgeon who denied that Prof. Bottomley has done a piece of work which holds out the promise both of benefiting horticulture and agriculture and of modifying profoundly our views on plant-nutrition.

FREDERICK KEEBLE.

The Essentials of Agriculture. By HENRY JACKSON WATERS. [Pp. x + 455 + xxxvi.] With numerous illustrations. (Ginn & Co., Boston, U.S.A., 1915. Price 5*s.* 6*d.* net.)

THE title of this volume raised hopes which the preface disappointed by stating that it is merely "for students who desire a practical working knowledge of the essentials of agriculture," and the practical farmer would probably reduce these to one only, namely, to make things pay. At the same time it is a book which any English farmer might peruse with interest, if only because its Transatlantic origin leads to a wide treatment in such matters as the relation of the farmer to the community at large; in discussion of labour in regard to machinery and cultivation methods, of man-yield and acre-yield, and so forth.

The illustrations are a good feature, though several might be much larger to do themselves justice. Some are almost startling to residents of the old country, such as the picture of a modern wheat-heading and -threshing machine drawn by thirty-three mules. Others are pictures of methods employed by native races in various parts of the world, shown to emphasise the contrast between backward and progressive races; those who know something of both, and of the labour-cost in the U.S.A., will perhaps be less impressed by the contrast. The authors—for the book is a joint production by many specialists—display a faithful belief in progress which might convince even a cynic, though occasionally they adduce evidence to the contrary, such as the statement that "dry-farming" (which was widely advertised a few years ago) is a very old practice of the Chinese.

A readjustment of the headlines in several places would facilitate the student's task; the spelling of "reenforce" on p. 97 seems to come from an unknown tongue; and the Egyptian cotton crop on p. 178 is stated at only one-third of its actual amount.

Accepting the view that an ordinary agricultural student can derive knowledge

as well as facts from a textbook of such wide scope, the authors are to be congratulated on the skill with which the compression has been effected, especially in the earlier pages, dealing with biology. The practical work suggested is simple, and should lead the student to think, while the view that the orchards, farms, and gardens of the neighbourhood should be his proper laboratory is worth noting.

L. B.

Oil Seeds and Feeding Cakes. ANONYMOUS, with a preface by WYNDHAM R. DUNSTAN, C.M.G., F.R.S. Imperial Institute Monographs on the War and New British Industries. [Pp. xxiii + 112.] (London: John Murray, 1915. Price 2*s.* 6*d.* net.)

THE present is the first of a series of small volumes dealing with colonial raw materials whose market has been disturbed by the war. The preface makes interesting reading, though one would have liked to hear more about the economic aspect of the proposed changes, especially with regard to cost of production and to the availability of technological knowledge in England.

The subsequent essays deal with copra, palm kernels, ground nuts, sesame seed, mowra seed, and the comparative values of the new feeding cakes. They bring together much useful information, though they lack critical value from the investigators' view-point through being unsigned. The manufacturer would prefer that more prominence should be given to the economic problems; it is remarkable that Germany should have taken more than half our British-grown copra, or one-third of the total crop of the world, but it would much enhance the value of subsequent volumes in the series if the why and wherefore of such a fact could be elucidated, concurrently with its statement. Some minor improvements might be made in the arrangements of the tables of exports and imports, which are at present a little confusing.

The subject is one of topical interest, now that people are discovering the virtues of "nut-butter" and other margarines, a great part of which supply can be derived from within the Empire, in the place of butter purchased from European countries.

L. B.

MEDICINE

Amœbiasis and the Dysenteries. By LLEWELLYN POWELL PHILLIPS, M.A., M.D., F.R.C.P., F.R.C.S. [Pp. xi + 147.] (London: H. K. Lewis, 1915. Price 6*s.* 6*d.* net.)

THE present interesting volume is the result of a praiseworthy attempt to compile a modern account of the various dysenteries due to amœbæ, bacilli, certain flagellates and ciliates, and Schistosoma. The author in his preface acknowledges the help received from the *Tropical Diseases Bulletins*, and the use made of them is very obvious in his text.

The book consists of twelve chapters, the first six of which deal with parasitic amœbæ and amœbiasis. The succeeding chapters relate to balantidian or ciliate dysentery, to flagellate dysentery due to Trichomonas, Tetramitus, and Lamblia, and to bilharzial dysentery, while the last three chapters are concerned with bacillary dysentery and its treatment. There is a bibliography of ten pages, but apart from references to work performed in the later years of the nineteenth century, and that summarised since 1912 in the *Tropical Diseases Bulletin*, much of the highly important work of the intervening period—more particularly that just prior to 1912—has been overlooked. Thus, the important memoirs of Hartmann and his

collaborators, of Noc and Greig and Wells on the peculiar dysenteric amœbæ found in India and Indo-China, and the well-known work of Craig on the parasitic amœbæ of man are omitted from the bibliography. Due prominence is not given to the important researches of Darling on the correlation of the dysenteric amœbæ.

The accurate diagnosis of parasites plays such an important part in modern medical practice that a feeling bordering on disappointment results from the perusal of the accounts of the structure and life-history of some of the causal agents of the various dysenteries discussed. More especially is this to be regretted when the clinical and therapeutical sections have reached such a high level. But most workers—and certainly those engaged in medical schools—will agree that such statements as the following are not conducive to progress. On page 84 it is stated that Trichomonas "in the active stage . . . is easily recognised by its active movement, the presence of a sort of tail and four flagellæ" (*sic*). Again, on page 88 we read that "*Lamblia intestinalis* is a double flagellate with four pairs of flagellæ" (*sic*). Doubtless, these lapses will be amended in a subsequent edition.

An instructive account is given of amœbiasis of organs other than the intestine. Attention is paid to the very important subject of liver abscess and its diagnosis. Hepato-pulmonary abscesses and their treatment are adequately explained. There are also interesting remarks on some of the less common amœbic infections, such as those of the spleen, brain, parotid gland, bladder, female generative organs, skin, and bones. The numerous treatments that have been tried in connection with amœbiasis are then discussed. The advantages and disadvantages of the use of emetine are set forth.

The chapters dealing with the dysenteries resulting from the presence of Balantidium, Lamblia, Trichomonas, and Tetramitus contain good accounts of the most recent successful treatments of the same. The author inclines to the view that Trichomonas is not pathogenic in the intestine, except in conditions of ill-health or in infancy.

The chapter on bilharzial dysentery is well and concisely written, as might be expected from one having the practical experience of the disease that can be acquired in the hospitals of an endemic area like Cairo. The treatment of this malady is difficult. It is recommended that the bowels should be carefully regulated by salines or liquid paraffin; astringent enemata may be useful, but surgical aid may be required to remove polypoid growths.

The author's account of bacillary dysentery is concise, yet full and up-to-date. He recommends injections of polyvalent serum as being very efficacious. Vaccine treatment having been instituted as a prophylactic, the author gives directions for the preparation of such vaccines.

The book possesses a good index. We regret, however, to notice the somewhat high price for such a small volume. We can understand the author's reluctance in omitting illustrations, but trust that, under happier conditions, the deficiency will be supplied in a subsequent edition. The book should prove useful to those in charge of military hospitals during the war.

F.

Lead Poisoning: From the Industrial, Medical, and Social Points of View. By SIR THOMAS OLIVER, M.A., M.D., F.R.C.P. [Pp. x + 294.] (London: H. K. Lewis, 1914. Price 5*s.* net.)

THIS is a publication in book form of a series of lectures delivered at the Royal Institute of Public Health. The name of the author is a sufficient recommendation of the accuracy of the information and of the practical utility of the book.

The volume commences with a description of the process of Lead Smelting, and of the manufacture of red and white lead. It is pointed out that the British miner is free from any danger of lead poisoning, but the trouble begins with the smelting of the ore. Not only the workmen, but even domestic animals which are exposed to the noxious fumes are liable to suffer. There are many interesting examples of cows, dogs, poultry, etc., rapidly succumbing to an acute form of the disease Herbage and flowers in the neighbourhood of the works may be destroyed.

There is an account of industrial lead poisoning, with special reference to the injurious emanations from freshly painted surfaces. Lead may not be the only harmful substance in paint. It is possible that carbon-monoxide gas, or certain aldehydic bodies given off as vapour, may produce symptoms resembling *Saturnism*. Again, turpentine, petroleum spirit, and benzene, employed in mixing paints, are liable to cause serious mischief.

The benefits to the workmen afforded by the Home Office regulations, which apply to the inspection of factories, have been particularly marked in industries in which lead is employed, and are likely to lead in the future to a still further reduction in the prevalence of lead poisoning. The statistics given in the book show the progressive diminution in the frequency of the disease. The question of the relative values of lead paints and leadless paints is discussed in detail, the difficulty in this respect being to meet the somewhat opposite requirements of the artisan and the sanitary authority.

Lead poisoning as it occurs in various trades—viz. among painters, in the manufacture of china and earthenware, in painting, plumbing, dyeing, glass-making, and tinning—is dealt with fully, the particularly dangerous department in each of these industries being clearly indicated. The author's experience in the potteries of Hungary makes good reading The ravages of lead are much worse there than in England. The workers themselves, and their wives and children, until comparatively recently, suffered terribly from its effects—paralysis, blindness, and insanity being rife.

The remainder of the volume is devoted chiefly to the medical aspect of the subject. The mode of entrance of the poison, the symptoms and complications of the disease, the pathological features, and the preventive measures and curative treatment are described in a concise and lucid manner. The dangers of lead in food and drinking-water are mentioned, and it is shown that in the latter case widespread suffering may be caused. It is, however, in the form of dust that lead is the most troublesome, since it can enter the body through the skin, the respiratory tract, and the mouth and alimentary canal. The author is of opinion that, even in cases where the dust is inhaled, the bulk of the poison first passes into the alimentary canal before entering the system. For this reason it is important that workmen should have a good meal in the morning, since, when proteid digestion is proceeding, the amount of lead absorbed is insignificant.

From the pathological aspect the *pros* and *cons* of basophilic granulation of the red blood corpuscles as a diagnostic sign of early plumbism are considered. Sir Thomas disagrees with the views of some British and Continental physicians who claim that it is of value. Individual sections of the book are devoted to the pathology and clinical features of the blue line on the gums, colic and constipation, headache, loss of vision, and muscular and nervous symptoms, etc. The frequency of a positive Wassermann reaction in lead poisoning is on a parallel with the close resemblance of saturnine pseudo-general paralysis to the condition found in general paralytics.

The physiology of the absorption, elimination, and storage of lead in the human body is described. The fact that females are more adversely affected by lead than males is pointed out, and the injurious effect on motherhood demonstrated.

A series of experiments, in conjunction with Prof. Bloxam, on the influence of lead injections on blood pressure, show that a general fall takes place, and the charts clearly illustrate this point. The preventive measures described should be thoroughly well known to employers in all trades where lead is extensively used. Attention is drawn to the use of what is called the "double-electrical bath treatment," with which the names of the author and Mr. T. M. Clague are associated. The book concludes with a compilation of Factory and Workshop Orders which relate to industries in which lead is employed.

The book is of a handy pocket size, and would afford interesting and valuable reading alike to the medical man, the technical expert, and the layman.

J. W. CROPPER.

GENERAL

Emma Darwin. A Century of Family Letters, 1792—1896. Edited by her daughter HENRIETTA LITCHFIELD. In 2 volumes. [Vol. I., pp. xxxi + 289, with 11 illustrations; Vol. II., pp. xxv + 325, with 8 illustrations.] (London: John Murray, 1915. Price 21*s*. net.)

THESE two pleasant volumes give us an insight into the home life and surroundings in which the Darwin family were brought up. When Charles Darwin married his cousin, Emma Wedgwood, three families that had long been friendly were bound more closely together by yet another tie. Charles was the grandson of Erasmus Darwin; his mother, Susannah Wedgwood, was the eldest and Emma's father, Josiah Wedgwood, the fourth child of Josiah Wedgwood the potter, and Emma's mother Elizabeth was one of the Allens, a family of landed gentry from the north of Ireland that had settled in Wales. The Wedgwoods and Allens were already closely connected by the marriage of two brothers of the former to two sisters of the latter.

The first volume consists, in the main, of letters of the Allens and Wedgwoods, which are for convenience termed the Maer letters, since they are mainly to do with the Wedgwoods of Maer, the family of Emma. Both families were large, and it must be admitted that but for the list of *dramatis personæ* given at the beginning it would be a puzzling task to follow the subsequent letters. With this aid it is not difficult, and one finds in these pages an intimate picture of intellectual, well-to-do middle-class life of nearly a century ago. In both families the men were firm and straightforward, the women sympathetic and vivacious, and together they formed a group keenly alive to the many interests of life and with a wide outlook. Of the Allens one married Sir James Mackintosh, a politician and remarkable conversationalist—in some opinions, including Darwin's, better than Carlyle or Macaulay—and another married Sismondi, the historian. Through the latter and Madame de Staël, with whom they were friends, they came into contact with the literary aristocracy at Geneva, one of the most remarkable associations of the time. Tom Wedgwood was, if not the inventor of photography, as has been claimed, one of the first to practise it; Hensleigh was a mathematician and philologist of no mean order; and John with Sir Joseph Banks were the co-founders of the Horticultural Society. Altogether they gathered round them a truly interesting circle of friends, and their letters gain an added value from the thumb-nail sketches of many

famous people of their time—Sidney Smith, Lady Romilly, Catalani, Von Humboldt, and de Candolle, to mention only a few.

Of the Wedgwood family at Maer Mrs. Henry Holland wrote in her Journal: "I never saw anything pleasanter than the ways of going on in this family, and one reason is the freedom of speech upon every subject; there is no difference in politics or principles of any kind that makes it treason to speak one's mind openly, and they all do it. There is a simplicity of good sense about them, that no one ever dreams of not differing upon any subject where they feel inclined." This view will be readily endorsed by the reader of these well-chosen letters. Before leaving this part one cannot help noting, in the light of recent happenings, the opinion that one of the Allens found prevalent in Paris in the autumn of 1815: "The women always spoke well of the English and otherwise of the Prussians, who they said took everything '*à point de l'épée.*'"

The first volume contains but few of Emma Wedgwood's letters, but the second, starting with her engagement to Charles Darwin, centres around her. In it her daughter, Mrs. Litchfield, has done the same for her mother as the son did for his father in *The Life and Letters of Charles Darwin.* The genealogical tables of the Allens, Wedgwoods, and Darwins that preface it are very useful, and make it easy to grasp the somewhat complicated relationships of the three families. It gives us a sympathetic account of a loving and much-loved mother and wife. Here, as in the previous volume, glimpses of famous people flash into its pages and form the subject of short paragraphs. Full though the domestic life at Down was, there is over it all a shadow of gloom, and throughout the book we find indications of Charles Darwin's constant struggle with ill-health. This gloom is to some extent relieved by his wife's unremitting care and affection. Much of his work was done, as we already know, in the intervals between his illnesses, but here we have revealed the fact that through it all he always found time for his family life. Things political must have been much the same in Darwin's time as now, for we find that he was an ardent Liberal, but "at the same time he often deplored the almost total lack of interest in science in the House of Commons."

Emma Darwin's character, as revealed by her letters, was an admirable one; she was very considerate and thoughtful, and the two on religion in particular, two of the most appealing in the collection, show that she felt deeply and was not lacking in courage.

Mrs. Litchfield has performed the arduous task of selecting and editing these family letters with zeal and taste, and the resulting volumes deserve much praise, and are worthy of the people with whom they deal. Throughout they command attention, and by their means the reader is privileged to enter into the pursuits, recreations, and thoughts of these noteworthy families. The scientific side of Darwin's life has long been known to the world, and in these books we have available an account of his home life, centred, as it should be, around his wife Emma, to whom he owed so much.

C. H. O'D.

Rifles and Ammunition, and Rifle Shooting. By H. OMMUNDSEN, G.M., G.C., and ERNEST H. ROBINSON. [Pp. xvi + 335, with 65 plates and many other illustrations.] (London: Cassell & Co., 1915. Price 21*s.*)

THE senior author of this book was killed in action shortly after its publication, thus realising a possibility which is quietly contemplated in the preface, where the

authors apologise for finishing during war a task begun in time of peace. There have been some good books written on the rifle, and this one, marking the end of an epoch, is a worthy companion to them.

It is divided into two parts, the first being historical, while the second deals with practical rifle shooting. The treatment of the history in chapters dealing alternately with the rifle and its ammunition, stage by stage, makes a very clear and connected account. The novel features of the second part are: a discussion of the simple but valuable device of "trajectory shooting," an examination of conventional ideas on the subject of "cant," and some indications of our new knowledge as to the military capacities of the rifle.

It is pleasant to read the authors' high opinion of the modern British service rifle as a weapon for field use, in spite of the design of the bearing-lugs and bolt. Their condemnation of its needlessly elaborate construction, which demands special machining, and so has not only delayed our supplies of rifles, but has cost us millions of pounds, is definitely expressed, though very briefly and quietly. The humour of many situations which have arisen during the steady conflict between the civilian rifleman and the official musketry expert is frequently indicated, and both parties obtain their share of credit. The illustrations are all excellent, from the delightful aperture-sight on an ancient cross-bow to the skiagraphs of bullets in flight. There is one bad misprint on p. 106.

In the hope that the surviving author may at some time bring out another edition, we venture to suggest that the deliberate omission of any account of range-finding devices is rather a pity, while an account of the variations in appearance of the "open" sights would be very useful indeed, with photographic illustrations. The definition of a colloid on p. 152 is scarcely satisfactory, and the complete novice might be hampered, in the chapter on "Simple Ballistics," by an omission to point out specifically that the relation between sight-adjustment and the consequent alteration of the point of impact on the target at any one range is a simple matter of triangles, depending on the ratio between the range and the distance from one sight to the other.

Though *Rifles and Ammunition* is not ostensibly a scientific treatise, we know of no better introduction to the subject for any scientist who wishes to learn the history, the art and the craft of the rifle, and to appreciate the various difficulties involved in any attempt at improvement. Lastly, it is a permanent memorial to a brilliant shot and gallant soldier.

L. B.

Pro Lithuania. A Monthly Review Published by the Lithuanian Information Bureau. No. 1, July 1915. [Pp. 32.] (Editorial Offices, 41, Boulevard des Batignolles, Paris, France. Annual Subscription, 8*s.*)

As the result of the great struggle now raging will be a complete alteration in the map of Europe, many small nations that have through various causes lost their nationality are hoping that this forthcoming readjustment will bring them autonomy. Among these figures Lithuania, a country often erroneously confused or identified with Poland. She is therefore endeavouring to gain a hearing in every State, and this little paper is the first issue of a new journal written in the English language. Most of us know so little of the country that the slight sketches contained in these pages awaken great interest, and we find appeal to our sympathies for this down-trodden race pathetic indeed. The journal contains a number of short articles, most of them devoted to the one end of enlightening the public on the

subject of the race, the history, and the woes of Lithuania. The article entitled "Lithuania and the Autonomy of Poland" shows clearly how impossible it would be to try to construct an autonomous Poland with Lithuania as an integral part; how the Poles and Lithuanians are distinct races that never have and never will amalgamate, and how advantageous it would be to restore the ancient country of Lithuania as it existed in the Middle Ages. Her country cut in two, half absorbed by Germany and half by Russia, oppressed by both, with Germany by far the greater culprit of the two, she still has managed to keep alive her language, her ambitions, and her hope of future salvation. At the present time the land is devastated by war, and fifty thousand of her sons are forced by the Germans to fight their own countrymen in the ranks of the Russian army—a sadder picture could hardly be. Every one in the interests of humanity should give some attention, if not support, to this new publication.

BOOKS RECEIVED

(Publishers are requested to notify prices)

A Course of Modern Analysis. An Introduction to the General Theory of Infinite Processes and of Analytic Functions, with an Account of the Principal Transcendental Functions. Second Edition. Completely Revised. By E. T. Whittaker, D.Sc., F.R.S., Professor of Mathematics in the University of Edinburgh, and G. N. Watson, M.A., Fellow of Trinity College, Cambridge, and Assistant Professor of Pure Mathematics at University College, London. Cambridge: at the University Press, 1915. (Pp. 560.) Price 18*s*. net.

Edinburgh Mathematical Tracts. London: G. Bell & Sons, York House, Portugal Street, 1915.

1. A Course in Descriptive Geometry and Photogrammetry for the Mathematical Laboratory. By E. Lindsay Ince, M.A., B.Sc. (Pp. xviii + 79.) Price 2*s*. 6*d*. net.
2. A Course in Interpolation and Numerical Integration for the Mathematical Laboratory. By David Gibb, M.A., B.Sc. (Pp. viii + 90.) Price 3*s*. 6*d*. net.
3. Relativity. By A. W. Conway, D.Sc., F.R.S. (Pp. viii + 43.) Price 2*s*. net.
4. A Course in Fourier's Analysis and Periodogram Analysis for the Mathematical Laboratory. By G. A. Carse, M.A., D.Sc., and G. Shearer, M.A., B.Sc. (Pp. viii + 66.) Price 3*s*. 6*d*. net.
5. A Course in the Solution of Spherical Triangles for the Mathematical Laboratory. By Herbert Bell, M.A., B.Sc. (Pp. viii + 66.) Price 2*s*. 6*d*. net.
6. An Introduction to the Theory of Automorphic Functions. By Lester R. Ford, M.A. (Pp. viii + 96.) Price 3*s*. 6*d*. net.

Is Venus Inhabited? By C. E. Housden, Member B.A.A. With Diagrams. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. 39.) Price 1*s*. 6*d*. net.

In this pamphlet the author puts forward some theories with regard to the physical condition of Venus, from a study of surface markings, and uses these to support the supposition that the planet is inhabited. Considering that neither a study of surface markings nor spectroscopic methods have been able conclusively to settle whether the rotational period of Venus is 23 hrs. 11 min. or 225 days, it is evident that the whole theory rests on the slenderest of evidence, and although such a fantastic structure of unproven suppositions may appeal to some minds, we cannot think that even the "broadminded considerations" for which the author pleads will gain it many adherents.

A Treatise on Light. By R. A. Houstoun, M.A., Ph.D., D.Sc., Lecturer on Physical Optics in the University of Glasgow. With 328 Diagrams. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. xi + 478.) Price 7*s*. 6*d*. net.

A Manual of Mechanics and Heat. By R. A. Gregory, Professor of Astronomy, Queen's College, London, and H. E. Hadley, B.Sc., Associate of the Royal College of Science, London; Principal of the School of Science, Kidderminster. London: Macmillan & Co., St. Martin's Street, 1915. (Pp viii + 309.) Price 3*s*. net.

Radium, X-rays, and the Living Cell. With Physical Introduction. By Hector A. Colwell, M.B. (Lond.), D.P.H. (Oxon.), late Assistant in the Cancer Research Laboratories, Middlesex Hospital, and Sidney Russ, D.Sc. (Lond.), Physicist to the Middlesex Hospital. With 2 Plates and 61 Figures. London: G. Bell & Sons, 1915. (Pp. x + 324.) Price 12*s.* 6*d.* net.

Monographs on Physics. Edited by Sir J. J. Thomson, O.M., F.R.S., Cavendish Professor of Experimental Physics, Cambridge, and Frank Horton, Sc.D., Professor of Physics in the University of London. London; Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. Relativity and the Electron Theory. By E. Cunningham, M.A., Fellow and Lecturer of St. John's College, Cambridge. With Diagrams. (Pp. v + 96.) Price 4*s.* net.

An Introduction to the Mechanics of Fluids. By Edwin H. Barton, D.Sc., F.R.S.E., A.M.I.E.E., F.P.S.L., Professor of Experimental Physics, University College, Nottingham. With Diagrams and Examples. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. xiv + 249.) Price 6*s.* net.

The Mathematical Analysis of Electrical and Optical Wave-Motion. On the Basis of Maxwell's Equations. By H. Bateman, M.A., Ph.D., late Fellow of Trinity College, Cambridge, Johnston Research Scholar, Johns Hopkins University, Baltimore. Cambridge: at the University Press, 1915. (Pp. vi + 159.) Price 7*s.* 6*d.* net.

Qualitative and Volumetric Analysis. By W. M. Hooton, M.A. (Oxon.), M.Sc., F.I.C., Chief Chemistry Master at Repton School. London: Edward Arnold, 1915. (Pp. 86.) Price 3*s.* net.

The Molecular Volumes of Liquid Chemical Compounds from the Point of View of Kopp. By Gervaise le Bas, B.Sc. (Lond.). With Diagrams. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. xii + 275.) Price 7*s.* 6*d.* net.

Manuals of Chemical Technology. Edited by Geoffrey Martin, Ph.D., D.Sc., B.Sc. London: Crosby Lockwood & Son, 7, Stationers' Hall Court, Ludgate Hill, E.C., and 5, Broadway, Westminster, S.W., 1915.

2. The Rare Earth Industry, Including the Manufacture of Incandescent Mantles, Pyrophoric Alloys, and Electrical Glow Lamps. By Sydney J. Johnstone, B.Sc. (Lond.), Senior Assistant, Scientific and Technical Department, Imperial Institute, London. Together with a Chapter on the Industry of Radioactive Substances by Alexander S. Russell, M.A., D.Sc., late Carnegie Research Fellow and 1851 Exhibition Scholar of the University of Glasgow. Illustrated. (Pp. xii + 136.) Price 7*s.* 6*d.* net.

3. Industrial Nitrogen Compounds and Explosives. A Practical Treatise on the Manufacture, Properties, and Industrial Uses of Nitric Acid, Nitrates, Ammonia, Ammonium Salts, Cyanides, Cyanamide, etc. Including the Most Recent Modern Explosives. By Geoffrey Martin, Ph.D., D.Sc., B.Sc., F.C.S., Industrial Chemist and Chemical Patent Expert, and William Barbour, M.A., B.Sc., F.I.C., F.C.S., Explosive Chemist. Illustrated. (Pp. xviii + 125.) Price 7*s.* 6*d.* net.

4. Chlorine and Chlorine Products: Including the Manufacture of Bleaching Powder, Hypochlorites, Chlorates, etc. With Sections on Bromine, Iodine, and Hydrofluoric Acid. By Geoffrey Martin, Ph.D., D.Sc., F.C.S., Industrial Chemist and Chemical Patent Expert. Together with a Chapter on Recent Oxidising Agents by G. S. Clough, B.Sc. Illustrated. (Pp. viii + 100.) Price 7*s.* 6*d.* net.

Text-Books of Physical Chemistry. Edited by Sir William Ramsay, K.C.B., F.R.S. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. The Theory of Valency. By

J. Newton Friend, D.Sc. (Birmingham), Ph.D. (Würz.), F.I.C., Carnegie Gold Medallist; Headmaster, Victoria Institute Science and Technical Schools, Worcester. Second Edition, Revised.

A Text-Book of Elementary Chemistry. By Alexander Smith, B.Sc. (Edin.), Ph.D. (Munich), Professor of Chemistry and Head of the Department of Chemistry of Columbia University. London: G. Bell & Sons, Ltd.; New York: The Century Co., 1915. (Pp. x + 457.) Price 5*s*. net.

Quantitative Laws of Biological Chemistry. By Svante Arrhenius, Ph.D., M.D., LL.D., F.R.S., Nobel Laureate, Director of the Nobel Institute of Physical Chemistry. London: G. Bell & Sons, Ltd., 1915. (Pp. xl + 164.) Price 6*s*. net.

The Birth-Time of the World, and other Scientific Essays. By J. Joly, M.A., Sc.D., F.R.S., Professor of Geology and Mineralogy in the University of Dublin. With 28 Plates and 15 Illustrations in the Text. London: T. Fisher Unwin, Ltd., Adelphi Terrace. (Pp. xv + 307.) Price 10*s*. 6*d*. net.

The Rural Science Series. Plant-Breeding. By L. H. Bailey. New Edition, revised by Arthur W. Gilbert, Ph.D., Professor of Plant-Breeding in the New York State College of Agriculture at Cornell University. Illustrated. New York: The Macmillan Company, 1915. (Pp. xviii + 474.) Price 8*s*. 6*d*. net.

Biology. By Gary N. Calkins, Ph.D., Professor of Protozoology in Columbia University. With Diagrams. London: G. Bell & Sons, York House, Portugal Street, W.C.; New York: Henry Holt & Co. (Pp. viii + 241.) Price 7*s*. 6*d*. net.

The Mosquitoes of North and Central America and the West Indies. By Leland G. Howard, Harrison G. Dyar, and Frederick Knab. Vol. III. Systematic Description (in Two Parts). Part I. Washington, D.C.: Published by the Carnegie Institution of Washington, 1915. (Pp. vi + 523.)

The Antiquity of Man. By Arthur Keith, M.D. (Aberdeen), LL.D., F.R.C.S. (Eng.), F.R.S., Conservator of the Museum and Hunterian Professor, Royal College of Surgeons of England, formerly President of the Royal College of Surgeons of England, formerly President of the Royal Anthropological Institute of Great Britain and Ireland. With 189 Illustrations. London: Williams & Norgate, 14, Henrietta Street, Covent Garden, W.C., 1915. (Pp. xx + 519.) Price 10*s*. 6*d*. net.

An Introduction to the Study of Prehistoric Art. By Ernest A. Parkyn, M.A., F.R.A.I., sometime Scholar of Christ's College, Cambridge. With 16 Plates and 318 Illustrations in the Text. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. xviii + 349.) Price 10*s*. 6*d*. net.

Bovine Tuberculosis in Man. By B. Stenhouse Williams, M.B., B.Sc., D.P.H., Research Bacteriologist in Dairying, University College, Reading, and E. H. R. Harries, M.D., B.Sc., D.P.H., Tuberculosis Physician, King Edward VII. Welsh National Memorial Association. Cambridge: at the University Press, 1915. (Pp. 14.)

The Cures of the Diseased. In forraine Attempts of the English Nation. London, 1598. Reproduced in Facsimile. With Introduction and Notes by Charles Singer. Oxford: at the Clarendon Press, 1915. (Pp. 34.) Price 1*s*. 6*d*. net.

Rural Sanitation in the Tropics. Being Notes and Observations in the Malay Archipelago, Panama, and other Lands. By Malcolm Watson, M.D., C.M., D.P.H. With Illustrations. London: John Murray, Albemarle Street, W., 1915. (Pp. xvi + 320.) Price 12*s*. net.

Geographical Aspects of Balkan Problems in their Relation to the Great European War. By Marion I. Newbigin, D.Sc. (Lond.), Editor of the *Scottish Geographical Magazine*. With a Coloured Map of South-Eastern Europe and

Sketch Maps. London: Constable & Co., Ltd., 1915. (Pp. vii + 238.) Price 7*s*. 6*d*. net.

Notes from the Bengal Fisheries Laboratory, Indian Museum. Nos. 2-3. By T. Southwell. Records of the Indian Museum. Vol. XI., Part IV., Nos. 16–17. (Pp. 24.) With 3 Plates. Calcutta: August 1915.

The first paper contains an account of some Indian parasites of fish, with a note on Carcinoma in trout. The second paper comprises notes on helminths from fish and aquatic birds in the Chilka Lake.

Modern Bullets in War and Sport. By C. Marsh Beadnell, F.C.S., Fleet-Surgeon, R.N. Reprinted from *Engineering*, August 6, 13, and 27, 1915. London: Offices of *Engineering*, 35 and 36, Bedford Street, Strand, W.C., 1915. (Pp. 16.)

The Positive Sciences of the Ancient Hindus. By Brajendranath Seal, M.A., Ph.D. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. viii + 295.) Price 12*s*. 6*d*. net.

The Scientific Australian, Vol. 21, No. 1. September 1915. Published Quarterly. (Pp. xvi + 26.) Price 2*s*. 6*d*. per annum.

ANNOUNCEMENTS

British Science Guild. Meetings, 3 p.m., January 25, February 22, March 28.

The Royal Society. Meetings, 4.30 p.m., January 27, February 3, 10, 17, 24, March 2, 9, 16, 23, 30.

Royal Astronomical Society. Meetings, 5 p.m., January 14, February 11.

Royal Meteorological Society. Annual General Meeting, January 19.

Chemical Society. Informal Meeting, 8 p.m., January 13. Meetings, 8.30 p.m., January 20, February 3 (with Lecture by Prof. W. H. Bragg) and 17, March 2 16.

Linnean Society. Meetings, 5 p.m., January 20, February 17, March 2.

Zoological Society. Meetings, 5 p.m., February 8 and 22, March 7 and 21.

The Institution of Mechanical Engineers. General Meetings, 8 p.m., January 21, March 17. Annual General Meeting, 8 p.m., February 18.

Harveian Society. Annual General Meeting, January 8.

Society of Tropical Medicine and Hygiene. Meetings, 8.30, January 21, February 18, March 17.

The Transatlantic Film Company, Ltd., 37-39, Oxford Street, W., have asked us to announce that on and after Boxing Night they will show at the Philharmonic Hall in London the Williamson Expedition Submarine Motion Pictures, said to consist of moving pictures of the Atlantic Ocean bed. This may be of interest to men of science.

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THE MATHEMATICAL THEORY OF ORGANIC VARIABILITY

By JAMES JOHNSTONE, D.Sc.
University, Liverpool

EVERY character of an organism, or of a part or organ of an organism, is variable. If, as is almost always the case, the characters studied are measurable, this variability can be treated mathematically; and even if the character is one which cannot be measured, its variability can still be investigated in a similar manner. In considering organic variation two series of variable values are formed: (1) the arbitrarily chosen values of the dimensions, position, colour, etc., of the variable character, and (2) the series of values representing the frequency with which each value of the variate occurs. The former series (following the usual terminology) is that of the argument, or independent variable, and the latter is that of the function, or dependent variable. Calling the values of the argument x-values, and those of the function y-values, we say that $y = f(x)$, that is to say, as x varies uniformly throughout a certain range of values, y also varies but not (in general) uniformly. There is a certain mathematical relationship between x and y which is expressed in saying that y is a function of x.

In very many cases the form of the function $f(x)$ can be found. Thus if we have a series of bodies of similar shape but of different diameters, and if we weigh all these bodies we shall find that the variation in weight can be expressed by the equation $W = ad^3$, W being the weight of any one of them, d its diameter, and a a constant. As a very general rule, however, no series of organic variates exhibits this physical simplicity. If we weigh a number of animals belonging to the same species, but all of which are of different lengths, we shall find that the weight is not proportional to the cube of any one diameter (say the length). If we make an empirically drawn curve representing the observed variation of weight with uniformly increasing length, and then try to fit to this curve a calculated one expressing the physical law $W = al^3$, we shall

not (generally) obtain agreement between observed and calculated functions. If we suppose that the increase of weight is proportional, not only to the cube of the length, but also to the square of the length, and to the length itself, we shall obtain a very much better agreement between observation and theory. It will then be possible to fit a curve, $W = a_0 + a_1l + a_2l^2 + a_3l^3 + \ldots$, to the empirical curve with a very close agreement. It is open to us to regard $a_0 + a_1l + a_2l^2 + a_3l^3 + \ldots$ as, in a way, an expansion of al^3, and to say that not only must we consider the first differential coefficient, but also the second and third derivatives; but it is likely, all the same, that increase in weight will depend on length and surface as well as on volume. The intensity of metabolism in an organism is, as we know, proportional not so much to the volume and weight, as to the surface; for the surface (all, or part of it) is the boundary through which metabolic exchange occurs. Therefore we find that the smaller an organism is, the greater, proportionally to its weight, is the rate of metabolism.

But even when we fit a parabola $a_0 + a_1l + a_2l^2 + a_3l^3 + \ldots$ to a series of measurements of the weights of similar animals of different ages, and obtain a very good correspondence between empirical and theoretical curves, we shall find that the parabola only describes this particular series well. Another series, as similar as possible to the first one, will be described by an expression containing significantly different coefficients a_0, a_1, a_2, etc. That means that the function $f(x)$ is really a much more complex one than we supposed. The function y is not one of a single variable, nor even of two or three variables, and we must write it $y = f(a, b, c, \ldots x)$. That is to say, the causes of variability in an organism are very numerous. And we must not even say that the variables, $a, b, c, \ldots x$, are independent of each other, for any one of them may be a function of some of the others. Further, it is generally impossible to find what are these variables on which the function depends, nor how they aree la ted to each other and to the function, because of the complexity itself of the relationships: the problem would be very similar to that of attempting to express the behaviour of (say) a cubic centimetre of a perfect gas in terms of the number of molecules in it, and of their velocities, directions of movements, and

masses. So long as we know absolutely nothing (in the physical sense) of the causes of organic variability, so long must the expression $f(a, b, c, \ldots x)$ remain a quite general one really concealing our ignorance.

The mathematical description of organic variability must therefore be approached from another point of view. Let the following two series of values represent a series of observations of organic variation :

Y	5	42	63	50	30	8	2	0	0	0	0
X	0	1	2	3	4	5	6	7	8	9	10

The independent variable X represents a series of values of the measurable character X of an organism. These values have their mean between 2 and 3. The dependent variable Y represents the number of times, or *frequency*, with which the values 0, 1, 2 . . . 9, 10 were observed : that is, there were 5 individuals giving a value of the character X equal to 0, 42 which gave a value = 1, 63 which gave the value 2, and so on. The whole series is called a frequency distribution, expressing the observed frequency of occurrence of the various values of the variable character X. Suppose now that we had an indefinitely great number of individuals which we can measure, and suppose that we measured 50, then 100, then 200, then 400, and so on. In each case we should obtain a rather different frequency distribution, but as the number of individuals measured became greater, we should find that the form of the distribution would tend to become "steady." It is sometimes said that when the number of measurements made is "infinite" the distribution takes a definite form—which is rather to be regarded as nonsense. *As the number of individuals becomes greater and greater the form of the distribution tends towards a limit.* In this particular case the limit is :

X	11.26	37.54	56.32	50.05	29.16	11.68	3.24	0.62	0.08	0.00	0.00
Y	0	1	2	3	4	5	6	7	8	9	10

neglecting the decimal figures after the second one. This is an evaluation of the series represented by $y = f(a, b, c, \ldots x)$

obtained by an application of the theorems of probability. It is only the *most probable* series of frequencies, for each particular one is subject to a certain error capable of being found for each working degree of probability of result. This limitation of a numerical conclusion as to organic variability must be noted *very* carefully. The frequency 50·05, for instance, is obtained for the value of the independent variable, 3. In a series of 200 measurements 3 occurs 50 times, that is, it will form about 25 per cent. of the cases in an indefinitely large series of observations. Now a conclusion expressible numerically in biometric work is only probable, and the degree of probability can be anything we like: it can be as 1 is to 1, as 2 to is 1, as 3 is to 1, and so on. We fix the working degree of probability; say we call the odds that our conclusion is true even (that is $\frac{1}{2}$, or 1 to 1). Having done so we find the error of our value V; it will be $\pm E$, and that means that any value within the range $V \pm E$ is equally probable. The higher the working degree of probability of our conclusion the wider are the limits $V \pm E$. Now so long as the results of the study of organic variability are expressed numerically, with these numerical limits of error, they are valuable, but otherwise one can hardly discuss them. That is to say, an empirically formed frequency distribution such as:

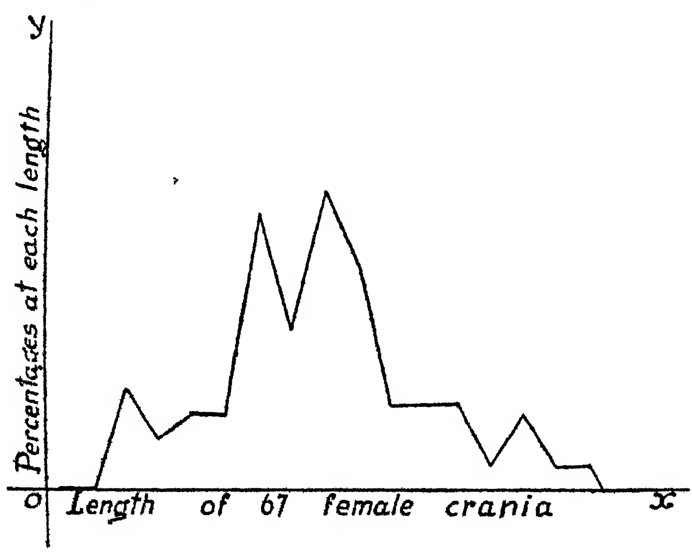

FIG. 1.

is only of very limited value. From this distribution, or its graph, we can obtain (1) the mean length; (2) the modal (or most prevalent length), which is not the same as the mean; and (3) the range of variation of length. But these constants of the distribution are not given very approximately, and some of them vary with the mode of grouping of the observations. The latter must be grouped and the choice of a group is arbitrary. Different graphs thus arise, and in spite of the honesty of the investigator he will usually exhibit (it may be unconscious) bias in selecting the form of grouping. *The empirical distribution and graph must be replaced by the theoretical ones*. That is to say, we must find the limit to which the observations tend were they to become indefinitely numerous, or to embrace the entire population (which is being investigated really by means of a sample).

We take, as a first approximation to this result, the hypothesis that the causes producing variation (*or deviation from the mean value* of the independent variable), are a multiplicity of small causes, all of which are independent of each other, and the result of the operation of which is as likely to give deviations in excess of the mean as it is to give deviations in defect of the mean. Given these conditions it is now easy (for the mathematician) to deduce the law of variability. Let a pack of cards contain ten each of spades, clubs, diamonds, and hearts, and let ten cards be drawn.[1] Let the cards be reshuffled and drawn again, and let this be done (say) 200 times. Then the probability of getting 0, 1, 2 . . . 8, 9, 10 spades is given by the successive terms of the expansion $200\left(\frac{1}{4}+\frac{3}{4}\right)^{10}$, for the chance of getting a spade, if one card be drawn is $\frac{1}{4}$ there being one spade to every four cards. The series given by trial is that first quoted on p. 535, and the series deduced by calculation is the second one. The general expression is $M(p+q)^n$, where p is the probability of getting a certain result, and q is the probability of failing to get it: $p+q=1$, $M=$ the number of trials, and n the number of different events (the numbers of spades that might be drawn in the example given). Now p and q may have any ratio between

[1] Note that to "draw" the cards so as to satisfy defined probabilities is not so simple as it appears.

them, so that the number of forms that the skew (or asymmetrical) expansion may take is indefinitely large. Suppose that $p = q$ and that the series thus becomes the terms of $200\left(\frac{1}{2} + \frac{1}{2}\right)^{10}$, or in general $M\left(\frac{1}{2} + \frac{1}{2}\right)^{n}$. Then we get the series which satisfies the conditions of our first approximation (p. 537), that is, the series given by the symmetrical binomial expansion. It would be the series given by drawing ten cards from a pack of ten each of all four suits, and recording the numbers of red cards drawn in (say) 200 trials. Thus:

Frequency	Found	Y^1	0	0	8	23	42	55	45	18	4	5	0
	Calculated	Y	0·19	1·95	8·79	23·44	41·01	49·22	41·01	23·44	8·79	1·95	0·19
No. of red cards		X	0	1	2	3	4	5	6	7	8	9	10

Obviously the symmetrical binomial series is only the limit to the asymmetrical series. In chance-series occurring naturally, the former are exceptional, and skew binomials represent chance-series more frequently than do the symmetrical binomials. This point is of some importance.

The series formed from the symmetrical and skew binomials are, however, discontinuous ones, while the series formed by biological frequency distributions are continuous. We might fit a binomial expansion to an observed frequency distribution, but the former would consist of a series of ordinates only, and would not be a continuous curve, while the frequency distribution, being formed from a series of grouped values, ought to be represented by such a continuous curve. We must therefore obtain the limiting expressions for the expansions $(p + q)^n$, first when $p + q = \frac{1}{2} + \frac{1}{2}$, and next when p is not equal to q, n being "infinite" in all cases. To find these limiting expressions is not easy, and the proofs usually given are difficult to follow. It is, however, established that the limit of $\left(\frac{1}{2} + \frac{1}{2}\right)^n$ when n becomes indefinitely large is the Gaussian curve $y = y_0 e^{-\frac{x^2}{2\sigma^2}}$, where y_0 is the maximum ordinate, e the exponential limit, and σ the "standard deviation," or "error of mean square." This Gaussian

curve is that long known in biology as the "normal curve of error," and for a time it was very generally applied to describe the frequency distributions formed when organic variability was studied.

It is very extraordinary that the "normal curve" should have been so extensively used in biology. It must have been seen that it usually did not fit frequency observations well, while there was no justification for the assumption on which it was based being generally applicable to variability in organisms. As we have seen, that assumption is that the causes producing deviations from the mean position of variability are independent, and without tendency to either the positive or negative sides of the mean. It must, over and over again, have been seen that frequency distributions were, as a general rule, asymmetrical—that is, observation of a great number of empirical graphs shows clearly that the curve generally rises steeply from zero to a maximum, and then falls less steeply or *vice versa*, so that the mean and maximal ordinates are not the same. Obviously (so it seems now) biologists ought to have applied the continuous curve obtained by Karl Pearson, as the limit to the skew binomials, in the same manner as the Gaussian curve was obtained as the limit to the symmetrical binomial. This curve is $y = y\left(1 + \frac{x}{a}\right)^{\gamma a} e^{-\gamma x}$, and it represents the probabilities giving rise to a chance distribution when the contributory causes of deviation from the mean are very numerous and independent, and when they tend to give deviations in excess of the mean, and in defect of the mean, which are no longer equal to each other.

But again something more general was required, that is, an expression for the probabilities that arise when the causes of variation may be either independent or correlated, and may or may not give deviations in excess of the mean equal to those in defect of the mean. An expression for such a series may be deduced (by a mathematician) from "elementary" theorems in probability. It is

$$\frac{pn(pn-1)\ldots(pn-r+1)}{n(n-1)\ldots(n-r+1)}\left\{1+\frac{rqn}{pn-r+1}+\right.$$
$$\left.\frac{r(r-1)}{\underline{|2}}\cdot\frac{qn(qn-1)}{(pn-r+1)(pn-r+2)}+\ldots\right\}$$

This is the hypergeometrical series and we have to deduce a series of curves from it in the same way that the curves mentioned above were deduced from the symmetrical and skew binomial series.

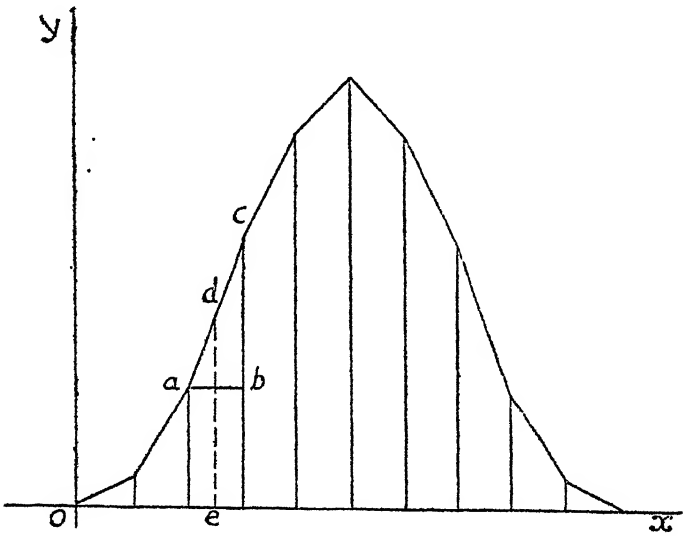

FIG. 2.

Let the graph of the symmetrical binomial expansion $\left(\frac{1}{2}+\frac{1}{2}\right)^n$ be as above. The slope (or rate of variation of y with respect to x) is given (for the mean ordinate ed) by the ratio bc/ab. The non-mathematical reader may then be able to puzzle out for himself (he will have to do so anyway, since the mathematicians omit the steps as obvious) that

$$\frac{\text{slope}}{\text{mean ordinate}} = -\frac{2 \times \text{mean abscissa}}{2\ (\text{standard deviation})^2}$$

and this relationship may be reduced to

$$\frac{1}{y}\frac{\delta y}{\delta x} = -\frac{x}{c_0}, \tag{1}$$

c_0 being a constant.

In the same way it can be shown that we have for the series of skew binomials

$$\frac{1}{y}\frac{\delta y}{\delta x} = -\frac{x}{c_0 + c_1 x}, \tag{2}$$

and for the hypergeometrical series

$$\frac{1}{y}\frac{y}{\delta x} = -\frac{x}{c_0 + c_1 x + c_2 x^2} \qquad (3)$$

The functions required to describe biological frequency distributions are usually continuous, so that finite differences must be replaced by differentials and equation (3) may be written

$$\frac{1}{y}\frac{dy}{dx} = \frac{a + x}{c_0 + c_1 x + c_2 x^2} \qquad (4)$$

Equation (4) is also written

$$\frac{dy}{dx} = \frac{y(a + x)}{c_0 + c_1 x + c_2 x^2} \qquad (5)$$

and this enables us to give it a geometrical meaning—

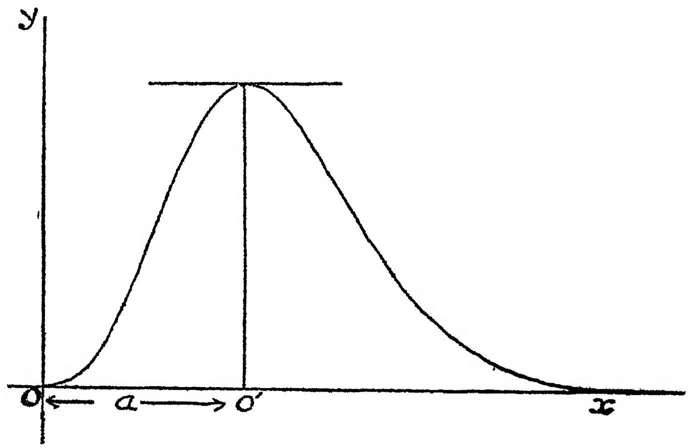

FIG. 3.

The ratio dy/dx is the first differential coefficient and is, geometrically, the tangent to the curve. At the ends of the curve the tangent becomes indistinguishable from the x-axis and therefore becomes zero, so that when $y = o$ (at the ends of the curve) dy/dx becomes zero. When $-a = x$ (that is, when the origin of the distribution is shifted from o to o^1) the tangent becomes parallel to the x-axis (the test of a maximal ordinate) and dy/dx again becomes zero. The differential equation (4) thus describes the salient characters of frequency distributions. It includes, as special cases, equations (2) and (1), (2) when the coefficient of c_2x^2 becomes zero, and (1) when

both the coefficients c_1 and c_2 become zero. It is the fundamental differential equation representing Pearson's Generalised Probability Curve.

We are dealing here only with the reasoning by means of which this generalised equation has been obtained, but its further development may very briefly be noticed. The constants a, c_0, c_1 and c_2 must be given numerical values depending on the particular frequency distribution to which the equation is to be fitted, and this is possible by applying the "method of moments," a method by means of which Pearson replaced the "method of least squares." Let the following graph represent a frequency distribution:

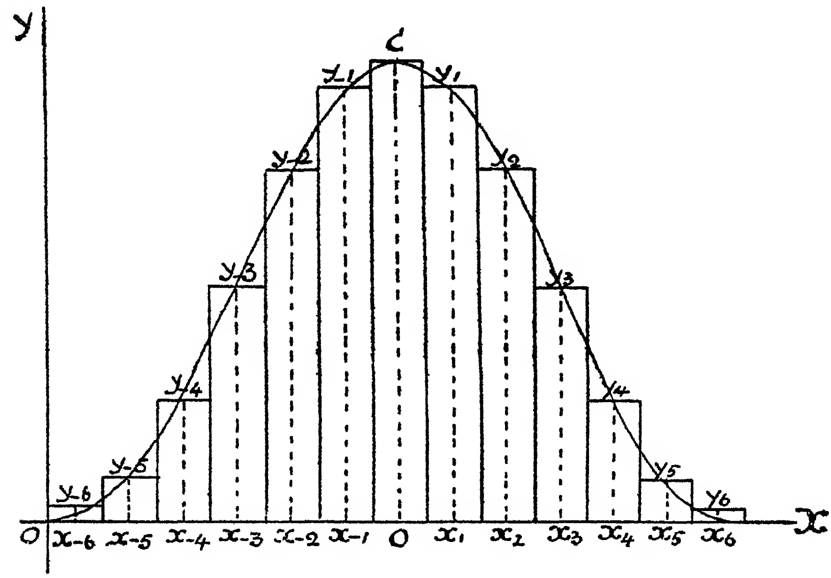

FIG. 4.

It is supposed that we are dealing with a numerous series of observations which have been grouped into thirteen classes represented by the rectangles. We further suppose (for simplicity) that the distribution is a symmetrical one. The mean is at co. All the observations between $x = 0{\cdot}5$ and $x = 1{\cdot}5$ are contained in the rectangle y_1, and so on. The dotted lines represent mid-ordinates, and we suppose that the area of each group is represented by the product of the mid-ordinate and the base on which it stands. The continuous curved line

represents the theoretical frequency curve which is to replace the rough series of observations, and this curve must be such that its area is equal to the sum of the areas of the rectangles representing the crude observations. Let the equation to the frequency curve be $y = f(x)$, then $\int y dx =$ area of the graph. If we now multiply each mid-ordinate by its distance from the mean and then take the sum of all these products, we obtain the first moment of inertia of the whole distribution about its mean, that is, $\ldots + y_{-2}x_{-2} + y_{-1}x_{-1} = y_1x_1 + y_2x_2 + \ldots$, since the distribution is supposed to be symmetrical. This gives us the equation $\int xy dx = 0$. In a similar way we find the second moment by multiplying each mid-ordinate by the square of its distance from the mean: this gives us $\int x^2 y dx = C$. Proceeding in this way we can make as many equations as there are constants in the expression to be found, and by solving these equations simultaneously the numerical values of the constants can be obtained and substituted in equation (5).

Equation (5) must then be integrated so as to put it in the form $y = f(x)$. There are a number of integrals all of which satisfy the equation since the denominator $c_0 + c_1x + c_2x^2$ can be written $(x - \alpha)(x - \beta)$, and the form of the integral giving the solution depends on the nature of the roots α and β in the quadratic $(x - \alpha)(x - \beta) = 0$. The actual integration involves mathematical technique which does not concern us here, but as its results we obtain the series of Pearson Frequency Curves, two of which, the "normal curve" (the limit to the symmetrical binomials), and the Type III curve (the limit to the asymmetrical binomials) we have already considered. The most common of these frequency curves in biological work on variability is Pearson's Type IV, which is

$$y = y_0\left(1 + \frac{x^2}{a^2}\right)^{-m} e^{-v \tan^{-1}\frac{x}{a}}$$

The philosophical basis of the Pearson Generalised Probability Curve should be noted, for this conception will probably come to be considered as perhaps the most distinctive and fertile advance made by general biology during the last decades of the nineteenth century. Organic variability is

clearly to be regarded as the resultant of a multiplicity of contributory causes, and its treatment thus becomes a matter for the application of statistical principles. Conceding this we have now to find what general results emerge. Let us note, first of all, the difference between the typical biological frequency distribution and the physical curve of error. The latter arises when a series of determinations of some physical constant are made in such a way that constant errors of experiment and errors resulting in bias are avoided. Even then divergent values for the constant must be obtained as the result of inevitable experimental errors, but the methods of determination are so designed as to make it equally probable that the error will be in excess of the mean or in defect of the mean. There is an unique, natural value to be obtained and the deviations from this observed in individual experiments have no reality (in a sense). The mean is the most probable value of the constant, and the deviations are a measure of the experimental error. But in the biological frequency distribution the mean is purely an abstraction. There is a modal, or most prevalent frequency, or value of the character studied, and this modal value is, in general, not the same as the value of the mean. Further, the deviates from the mode, or from the mean, are all real natural things. If we compare a biological frequency distribution with a physical curve of error we must think of the individuals of an elementary species as the results of an experiment. There *ought to be* unique values for each character, but imperfections in the developmental methods have given rise to the variability. The idea is highly artificial, and without considering where it may lead we can hardly accept it.

We do not, in the least, know what are the contributory causes that produce variability, but we can find what is their resultant in any particular case from the mathematical analysis based on Pearson's methods. The generalised probability equation (5) gives us, as we have seen, three cases: (1) when the coefficients c_1 and c_2 vanish, when we obtain the "normal curve of error." The contributory causes of variation are now numerous, they are all independent of each other, and their result is as likely to produce variations in excess of the mean as it is to produce variations in defect of the mean. That is to say, the variations are "fortuitous" and without tendency,

or bias, in one direction or the other. This was the older conception of variability, and it was held to be " normal " and analogous to a physical curve of error. But very little experience of biometric work shows that frequency distributions described by the " normal curve of error " are quite exceptional. We may indeed describe distributions in this way, but analysis shows that they are, as a rule, far better described by other types of the general equation. (2) The second case arises when the coefficient c_2 vanishes, leaving c_0 and c_1. The contributory causes of variability are still very numerous, and they are independent of each other, but they have bias and the distribution is asymmetrical, on one side or other, of the mean. This case is more frequent than the first one. (3) The third case arises when all three co-efficients, c_0, c_1, and c_2, are present ; the numerous contributory causes of variability are then no longer independent of each other, but are correlated. They give asymmetrical distributions, that is to say, series in which the variations exhibit a definite tendency, and this is by far the most common type of organic variability.

THE SPECIFIC CHARACTERISTICS OF VITALITY

By Prof. DAVID FRASER HARRIS, M.D., D.Sc., F.R.S.E.
Professor of Physiology and Histology in Dalhousie University, Halifax, N.S.

The attempts to identify life with "mechanism" depend chiefly on certain analogies between living beings and man-made machines or engines of some sort.

In a certain sense living beings are mechanisms, as when we choose to regard the body as a means of transmuting the chemical energy of food into the kinetic forms of heat and movement: here we may speak of the body as a heat engine. But to insist on describing a warm-blooded animal as a heat-engine is to concentrate our attention on one phase of its activity and totally to overlook a number of aspects in which it does not at all resemble a man-made machine.

Certain tissues of the living body are so disposed that they perform the functions of valves, levers, pulleys, hinges, all of which contrivances act precisely as metal valves, levers, pulleys, or hinges; but to admit this is not by any means to see no essential difference, physico-chemically, between protoplasm and machines of purely human construction. To say that there is no essential difference between the growth of a crystal and of an infant is to magnify the resemblances out of all proportion to the differences. But this sort of thing is what Prof. Ostwald does when he writes,[1] "Our observations so far have shown the organisms to be extremely specialised individual instances of physico-chemical *machines*."

He has been comparing a living organism to a flow of water through a pipe; and, by laying great stress on the truth that each is not a stable but a stationary form of matter, that is, the form although not the matter of the jet and of the body srepectively is conserved from moment to moment, Prof. Ostwald finally loses sight of any fundamental differences between the non-living and the living forms.

[1] Ostwald, *Natural Philosophy.* London: Williams & Norgate, 1911.

That living matter is able to transform energy and is itself highly unstable, chemically speaking, need not compel us to regard it as precisely in the same category as certain other much more stable forms of matter which also are able to transform energy.

The following enumeration of the properties of protoplasm or living matter would probably be disputed by no philosophical biologist as constituting the characteristics of the highest (mammalian) type of protoplasm.

1. The possession of affectability (irritability);
2. The possession of functional inertia;
3. The power of transmuting the chemical energy of food into the kinetic forms of heat, movement, nerve energy, electric current, and sound.
4. The power of assimilating matter which is chemically unlike the living substance, and of growing thereby.
5. The power of producing, under the influence of foreign substances or poisons, certain anti-bodies (anti-toxins, or ferments) designed to render the former innocuous.
6. The power of reproduction, that is to cast off an organism capable of independent existence which shall perpetuate the parental type.
7. The power or propensity to evolve, from a relatively simple microscopic layer of cells, all the highly differentiated tissues and systems of the adult body.

Now it is undeniable that some of these characteristics of livingness are also possessed by matter which is not living and has never lived. For instance, affectability or irritability, found in every text-book of physiology as a property of living matter, is certainly a property of such lifeless matter as gunpowder or dynamite and of many another unstable chemical substance. Gunpowder has affectability towards the stimulus of a spark, and dynamite has affectability towards the stimulus of concussion, for affectability is but the power of responding to a stimulus.

Similarly functional inertia—or the power of *non*-response to a stimulus—is a property possessed both by living and by non-living matter. In living matter it is the property by which limits are set to its activity, by which it is made insusceptible to certain stimulations, by which rates of response

beyond a certain number are made impossible; it is that property which prevents indefinitely long-continued action and unending response.

But clearly this same property is possessed by certain chemical systems. Thus dynamite has inertia towards a spark; it explodes by concussion, not by ignition.

The extreme molecular instability of certain substances, the picrates, fulminate of mercury or of silver, may certainly be called their affectability towards stimuli tending to cause their disruption, but their possession of this high affectability does not entitle these materials to be called living.

Affectability is a property not of living matter exclusively, but of many varieties of non-living matter as well: affectability is correctly classified as one of the fundamental properties of protoplasm, but protoplasm is not living matter because it possesses affectability. Living matter also possesses functional inertia, which is the power to disregard or be oblivious to certain kinds of stimuli; but some kinds of matter that are non-living also possess this property in a high degree. The possession, therefore, of a degree of inertness towards certain forms of stimulation does not distinguish living from non-living substance. Both affectability and functional inertia are vital properties, but they are properties not possessed exclusively by living matter.

When we pass on to the other characteristics of vitality, we find ourselves on very different ground.

Living animal bioplasm has the power of growing, that is of assimilating matter in most cases chemically quite unlike that of its own constitution. Now this is a remarkable power, not in the least degree shared by non-living matter. Its very familiarity has blinded us to its uniqueness as a chemical phenomenon. The mere fact that a man eating beef, bird, fish, lobster, sugar, fat and innumerable other things can transform these into human bioplasm, something chemically very different even from that of them which most resembles human tissue, is one of the most extraordinary facts in animal physiology. A crystal growing in a solution is not only not analogous to this process, it is in the sharpest possible contrast with it. The crystal grows only in the sense that it increases in bulk by accretions to its exterior, and only does that by being immersed in a solution of the same material as

its own substance. It takes up to itself only material which is already similar to itself; this is not assimilation, it is merely incorporation.

Writers of a materialistic bias have, however, striven to regard such growth of crystals as not essentially differing from protoplasmic growth. Except that matter is taken up from outside in both cases, there is no similarity between the two processes at all. Growth of bioplasm, which includes and is the outcome of the incorporation of the dissimilar, the essence of the metabolic phenomena characteristic of living matter, is something *sui generis*.

The term "growth," strictly speaking, can be applied only to metabolism in the immature or convalescent organism. The healthy adult is not "growing" in this sense; when of constant weight he is adding neither to his stature nor his girth, and yet he is assimilating as truly as ever he did. Put more technically: in the adult of stationary weight, anabolism is quantitatively equal to katabolism, whereas in the truly growing organism anabolism is prevailing over katabolism, and reversely in the wasting of an organism or in senile decay, katabolism is prevailing over anabolism. The crystal in its solution offers no analogies with the adult or the senile states—but these are of the very essence of the life of an organism. The crystal, when not incorporating molecules exactly similar to itself, is not in any sense active; but an organism at all stages of its life-history is active—unless indeed it is in the state of latent life.

The fact, of course, familiar to every beginner in biology is that the crystal is only incorporating and not excreting anything, whereas the living matter is always excreting as well as assimilating. This one-sided metabolism—if it can be dignified with that term—is indeed characteristic of the crystal, but it is at no time characteristic of the living organism. The organism, whether truly growing or only in metabolic equilibrium, is constantly taking up material to replace effete material, is replenishing because it has previously displenished itself or cast off material. The resemblance between a so-called "growing" crystal and a growing organism is verily of the most superficial kind.

It is true that a living organism resembles a wave—the form persisting, the matter changing—but that does not

entitle us to lose sight of the inherent differences between the two; it justifies us neither in calling the wave alive nor the living organism "mechanical." Assimilation of matter is, then, a vital characteristic, specific for livingness in a sense in which neither affectability nor functional inertia is specific for it.

But when we pass on to that vital property, the power to produce anti-bodies, we encounter a property whose specificity is absolute. This is a property known only within the last few years; and as it is the basis of immunity from the poisonings due to disease-producing micro-organisms, it is being very carefully studied at the present time by workers in pathological chemistry.

Hardly any subject of biochemistry is so unsuited to popular presentation, but the principle of it may be stated as follows: if material foreign to the blood or tissues of an animal be introduced into that animal's blood, then the body cells proceed to elaborate a substance (anti-body) designed to counteract or neutralise the foreign substance introduced.

The substance introduced is known as an antigen; the substance produced as a vital response to the foreign stuff is the anti-body. This is evidently a protective chemical procedure, or "mechanism," as it is called for short; it is the expression of the body's power of combating chemical insults by chemical means. As a chemical process it is highly specific, that is to say, the particular anti-body which neutralises the poison of diphtheria, for instance, will not also antagonise the poison of typhoid fever or of pneumonia. Each toxin is responded to only by its own specific antitoxin. Now this is wholly without parallel or analogy in the world of the non-living. *It is characteristic of life alone*, it is specific. Modern biochemistry has, then, given us a new feature or differentia of living matter, a power of protoplasm not recognised a few years ago, not known to the men who regarded affectability as *the* chief vital manifestation.

The properties of reproduction and differentiation of tissues, though recognised for a much longer time than the formation of anti-bodies, are none the less confined to life alone. Non-living matter behaves in no wise which could be construed into regarding it as capable of reproduction or of progressive morphological differentiation.

Homogeneous non-living matter makes no spontaneous efforts from within towards heterogeneity.

If any one cares to regard the breaking off of a portion of a crystal as analogous to the giving birth to an immature organism, capable, in due time, of an independent existence, he may do so as a poetical exercise, but not as any contribution to the philosophy of biology.

The power of sexual reproduction is entirely *sui generis*, and has no analogy in the non-living world. The so-called similarities between sexual phenomena and the attraction of oppositely electrified atoms or ions are really only pretty fancies. Possibly the most mysterious or wonder-rousing property of living matter is its inherent power of progressive cell-differentiation.

Out of the single fertilized ovum (or egg), usually of microscopic size, there arises in due time the perfect animal; the simple ovum having given place to millions of cells, each having its own niche in the living mosaic. And the end-product of the cell evolution is highly characteristic, for the microscopist can tell at a glance a brain-cell from a liver-cell, a bone-cell from a cell of the retina, and so on. Morphological differentiation evidently underlies functional specialisation.

Sometimes these self-developing systems are spoken of as "mechanisms," but that is justified only in the interests of brevity of expression: no man-made mechanism ever evolved itself, or, being made, ever became anything else; but this is precisely what the "mechanisms" of protoplasm are continually doing. The single and simple can evolve into the multiform and complicated merely, apparently, by possessing the power of assimilation. The like can evolve into the unlike; the cells of the embryo which at one stage are practically all alike are destined to become something exceedingly different, as different as brain from bone. A mere mass of spherules will shortly become all the complexities of the eye or ear; some lens cells detached from the lens of the eye can produce a lens outside of and away from the eye altogether; some bone cells placed on the skin forthwith proceed to develop bone on the outside of the body.

It is customary to make a great deal out of radium being able to disintegrate and become something else as though

this interesting transformation could blur the sharp lines of distinction between the living and the non-living.

It is characteristic of vitality to have its youth, its prime, and its decline, but there is nothing of this in the non-living. The hills are everlasting; but neither the sheep nor the shepherds are. "Change and decay" applies only to the living environment: the "birth" and "death" of worlds is poetry.

It is of the essence of living things to have phases or rhythms in their metabolism and phases or stages in their life-history. If the non-living corresponds at all to any phase or stage of the living matter, it is to the phase of latent life.

Latent life is that condition of living matter where there are no signs of life, only the potentialities of life in the future. The vital material of the frozen frog, insect, fish, or rotifer, while it remains in latent life, is in the *status quo ante* just as a stone is; but whereas the frozen organism can *re*vive, as we say, or again shows signs of vitality, that is, come out of its latent phase, the stone or crystal cannot, and all attempts to represent it doing so are based on metaphors.

The living thing tends to death; the non-living tends to nothing at all. Death is the natural end of life; physical science as such knows nothing of the endless existence of living things.

In life there is some affectability and some functional inertia; in latent life there is minimal affectability and much functional inertia; in death there is no affectability and maximal functional inertia.

This is, then, a new definition of death, for it is a positive one, all others, such as "death is the cessation of existence," are negative. Whereas in life there are signs and potentialities for future life; in latent life there are no signs but only potentialities; in death there are no signs and no potentialities. This is another new positive definition of death.

These conclusions do not necessarily preclude the belief that living matter has been evolved from non-living as Sir Edward Schäfer and Prof. B. Moore indicate. Possessing the fundamental properties of affectability, functional inertia, and the power to transmute energy, it is conceivable that non-living matter became, physico-chemically, so unstable that it finally acquired the four properties which have just been

described as specifically characteristic of it: when it had acquired these it was alive.

Functional endowment, therefore, and not the possession of any "vital force," constitutes livingness. The desire of the chemists to create living out of non-living matter is not hereby ridiculed, neither is it pronounced impossible. But we must be prepared beforehand to recognise this man-made living matter whenever and wherever we may be privileged to see it. Presumably it will be first presented to us in a test-tube. How shall we know that it lives? The biochemist says that protoplasm is an irreversible colloidal hydrosol of emulsoid constitution: excellent, but how may we know that this hydrosol is alive? Not from its conforming to the above description, for matter entirely dead might still be so described. By its "extreme complexity"? No; for again extreme chemical complexity can characterise both non-living and dead matter. By its possessing affectability, functional inertia, and the power of transmuting energy? Once more, no, for all these three are possessed by certain lifeless substances. Matter, whatever its origin, cannot be pronounced alive unless it is capable of assimilating the unlike, of producing anti-bodies, of reproducing itself and of undergoing spontaneously a certain degree of morphological differentiation.

These are the credentials of living matter as we find it in the universe to-day; matter, wherever it has arisen, which possesses these four properties must be called alive.

Some years ago I contended [1] that affectability and functional inertia were *the two fundamental* properties of living matter, and such indeed we must regard them. No kind of matter which does not respond to a stimulus can be called alive; all matter which is alive can or may respond to a stimulus, but affectability is shared by non-living matter. Conversely, matter which was the everlasting plaything of stimulation, which could not resist or be oblivious to certain stimuli at certain times, would not be alive. It is of the essence of livingness to disregard *some* stimuli, to prevent some responses, to set limits to activity, to act rhythmically and not constantly or continually; but insusceptibility to stimulus is a property shared also by non-living matter.

[1] Fraser Harris, *The Functional Inertia of Living Matter*. London: Churchill, 1908.

It is theoretically possible that both these fundamental properties of living matter came to be found in it because they were inherent in the pre-existing non-living matter from which the living may be supposed to have been evolved. Similarly for the power of transmuting energy, a property notoriously not confined to livingness.

Nor does anything said here contradict the belief that affectability, functional inertia, and the transmutation of energy are primary or fundamental properties, while the last four are secondary or derived. For instance, if living matter had no affectability, it would be unaffected by the presence of food material, and so would not respond to it in the direction of assimilating it.

Recent physiological work has made us aware of a power in living tissues to offer resistance to the so-called "physical forces." We have been shown that the important function of absorption of food from the intestine is not capable of being explained by the laws of "physical" osmosis alone. The "mechanism" of absorption is not mechanical in the sense that it is the outcome of laws operative entirely in non-living matter. Prof. Waymouth Reid has demonstrated that if the living cells lining the alimentary canal be removed, the food is absorbed less perfectly than before, although now it is of course actually nearer to the blood which is ultimately to receive it. Similarly, Prof. Gregor Brodie has proved that in the kidney, merely physical forces, blood-pressure, etc., will not account for the formation of the watery secretion of that gland; he is compelled to speak even of the water as being vitally secreted. Dr. J. S. Haldane, of Oxford, asserts that under certain conditions the living lining of the lung acts, as regards the excretion of carbonic acid gas and the intake of oxygen, in direct opposition to purely physical diffusion.

I have purposely not grounded the thesis of this paper on the phenomena of consciousness. The existence of consciousness *is* the supreme distinguishing feature between the living and the non-living; but our uncertainty as to the existence of self-consciousness in the lower forms of life precludes us from making dogmatic statements about its significance as a differentia of livingness in every one of the types of living matter. It seems safe to assume that consciousness does not

arise until the related neuroplasm has attained to a certain degree of functional differentiation.

Certain biologists, for instance Haeckel, have, *on the contrary*, assumed that consciousness does exist in every animal form from amœba to man ; but as this position is absolutely beyond the reach of proof, it cannot be used as a datum for further argument.

The assumption seems extremely improbable ; were it true, the *fœtus in utero*, for instance, would be a self-conscious existence ; but if we are sure of anything, we are sure that is not. If we could be certain that consciousness is an accompaniment of all living matter, then we could regard it as an additional differentia of protoplasm, for no one holds, save as poetry, that non-living matter is conscious.

In those animals which possess it, consciousness is a characteristic of life *sui generis*, in which respect it resembles the power of producing anti-bodies ; but only somewhat highly differentiated protoplasm possesses either of these properties.

Enough has been said to justify us in refusing to see no essential differences between matter in the living and in the non-living state. That living matter had its origin in the non-living is a conceivable evolutionary possibility; but that we may discover in matter which has never lived all the properties whereby we recognise vitality does not seem a correct interpretation of the world of life as we find it.

By magnifying resemblances and by explanations based on analogies, we can endow the non-living with the semblance of life, but we are somehow certain after all that differences are there, and that it is poetry and not science that has been enriched. Between the living and the non-living there is a great gulf fixed, and no efforts of ours, however heroic, have as yet bridged it over.

THE NATURAL HISTORY OF TUMOURS

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THE earliest sign of a tumour is a bud growing from some tissue which appears to be healthy, but which in a large proportion of instances has been subjected to continued irritation.

These buds are like the buds which in the less highly organised forms of animal life grow into organs or limbs, or even complete individuals. In a sense they are independent of the parent organism. Their life is not part of its life. It is true they are not capable of maintaining themselves. They are indebted to the parent organism for all that they want in the way of sustenance. But they thrive though the parent starves, and drain it of all its strength that they may continue to grow. They grow out from the parent organism. They live upon it, but they are not part of it, and they never do anything for it.

The buds that grow into organs or limbs owe their formation to the power of reproducing themselves asexually which the tissues possess as their birthright. In the less highly organised animals this power can be exercised at almost any time of life in almost any degree. With the advance of organisation its activity diminishes until at last in the most highly organised only certain tissues can exercise it, and these only to a limited extent. Tumour buds are due to this power, or rather to so much of it as the tissues retain at the moment, roused into action by some exciting cause such as local irritation.

DIRECT REPRODUCTION

One of the points that distinguish living organisms from inert matter is their power of absorbing and assimilating foreign material, in virtue of which they are able to replace the waste of living, to increase in size, and when the limits of size are reached, to increase in number. This power of

growth is inherent in all living things, so long as they are living. It is born in them with their birth, lives with them throughout their lives, and dies with them when they die. So long as conditions are favourable, it knows no bounds or limits. Reproduction is growth beyond the individual.

This is not difficult to understand in the case of primitive organisms such as those which existed in that immeasurably remote period when life made its appearance upon earth. Their structure was of the simplest, and their growth continued until at last their size became so great that they were compelled to divide. The dimensions reached before division took place probably varied in each case. There was no definite limit. It depended entirely upon circumstances. But when circumstances continued to be the same for generation after generation, the intervals gradually became more and more regular, and a certain uniformity was introduced. The power of reproducing itself directly as soon as a certain size was reached belonged to every organism as part of its birthright. It is part of the birthright still.

The Division of the Organism into Germ Cells and Somatic Cells

A change took place when the metazoa came into existence. Increase in size in a multicellular organism necessarily involves division of labour. The function of reproduction, for example, can no longer be carried on equally well by all parts of the organism indiscriminately. One part better suited by locality or arrangement is marked off from the rest and undertakes the work of maintaining the race. The cells of which it is composed give up everything else. They do nothing for themselves. The rest of the cells, on the other hand, the somatic ones, lose all concern with reproduction and devote themselves entirely to the work of every-day life and the maintenance of the individual. This is the primary division. On this is based the classification of the tissues and of the tumours that grow from them.

At the first the distinction between the two sections is not well marked. For a long time the cells of one group can take the place of those of the other and perform all their duties if required. In the ova of some of the echinodermata,

for example, the germ cells and the somatic cells can be displaced artificially and each will carry on the work of the other. Even when this is no longer possible, the somatic cells cling tenaciously to their ancient privilege and only give it up little by little.

Direct Reproduction from Germ Cells

The germ cells are concerned solely with reproduction. Their mission in life is to form and give off the buds that will grow into living organisms for the next generation. At first there is no suggestion of such a thing as sex. How and when it first came about that germs to reproduce the race, instead of growing from a single cell, should require a stimulus coming from a second one to start them on their course, is not known; but it is obvious that germs formed in this way, by the fusion of parts of two cells, would stand a better chance of success in the struggle for existence than those formed from a single cell, if only because of the greater likelihood of the occurrence of useful variations. When this bi-cellular origin was once initiated, the division of the primitive germ organ into two parts, an ovary and a testis, each providing its own special factor in reproduction; the maturing of the two products at different times, so that the conjunction of two separate individuals would be required, and the comparative weakness of self-fertilisation avoided; and then the suppression of one of these organs in one of the individuals and of the other in the other, with the result of separating the sexes entirely, are such obvious advantages that they follow as a matter of course.

Sexual reproduction, therefore, which bulks so largely in our vision that it has caused the existence of the primitive method of direct reproduction to be almost ignored, is only a specialisation. The primitive method, the birthright of all living tissues, continues to exist in the background. It does not die out or disappear because the sexual organs have been developed. It is still there, though its range of action grows more and more limited, and if occasion requires can be called into play at once. In many insects, for example, generations in all respects identical with those that precede them can be produced asexually, even though functional sexual organs

are in existence; and in animals so highly specialised as frogs, ova that have never been fertilised by spermatozoa can be made to develop even into the tadpole stage by applying a suitable chemical stimulus. The same thing within narrower limits is true of the germ organ in man. It may not be possible for it to give birth to a new and perfect individual, but in embryonic life at any rate its power of direct reproduction is strong enough to enable it to give off buds capable of growing into highly complex structures; and though as development advances, the range and strength of this power diminish, what is left of it can be called into action at almost any time if a suitable stimulus is applied.

Tumours that Grow from Germ Cells

The growths that originate by direct reproduction from the germ organ and its derivatives (especially the ovary) assume many different forms. The most elaborate spring from the as yet undifferentiated germ organ, in the earliest moments of existence, before the ovary and testis are formed, and occur therefore in both sexes. Others, not quite so perfect, are composed of organs which are sometimes well developed and arranged more or less regularly. These probably develop from the ovary, but just as the stage in which the germ organ is still undifferentiated shades into that in which there is a functional ovary, so no hard and fast line can be drawn between these and included fœtuses. Ovarian dermoids which do not make their appearance until later in life undoubtedly arise from the asexual development of ova. There is one long unbroken series of tumours arising from the germ organ and its derivative, the ovary, by the process of direct reproduction, beginning with an organism, that is almost as perfect as its parent, and ending in a shapeless mass of epithelial cells. Just as in the evolution of the race the primitive method of reproduction slowly disappears, leaving here and there behind it evidence of what it once could do, so in the individual this same power, which is so strong in the early embryo, gradually becomes feebler and feebler until it almost dies out. The tumours that grow as buds from the germ organ and its derivatives are the expression of so much of the primitive power of direct reproduction as the parent stock retains, set free from

the influence that should control it, and stung into activity by a stimulus which, judging from the analogy of the race, is in all probability a chemical one.

The testis, it may be remarked, the complementary half of the primitive germ organ, does not retain this power in anything like the same degree as the ovary, probably because it has become more highly specialised.

Direct Reproduction from Somatic Cells

All that is true of the origin of the tumours that grow from germ cells is equally true of those that grow from somatic ones. In both the formation of tumour buds is due to a sudden awakening of the primitive power of direct reproduction innate in the tissues, or rather of all that is left of it.

In the earliest days of existence the function of reproduction is shared by all the cells of the organism alike. When, owing to increase in size, labour has to be divided this function is assigned to one particular group, known henceforth as germ cells. All reproduction is direct until the bi-sexual method is developed. Then the direct method gradually ceases to be used, and from being employed so seldom, falls into the background and all but dies out.

The same thing occurs with the somatic cells. At the beginning their power of direct reproduction is equal in all respects to that of the germ cells. They, too, can give off buds which are capable of developing into organisms like themselves. But when the division of labour begins, and somatic cells have all to undertake different duties, they surrender this one to the germ cells, and never carrying it out themselves they lose the power of doing so.

They do not lose it all at once. The power of direct reproduction diminishes step by step as organisation advances. The process can be traced as well in the history of the race as in that of the individual. In the simplest forms of animal life and in the earliest days of those that are more highly organised the power the somatic cells enjoy is almost unlimited. Many of the cœlenterata, for instance, can regenerate themselves from fragments taken from any part of the original body, and buds capable of developing into imperfect fœtuses can grow from different portions of the embryo even

in man. When the advance of organisation has rendered reproduction on such a scale as this impossible, the somatic cells still remain able to reproduce portions of the body, such as limbs. This can take place, for example, in many of the cold-blooded vertebrata, and it is possible that it may occur even in man, in the first few days of existence. At least it has been suggested that the little out-growths occasionally found on the sites of limbs that have been lost early in uterine life are attempts at reproduction. But as the progress of development continues, the possibilities of direct reproduction diminish, and when it comes to the most highly organised animals and tissues the limitations are very closely drawn. Reproduction and the formation of tumour buds are both restricted to structures composed of simple tissues. Complex structures such as limbs or organs cannot be reproduced, and, at any rate in adult life, complex tumour buds are never formed. The final stage is reached when specialisation has advanced so far as it has in the cells of the cerebral cortex and of the organs of special sense. These have lost almost all their primitive power. It is very doubtful if they can reproduce their like or replace what has been injured or give birth to tumour buds. Tumours composed of such highly specialised structures are practically unknown. Development has been carried to such a point in them that they have lost all their other powers in devoting themselves to one. The power of reproducing themselves which has been steadily diminishing in scope as development advanced, is no longer within their capacity, and when it is lost the power of giving birth to tumour buds is lost with it. They are one.

Tumours from Somatic Cells

With the exception of such highly specialised structures as these, every organ and every tissue in the body developed from the somatic cells forms its own kind of tumour, just as it possesses its own kind of structure. However much one tumour resembles others in general arrangement, it differs from them just as the parent organ or tissue differs from the rest. Under the term adenoma, for example, are included all tumours built upon the lines of glandular tissue; but those that grow from the parotid gland are as different from those

that grow from the mammary gland as one organ is from the other. And tumours that grow from such organs as the thyroid or prostrate are so different that they can scarcely be brought into the ordinary system of classification. Every organ and every tissue has its own kind of tumour which is peculiar to it.

The structure of tumour buds depends upon that of the parent stem. It is never so perfect or well formed, but there is always a general resemblance. The clinical character of the tumour, however, whether it is malignant or not, depends upon the degree of maturity that had been reached by the parent cells when the bud began to grow. Tumours may assume many different forms—a form that grows fast or a form that grows slowly; a form that remains circumscribed and limited or a form that retains its embryonic characteristics, spreads in all directions and invades other organs. If the parent cells are still in the actively growing embryonic stage when the tumour bud is formed, the tumour bud will be embryonic too. If they have already reached adult age, the bud will increase in size with proportionate slowness, and push surrounding structures to one side. Every organ and every tissue developed from the somatic cells has its own kind of tumour bud, which, according to the degree of maturity of the parent cells at the moment of its birth, may be benign or malignant, or benign first and malignant afterwards, or so evenly balanced between the two that it is impossible to say to which side it belongs. Malignant growths are not a separate class of tumours but a phase that occurs in all classes.

The Influence of Development over the Power of Direct Reproduction

The power the tissues possess of reproducing their like and that of giving birth to tumour buds rise and fall together. They both vary inversely with the degree of specialisation that development has reached. The higher the grade of specialisation the more their capacity decreases. But there is one material difference between them. In the one case the newly formed cell merely replaces another from which it is so slightly different that they can scarcely be distinguished apart. In the other a new cell is born which from the first

manifests its independence by multiplying without limit and living like a parasite upon the parent. In the one case development pursues a normal course. In the other the tissues suddenly exert a power which their racial ancestors used to possess, but which, apparently, in the course of generations has become almost extinct. This they are enabled to do because their development has been arrested.

The development of the individual is an epitome of the development of the race. This is true not only of the individual, but also of the tissues that compose it. All the tissues of the body, in the course of their development, epitomise the changes that took place in their ancestors during the evolution of the race. If the development of any tissue is interrupted at any point, so that it remains on a plane which was normal for some of its predecessors, that tissue retains and can exercise (supposing conditions are favourable) all the powers those predecessors possessed, no matter how far back they may have been in the evolution of the race.

The tissues, for example, may be able to exercise the power which was enjoyed by many of their remote ancestors, of throwing off buds capable of independent growth. In those days the buds developed into organs or limbs, or even sometimes into complete individuals. Now they can only grow into tumours, shapeless masses, the tissues of which present a general resemblance to those of the parent, capable of unlimited increase, but, unlike the buds of former days, incapable of progressive development, for development has been stopped.

All that is needed is some exciting cause, such as local irritation, to give the growth a start.

The influence of interference with development in the production of tumours is well shown by the growths that sometimes originate from embryonic or fœtal relics. Development implies not only the progressive advance of tissues that are of use, but no less also the recession and disappearance of those that have ceased to be of use. Evolution stops when the power of hereditary transmission fails, but so also does involution. If the progress of development comes to an end, fragments of organs that ought to disappear and become absorbed persist, and continuing to grow and work after a fashion sometimes become the nucleus of colossal tumours.

The Chemical Basis of Development

Development, the arrest of which leads indirectly to the formation of tumours, like growth, is the outcome of the chemical changes that take place in living tissues—the changes upon which their working and their life depend. This is acknowledged in the case of growth. Foreign substances come into contact with a living cell. They are absorbed by it and broken up in it by catalytic agents. The products of this catalysis are built up again in a different combination, and if the addition exceeds the consumption the result is, first, increase in size, and then, if circumstances are favourable, increase in number, each newly formed cell possessing the same power as the parent of which it once was part.

The same thing is true of development. Development, like growth, is the outcome of chemical changes that take place in the living cell, but there is this difference, that while growth depends upon the maintenance of all the reactions in their relative proportions, development depends upon the predominance of one. Development is the result of one of these chemical reactions, or rather of one series of them, being carried on by one part of the body in excess of all the rest for generation after generation. Division of labour means that every part of the body has to undertake a special kind of work. Special work entails a special chemical reaction or series of reactions. The more thoroughly the work is done, the more completely does this special reaction predominate over all the others at that particular spot. This, continued in the same group of cells for generation after generation, of necessity involves progressive modification of chemical constitution and of structure, or in other words development.

The layer of cells, for example, on the exterior of the organism, always exposed to foreign influences, becomes modified in course of time. They become harder and more resistant, keratin or some similar substance forms in them, and as generation succeeds generation and a similar modification takes place in each, at last an outer protecting layer is developed characteristic of that particular kind of organism. The same thing is true of the cells that line the digestive tract. Their special work, or one part of it, is the manufacture and discharge of catalytic ferments. The chemical reactions upon

which their working depends have been gradually evolved in the course of ages from simpler ones of the same kind. According to Ehrlich and others, the process of chemical evolution is still going on. New ferments are being formed, even at the present time, to deal with new conditions, and structure and arrangements are modified accordingly. So it is with other tissues and other parts of the body—those, for example, that are concerned with the transmission of impulses from one organ to another. Special work everywhere involves the evolution of special chemical reactions ; and as generation succeeds generation, and the reactions become better adapted to the end in view, the structure and arrangement of the part become modified to suit.

The development of a tissue or of an organ, in other words, is the outcome of the persistent cultivation of a special series of chemical reactions, brought into existence by the special work assigned to that particular part of the body. At the beginning these reactions were of the simplest character. As time went on and the race was built up, they were built up, gradually, step by step ; and as the individual is an epitome of the race in its chemical reactions as well as in its structural details, so all the chemical stages through which the race has passed are reproduced in the development of the individual, only compressed beyond recognition. The development of the individual is in part the product of the chemical reactions that have taken place in its ancestors from time immemorial, handed down by inheritance from generation to generation, in part the result of the chemical changes that are taking place in its own tissues at the present time. The essential point is that all development, like growth, is ultimately the outcome of chemical changes in the tissues, and that anything that interferes either with the inheritance of the products of past chemical changes, or with the effects of present ones, interferes with the development of those tissues, so that they remain on the same plane as their racial ancestors, and enjoy the same powers of reproduction.

So little is known of the intimate nature of the chemical changes that take place in the tissues that it is not easy to cite instances in which the failure of any particular reaction has led directly to the cessation of development and the birth of a tumour. There are, however, many isolated facts that

point in this direction. One of these relates to the occasional disappearance of tumours. It is well known that tumours, especially those of an embryonic type, sometimes stop growing, diminish in size and even disappear under the influence of remedies which can only act through the medium of the general nutrition. Cancer of the breast, for example, has been known to shrink in size and even to disappear after removal of the ovaries. The same thing occasionally happens without special treatment of any kind, perhaps more frequently than is usually believed. At least it is difficult in any other way to account for the single successes of queer remedies which are recorded from time to time, apparently in perfect good faith, and which can never be repeated. In the case of most of these it is not possible to say how or by what means the remedy acts; but in some it is almost certain that the effect is due to the reagent entering into chemical combination with the living substance of the tumour cells, and thereby altering the reactions that take place in them. Arsenic, for instance, administered internally, sometimes causes certain glandular growths to recede and almost disappear. Then, only too often, after a little while the growth begins to increase in size again, and now the same drug, even in far larger doses, has absolutely no effect. The tumour cells, in other words, have acquired immunity and have become arsenic-fast, just as Ehrlich has shown the spirochæta can, and for the same reason—the drug has entered into chemical combination with some element in the substance of the growing cell.

Another of these facts, in many ways even more striking, relates to the production of tumours. It is well known that the continued employment of certain substances, in industry as well as in medicine, is liable to be followed by the growth of certain kinds of tumours. The substances themselves are not the immediate cause of the growth. There is often an interval of years between their administration and the appearance of the tumour. But they initiate such changes in the nutrition of the tissues that when irritation of any kind leads to the rapid production of a large number of young cells, some of these cells stop short of the normal standard of development and form the nucleus from which a tumour bud begins to grow. This has long been known in connection with arsenic. The late Sir Jonathan Hutchinson was the

first to lay emphasis upon the fact that the prolonged internal administration of arsenic had sometimes led to the development of multiple tumours of the skin. The nutrition of the skin becomes affected; its texture becomes altered, and then, at some spot where continued irritation leads to the rapid production of a number of young cells, a tumour develops.

A closely similar course of events has been described in connection with some of the aniline dyes, only in this case the tumours do not involve the skin but the urinary organs, and assume different forms according to their origin. Rehn, in 1906, had already collected more than thirty cases of workers in fuchsin who were suffering from some form of tumour of the bladder. Leuenberger has collected many more and has shown that in Basle the deaths caused by tumours of the bladder in the years 1901–1910 were thirty-three times more common among the workers in aniline colours and similar substances than they were among the rest of the male population; and that more than half of the cases of tumour of the bladder observed in the last fifty years in the male surgical clinic at Basle came from dyers and those engaged in aniline dye works. Some grew in the bladder; others in the kidney and ureter; and, what is of peculiar significance, in some instances the growths did not make their appearance, or at least cause symptoms, until years after the worker had left his employment. A similar condition of things has been recorded of the workers in the cobalt mines of the Schneeberg, only in this case the growths, which are equally varied in their character, are met with in the lungs.

The tumours that so often follow the continued application to the skin of soot, tar, paraffin, and the like arise in a similar manner. Some substance is absorbed which, in course of time, affects the nutrition and functional activity of the skin, so that it becomes harsh and dry to the touch. The cells that compose it cannot carry on their work as they should. Their development, which depends upon the chemical changes that take place in them during their work, remains imperfect. It comes to an end before it should, while the cells are still in a stage that was perfect for their remote ancestors, but should only have been a transition stage for them, and, as a consequence, at a time when they are still capable of exercising

the powers those ancestors possessed. The result is the formation of a bud, like the buds that were thrown off from time to time by their ancestors, capable of independent growth and composed of cells, the rate of whose growth and multiplication depends upon the maturity of the parent-stock at the moment. If the affected cells have all but reached adult age before the interference is felt, the buds that grow from them are all but adult too. The tumour is composed of tissues that resemble those of the normal skin. But if owing to irritation, whether it is mechanical or chemical or due to the action of living organisms, there is a great increase in the proportion of young rapidly growing cells, and the development of these is checked in their youth, the buds that spring from them resemble them, and then the tumour increases rapidly and spreads wherever it can.

Tumours, in short, irrespective of their clinical characters, are the product of the innate power of asexual reproduction present in some measure in all tissues, except perhaps the most specialised of all. So long as the development of the tissues continues to be normal, growth and reproduction are normal too. But the development of the tissues is based upon special chemical reactions, which have been evolved in the course of ages, and are still being evolved. If these reactions are interfered with, for example, by some strange element such as arsenic entering into chemical combination with the substance of the cutaneous cells, work is interfered with, so that the skin becomes harsh and dry, development is checked, leaving the surface glazed and rough; and the young cells that are formed from time to time to replace those that are worn out, instead of attaining their true standard remain upon a lower plane of evolution—a plane that was normal for their ancestors—with the same powers of reproduction and bud formation that those ancestors possessed, and only require some local stimulus to start them on their career of growth.

In the illustrations I have given the interference with the nutrition, and the chemical reactions of the tissues has been caused by the slow continuous working of substances introduced into the body from without. Some have entered through the digestive tract, others through the lungs, and others, again, through the skin. Whether substances capable

of causing a similar effect can be produced in the body is not known. (The alimentary canal, so far as concerns the formation of toxins, is outside the body.) There are many things that suggest it, such for example as the occasional disappearance of tumours under the influence of thyroid extract; the rapidly increasing incidence of tumours as age advances; the occurrence of multiple and of symmetrical tumours, and the tendency of the same kind of tumour to occur in members of the same family at about the same time of life, or in consecutive generations; but there is no evidence comparable to that furnished by the action of arsenic and the other substances I have mentioned. Nor is it likely there will be until we know far more of the chemical changes that take place in living tissues than we do at present. In the meanwhile a great deal of valuable information might be obtained by the study first of the chemical and developmental changes that are induced in tissues by substances which it is known may lead to the growth of tumours; and then of the effects that local irritation produces in normal cells, and in those the nutrition and development of which have been interfered with by the administration of those substances.

THE KNOWLEDGE OF THE ANCIENTS REGARDING THE PROPAGATION OF DISEASE BY FLIES AND RODENTS

By JOSEPH OFFORD

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THE remarkable progress achieved in recent years in our knowledge of the civilisation and daily life of the great nations of antiquity, by means of the recovery of their records either upon monuments or in manuscripts, has provided pertinent proofs that the people of their eras were aware of the danger of diseases being distributed by means of animals and insects.

That in those early days persons comprehended, as we now do, the propagation of epidemics by means of parasites and germs, contact with which was produced by vermin and flies, is not probable. But that they had a certain well-founded, although perhaps only vague, knowledge of the foes who conveyed to mankind various maladies is now quite ascertained. So also we now possess evidence indicating that they used the best measures then familiar to them to minimise the danger. An excellent instance of this is connected with one of the Western Asiatic Cypriote and Hellenic deities, Apollo, one rôle of whose many phases was that of a pestilence god.

As Apollo Smyntheus, his cult was intimately linked with the worship of a similar deity Eshmun, the Phœnician and Punic god who by the classics was equated with Asklepios. To Apollo Smyntheus we have long known that mice, and sometimes rats, were presented as votive offerings. Statues representing this type of Apollo often depict him grasping one or other of these rodents ; or, they are carved as accessories to his figure.

That the connection of rats with disease was apparent to the ancients is demonstrated by the story related by Herodotus concerning the destruction of Sennacherib's army by a plague of them ; thus distinctly accepting the modern view as to these carriers of contagion.

A short time ago Prof. Sayce published a Carian inscription, discovered in Egypt, engraved upon a bronze figure of a rat reading " To the Rat Destroyer this rat (have I) consecrated." The figurine bears also an Egyptian text saying, " To Atum the great god giver of health and life." This identifies Atum with Apollo Smyntheus, the anti-epidemic deity.

In Egypt similar ideas were current connecting the Ichneumon with such concepts of malevolence, and numerous models of these creatures are found with other Egyptian antiquities all over the Nile valley—more frequently in the Delta. But we have not space to discuss the Egyptian part of the matter.

In the first book of Samuel the occurrence of plague is twice directly attributed to the prevalence of mice in Philistia. The Philistines—with whom we are now so much better acquainted because of the publication of Prof. Macalister's book about them—for a prophylactic accordingly dedicated offerings in the shape of mice to their god Reseph-Apollo.

This Reseph, in Phœnician, Cypriote, and other memorial inscriptions, possessed all the attributes concerning maladies of his Hellenic foster-brother, Apollo, and was also a Hittite and Syrian deity.

The votive mice were placed inside the Hebrew Ark of the Covenant with models of the so-called (translated) Emerods. Probably these were copies of the grievous boils of bubonic plague, just as models of injuries were made and deposited in the shrines of Asklepios. Complete confirmation of the custom detailed in this ancient record was provided some years ago by the discovery of silver votive mice models in a river on the Syrian Coast, near Sidon. Moreover, Punic and Phœnician monuments, as may be seen in the heliogravures of the Corpus of Semitic inscriptions, have mice carved upon them.

Just as the views of recent researchers as to the spread of disease by rodents is found to be alluded to by the Old Testament and ancient authors, likewise is the potent part played by flies and mosquitoes in the dissemination of contagion.

When Ahaz was attacked by sudden illness the special place he immediately addressed himself to, concerning a cure, was Ekron, site of a shrine of Baal-Zebub, " The Lord of Flies "; evidently because the king's malady was considered to be connected with, if not caused by, flies or insects.

We also possess, from other sources, ample corroborative

evidence that the Ekronian deity was one possessing the special powers of protecting against the conveyance of disease by flies; because in the Hebrew Talmud, an author—although apparently unaware of the purpose for which it was done—informs us that the Ekronites made miniature images of flies. These they carried on their persons, and sometimes kissed them. The Talmud connects the Baal-Berek of Shechem with Baal-Zebub of Ekron; repeating that the latter was a fly deity. The Greek equivalent Josephus gives for Zebub is Muia, which at once calls up to memory the Greek god Zeus Apomuios, at Olympia.

Among the immense number of antiquities discovered during the last ten years by French explorers at Susa, in Persia, dating from the old Elamite and Babylonian eras, was a small votive object bearing, in cuneiform characters, a conjuration against mosquitoes. It, magically, calls upon them to fly away (from the amulet's possessor) and connects the insects in some way with refuse. The precise translation, with our present knowledge, is obscure.

An illustration of this text is provided by a cylinder seal in Mr. Pierpont Morgan's collection, bearing what Dr. Theo G. Pinches, the Assyriologist, terms "the Fly Symbol" emblem of Nergal, the Mesopotamian deity of disease and of death. Other seals with a fly placed beside a god have been published by Mr. Stephen Langdon and Dr. Pinches.

Doubtless now that attention is called to these two relics other inscriptions, or pictographs, of similar character will be found among the hundreds of cuneiform incantations against maladies and misfortunes, and the thousands of Assyrio-Babylonian cylinder seals.[1]

These facts concerning ancient statements exhibiting views prevalent in antiquity as to the distribution of disease, prove—as is the case in many other matters—that the peoples of old times often possessed in some cases more than merely a dim notion of the discoveries of modern times.

[1] For such seals see *Proceedings Society of Biblical Archæology*, 1911, p. 132, and 1912, p. 158. In the *Revue Assyriologique*, 1914, p. 119, a cuneiform vocabulary containing a list of insects is translated.

THE SOLUTION OF EQUATIONS BY OPERATIVE DIVISION.—PART III[1]

BY SIR RONALD ROSS, K.C.B., F.R.S., D.Sc.

CONTENTS OF PART III

VI. (1) I OWE my profound apologies to the readers of SCIENCE PROGRESS for the length of this article—due to the necessity of writing it *à plusieurs reprises*. It is better, however, to trespass still further on their indulgence than to leave it incomplete.

The use of Taylor's theorem for the proof of the law of convergence of iteration given in Part II, p. 410, may be avoided as follows. We have, where $D = d/d0$,

$$D\,[0 + f]x_0 = [1 + f']x_0 = 1 + f'x_0\,;$$
$$D\,[0+f]^2x_0 = [1+f']x_0 \,.\, [1+f']\,[0+f]x_0 = (1+f'x_0)(1+f'x_1)\,;$$
$$D\,[0+f]^n x_0 = (1+f'x_0)\,(1+f'x_1)\,(1+f'x_2)\,\ldots\,(1+f'x_{n-1}).$$

All these factors will be numerically < 1 if all the numbers $f'x_0, f'x_1, \ldots$ are < 0 and > -2; and if this is the case $D\,[0 + f]^n x_0$ will become infinitely small as n is indefinitely

[1] Parts I and II appeared in the two previous numbers of SCIENCE PROGRESS.

increased. That is, the slope of the curve $[0 + f]^{\infty}$ will be zero and the curve will be parallel to the axis of x, not only for a specific value of x_0, but for all tracts of x within which $f'x < 0$ and > -2. The remainder of the demonstration is the same as that briefly given in Part II, p. 410.

If $f'x_0, f'x_1, \ldots$ are < 0 and > -1, all the factors will be positive and the iteration will be progressive. But if some of these numbers lie between -1 and -2, some of the factors will be negative, and the iteration may be alternating. In the latter case, however, the alternate iterants, say $x_0, x_2, x_4, \ldots$ will approach the root progressively from one side, while the others, $x_1, x_3, x_5, \ldots$ will approach it progressively from the other side, and we have in fact to deal with the iteration, not of $0 + f$, but of $[0 + f]^2$. This of course follows the same rules; but if the equation has more than one real root, x_0 must be taken so near to the required root that x_1 does not surpass the other one.

Observe that the earlier factors in the value of $D\,[0 + f]^{\infty}x_0$ do not count in comparison to those near the root; for if the latter are numerically < 1 the infinite iteration of them will still ultimately reduce the slope of the curve to zero even if many of the earlier factors are very large. Where the root sought is nearly equal to the next one, the factors near the root may be nearly unity, but for the *proper* root it must be still < 1. Where the root sought is one of a group of equal roots, the factor at the root, namely $1 + f'X$, will be unity; but the earlier factors will be < 1, and the equal roots will lie near the mean of two iterations, one taken from above and the other from below. Where $1 + f'X = -\infty$ (as in the case of $0 + f^{\frac{1}{r}}$ where $r > 1$), or where it is < -2, we can find vicarious operations in which these difficulties do not exist.

(2) Perhaps the most general proof of the law of convergence of iteration is briefly as follows. Suppose that we seek the abscissa X of the common-point of any two curves ζ and ξ. Let x_0 be any abscissa near it. Then by Part I, Section II (7) the operative ratios $\zeta x_0 // x_0$ and $\xi x_0 // x_0$ denote all the curves which pass through the points $(\zeta x_0, x_0)$ and $(\xi x_0, x_0)$ respectively. Let two of these curves be straight lines with tangents m and n to the axis of x. Then

$$\zeta x_0 // x_0 = \zeta x_0 - m x_0 + m0 \qquad \xi x_0 // x_0 = \xi x_0 - n x_0 + n0\,;$$

and the expressions on the right denote the straight lines in operative notation. If m and n are not equal, the lines meet at some point. Let x_1 be the abscissa of that point. Then (*figure* 1)

$$[\zeta x_0 - mx_0 + m\mathrm{o}]x_1 = [\xi x_0 - nx_0 + n\mathrm{o}]x_1,$$

and
$$x_1 = \left[\mathrm{o} + \frac{\zeta - \xi}{n - m}\right]x_0.$$

Next, through the same points $(\zeta x_0, x_0)$ and $(\xi x_0, x_0)$ draw tangents to ζ and ξ and let these tangents meet at the point of which the abscissa is x'_1. Then (*figure* 2)

$$x'_1 = \left[\mathrm{o} + \frac{\zeta - \xi}{\xi' - \zeta'}\right]x_0.$$

Now if $(\zeta x_0 - \xi x_0)/(n - m)$ and $(\zeta x_0 - \xi x_0)/(\xi' x_0 - \zeta' x_0)$ have the same sign, and also $n - m > \xi' x_0 - \zeta' x_0$, then x_1 lies between x_0 and x'_1. Hence (changing their order) if $\zeta' x - \xi' x$ is *always* $> m - n$ between x_0 and X, then the iterants $x_1, x_2, x_3, \ldots$ will always be less than the abscissæ of the intersections of the corresponding tangents. But very near to X the common-points of the tangents are very nearly the same as the common-point of ζ and ξ themselves; so that if this condition holds the iterants must always be $< X$ if $x_0 < X$, or be $> X$ if $x_0 > X$, but in both cases will infallibly approach X when $\zeta x - \xi x$ becomes very small—as it must do if ζ and ξ meet at all. The only exception to this is when the curves cross each other at a right angle; but in this case vicarious curves Z and Ξ can be found which have the same common-point but which cross each other at another angle. If ζ and ξ approach each other at a very small angle, as when they merely osculate, the iteration may be very slow.

We may denote the iterand by I, and its tangent at the common-point by $I'X$; so that

$$I = \mathrm{o} + (\zeta - \xi)/(n - m)\,; \qquad I'X = 1 + (\zeta' X - \xi' X)/(n - m).$$

We may call $I'X$ the *root-tangent,* and m and n the *iteration-tangents*.

Observe that the latter are arbitrary numbers which we may select as we please so as to make the iteration valid or rapid. They need not necessarily be constants since the arbitrary curves $\zeta x_0 // x_0$ and $\xi x_0 // x_0$ need not necessarily be

straight lines—though if they are not, $I'X$ will have a somewhat different form. Observe also that the iterand may be varied at each step, even for the same root; for in the expression

$$X = \frac{x_r}{x_{r-1}} \frac{x_{r-1}}{x_{r-2}} \cdots \frac{x_2}{x_1} \frac{x_1}{x_0} x_0$$

each operative factor may be different. The first theorem is included in the second.

The equation $fX = 0$ can be set in the form $\zeta X - \xi X = 0$ in many ways. If $f = \zeta - \xi$, then the iteration will be progressive if $f'x > m - n$ between x_0 and X—since $f' = \zeta' - \xi'$.

(3) The simplest setting gives what may be called *simple* or *axial iteration.* In this we take $\zeta = f$ and $\xi = 00$ (that is, the axis of x itself), while $n = 0$. If $m = -1$, we have the *unmodified* iteration of $0 + f$, in which each iterant x_1 is found by drawing a straight line from the summit of the ordinate fx_0 downward at an angle of $-45°$ till it meets the axis of x (*figure* 3). If m is some other negative number we have the *modified* iteration of $0 - f/m$, in which the straight line is drawn downward from the summit of the ordinate fx_0 at some other and more convenient angle. Thus m should be numerically large if X is near to x_0, and, *ceteris paribus*, smaller if it is further (*figure* 4). If m is numerically large, the iteration will be more slow, but more sure; and if it is small the iteration may become alternating. In both cases, for the most rapid approach near the root we should there have $m = f'X$ as nearly as possible. If f is continuous and $f(0)$ is positive, m must be negative for the odd positive roots and positive for the even ones—as $f'x$ is. See also Part II, p. 406.

The condition for progressive convergence is that $f'x$ be never algebraically $< m$ between x_0 and X—though it may be as much greater as we please; but the reader will do well to examine this particular form of iteration for itself. Let $y_n = fx_n$ be called an *iteration-ordinate* (unmodified)—so that $x_{n+1} = x_n + y_n$. Then

$$X = x_0 + y_0 + y_1 + y_2 \cdots y_\infty;$$

(and we note in passing the bearing of this theorem on the general subject of convergence, since the same result is reached

whatever value within limits x_0 may have). That is, X is the sum of x_0 and all the iteration-ordinates. Now draw chords through the summits of successive pairs of these ordinates and let t be the tangent of one of these chords. Then

$$t_n = (y_{n+1} - y_n)/(x_{n+1} - x_n) = y_{n+1}/y_n - 1 = \Delta^2 x_n/\Delta x_n.$$

These chords constitute a polygon which cannot be crossed by the curve f between x_0 and X unless $f'x$ is somewhere less than t; that is, X always lies outside the polygon if $f'x > t$. And if y_{n+1} and y_n have the same sign, $t_n > -1$. The study of the case when they have different signs (alternating iteration) must be left to the reader—the condition for convergence is briefly that $f'x > -2$ between x_0 and x, when these are on different sides of X—see (1) above.

The root is therefore always attainable by iteration, either by taking the arbitrary iteration-tangents less than the root-tangent, or by using a vicarious operation with a root-tangent greater than the proposed iteration-tangents. Thus in $\mathrm{o} \pm f/m$, the use of m may be interpreted in two ways; either f/m is a vicarious operation with a suitable root-tangent of about -1; or m is a suitable iteration-tangent. In the former case, in fact, we use a vicarious curve with a *flatter trajectory* at the root. Thus in Part I, Section III (2), p. 228, we saw that $\mathrm{o} = 1 - x - x^2$ is not suitable for simple ascending division (because the root-tangent is $-2{\cdot}236$); but on p. 230 we rendered it so by substitutions which gave a *flatter curve* with a root-tangent of $-0{\cdot}28$. The same simple explanation holds for the results of Part II, pp. 406, 407.

(4) Newton's form (Section V) may be called *simple tangential iteration* (*figure* 5). In it we change m at each step by putting $m = f'x_n$. It may go wrong at first because f/f' becomes infinite at least once between each pair of roots of f; but it is quickest at the root if this is single because then $f/f' = 1$ and $I'X = 0$. If the roots are multiple, I suggest $I = \mathrm{o} - rf/f'$ where r is the number of equal roots in the group being investigated. For suppose that $f = \psi^r.\chi$, where ψ^r consists of r equal factors of the form $a - \mathrm{o}$ and χ is the product of the remaining factors. Then

$$\begin{aligned} f &= \psi^r \cdot \chi; \\ f' &= \psi^{r-1} \cdot (r\chi + \psi \cdot \chi'); \\ f'' &= \psi^{r-2} \cdot \{r(r-1)\chi + 2r\psi \cdot \chi' + (\psi)^2 \cdot \chi''\}. \end{aligned}$$

When these operate on X the ψX factors vanish, and since

$$I'X = 1 - r[1 - f/f' \cdot f''/f']X,$$

this becomes $1 - r\{1 - (r - 1)/r\} = 0$ at the root. For example, for the two equal roots of $x^3 - 3x + 2 = 0$,

$$I = 0 - 2f/f' = (0^3 + 30 - 4)/(30^2 - 3),$$

and we have the iterants 0, 4/3, 64/63, 24004/24003 . . .; which scarcely support the common statement that Newton's method is useless for multiple and for commensurable roots.

Fourier's rule for applying Newton's method merely amounts to this, that if x_0 be taken on that side of X at which f is convex to the axis of x then the iteration will be *progressive*. But this is by no means always obligatory; for if x_0 be taken on the other side of X but sufficiently near to it, then x_1 will generally conform to Fourier's rule. Newton's is the most valuable method of arithmetic iteration we possess for equations of most kinds *provided that x_0 is near to X*. Both it and the previous method can generally be worked throughout with rational figures.

(5) Arithmetical iteration is not the subject of this paper; but in order to survey the subject in gross I will add a few remarks about other *vicarious forms* as defined in Section V (2), p. 406. These forms are generated as follows. If $fX = 0$, then $0//0 : fX = 0$. If $\zeta X - \xi X = 0$, then $0//0 : (\zeta X - \xi X) = 0$; $0//0 : \zeta X - 0//0 : \xi X = 0$; and indeed $\phi\zeta X - \phi\xi X = 0$, where $0//0$ and ϕ can have many values. The parameters of the operations $0//0$ or ϕ may be constants or functions of x, so long as we do not thus introduce new roots which clash with the original ones. These theorems contain of course the fundamental properties of equivalence.

If in the equations of subsection (2) above, $\xi = \mathbf{o}$, $m = 0$, and $n = 1$, then we have the case of *mid-axial iteration* in which $I = \mathbf{o} + \zeta - \mathbf{o} = \zeta$ (*figure* 6). This is a form much discussed by previous writers, including myself; and Dary's form given in Part II, p. 404 is an example. We put the original equation in the setting $x = \zeta x$—which can be done in many ways—and then find the common-point of the mid-axis $\mathbf{o}$ (or $y = x$) and ζ. The tangential form of it, namely $I = \mathbf{o} + (\zeta - \mathbf{o})/(1 - \zeta')$ is quick near the root since $I'X = 0$. Mid-axial iteration has advantages in some special cases and disadvantages in others.

Root-tangents are always required to test convergence and can generally be put in simple forms owing to the disappearance of part of the tangential at the root. For example

$$D\,[0+0//0:f]\,X = 1 + f'X\,.\,D0//0:0\,;$$
$$D\,[0//0:\zeta]\,X = f'X\,.\,D0//0:\zeta X \text{ (or } \xi X).$$

The iteration is quickest near the root when $I'X = 0$, and we can select the parameters of an iterand so as to give this result. For example let $I = 0\ (\zeta + m)/(\xi + m)$. Then $I'X = 1 + Xf'X/(\xi X + m)$; and if this $= 0, m = -(\xi X + Xf'X)$. In using this fractional iterand, we obtain the odd positive roots by putting the absolute term (positive) in ζ, and the even roots by reversing the fraction. We can also put all the terms with one sign in ζ and those with the other sign in ξ. Legendre's *fonctions omales* are of this class, but he does not suggest the modifier m, which is necessary for rapidity.

Many vicarious operations may be interpreted in terms of iteration-tangents. Thus $I = 0\zeta/\xi$ may be written $I = 0 + 0(\zeta - \xi)/\xi$; showing that the iteration-tangent from ζ is zero, while that from ξ is $\xi/0$.

Other settings which may be occasionally useful for transcendental and high-power equations, especially with the aid of partial roots, are

$$I = 0\sqrt[m]{1 \pm f/\mu}, \qquad I'X = 1 \pm mXf'X/\mu X\,;$$
$$I = 0 + m\log\zeta/\xi, \qquad I'X = 1 + mf'X/\zeta X \text{ (or } \xi X).$$

We may add many exponential, trigonometrical forms, etc.

VII. (1) When an equation $fx = 0$ is arranged in the form $\zeta x - \xi x = 0$, each arrangement of this kind may be called a *setting* ; ζ and ξ may be called *partial operations* ; and the roots of $\zeta x = 0$ and of $\xi x = 0$ may be called *partial roots*, the roots of $fx = 0$ being the *actual roots*. Now definite relations exist between the partial and the actual roots—relations which often serve to locate the latter easily and closely. I do not know whether the subject has ever been fully treated, but it is suggested by the fact that the inverts given by operative division are always functions of partial roots ; and it must therefore be briefly dealt with here.

A partial root may be said to *refer to* an actual root when no other actual root or other partial root of the same setting comes between them. Then the following propositions can

be easily verified from *figure 7*—all the curves being continuous and the roots real roots.

(*a*) A partial root can refer only to one actual root, if to any. For if it lies between two actual roots, it must be separated from one of them by another partial root, either of the same partial curve or of the other one.

(*b*) If a partial curve possesses only one positive root not zero, then if this partial root lies between two actual roots it must refer to one of them.

(*c*) If $\zeta(0)$ and $\xi(0)$ have different signs, the odd positive roots of both counted upwards will refer only to odd positive actual roots; and the even positive roots of both, only to even positive actual roots. But if they have the same sign this will hold true only of the function which is numerically the greater when $x = 0$, while the converse will hold true regarding the other partial curve, zero roots being neglected.

(*d*) The question whether a partial root is ***prospective*** or ***retrospective***, that is, less or greater than the actual root to which it refers, can be determined by the sign of fx at the partial root, since fx changes sign at each actual root.

(2) With these and similar theorems, partial roots derived even from a single well-chosen setting will often give easy analyses of equations (*Note* IV); but our present subject requires rather the use of partial roots of different settings. I have space only for power-series equations. Consider

$$fx = p_0x^n + p_1x^{n-1} + \ . \ . \ . + p_{n-1}x + p_n = 0.$$

This may be set in many ways by putting any two of the terms on one side of the equation, and the other terms on the other side. Such a pair of terms may be called a ***critical pair***; the term with the higher power of x may be called the ***superior term***; and the real root or roots (other than zero) of the pair may be called ***critical points***. Thus if $p_rx^{n-r} + p_qx^{n-q}$ be a critical pair, the corresponding critical point or points will be $(-p_q/p_r)^{1/(q-r)}$; and this cannot possess more than one real value of one sign. Then we have the following propositions.

(*e*) Every positive critical point which lies between the least and the greatest positive actual roots must refer to one of the positive actual roots next to it. From (*a*) and (*b*) of the previous subsection.

(*f*) A critical point is prospective if at that point fx has the

same sign as the inferior critical term as placed in the original equation: otherwise it is retrospective. From (*d*) of the previous subsection.

(*g*) If the highest term of the equation be positive, all positive critical points derived from pairs of which the inferior terms are negative refer to odd positive actual roots, counting from the greatest root downwards; otherwise the positive critical points refer to the even positive actual roots. This is merely a restatement of proposition (*c*), because by Descartes' Rule, if p_0 and p_n have the same sign there may be an even number of positive actual roots, and if they have different signs there must be an odd number of such roots.

Thus if *f* has only one change of sign, all the positive critical points are derived from pairs with negative inferior terms and must therefore refer each to an odd positive root; that is, they all refer to the same root—the first and only positive root. If *f* has two changes of sign, the critical points from pairs with negative inferior terms refer to the first and greater root; and the others refer to the second and lesser root. If there are three changes of sign the critical points from pairs with negative inferior terms must all refer either to the first and greatest or to the third and least root; and the others to the middle root. And so on—equal roots being counted separately.

Note that by (*e*) critical points which are greater than the greatest root or less than the least root need not necessarily refer to any root at all; and that as roots become unreal in pairs, their doing so does not affect the truth of the propositions given above.

(3) I have space only for a few examples. If only the highest term of the equation be positive, all the critical points refer to the only root and, by (*f*), are all prospective—so that the root is greater than the greatest of them. Thus in $x^3 - 2x^2 - 2x - 3 = 0$, the critical points are 2, $\sqrt{2}$, and $\sqrt[3]{3}$, and the first of these is nearest the root ($= 3$). In $x^3 - x^2 - 81x - 90 = 0$ the critical point 9 is nearest the root ($= 10$). In Newton's equation $\sqrt[3]{5}$ is nearest the root ($= 2{\cdot}09\ldots$). Observe therefore that *the nearest critical point should be a convenient base for iteration*; and, secondly, that in the case of Newton's equation it is *the subject of the invert given by descending operative division*—compare Part I, p. 232, and Part II, pp. 394, 409.

If only the last and absolute term be negative all the critical points refer to the only root but are all retrospective—so that the root is less than the least of them. In $x^4 + 100x^3 + x - 1 = 0$, the root ($= 0{\cdot}095 \ldots$) is less than $0{\cdot}1$. By putting $1/z$ for x we transform the first of these cases into the second and *vice versa*, but get no new information.

Where f consists of a set of positive terms followed by a set of negative ones, the root lies between the greatest prospective and the least retrospective critical point. Thus in

$$\begin{array}{cccc} x^3 + & 3x^2 - & 2x - & 5 = 0\,; \\ & & \sqrt{2} - & \sqrt[3]{5} - \\ & & 2/3 + & \sqrt{5/3} + \end{array}$$

the root ($= 1{\cdot}33 \ldots$) lies between $\sqrt{5/3}+$ and $\sqrt{2}-$. (Observe the method used for denoting prospective and retrospective critical points.)

For two changes of sign, consider

$$\begin{array}{cccc} x^3 - & 3x^2 - & 2x + & 5 = 0. \\ & 3 + & \sqrt{2} + & \\ & & & \sqrt{5/3} - \\ & & & 5/2 - \end{array}$$

The critical points under negative terms refer to the odd and greater root, and those under the positive term to the even root—if these really exist. Thus the former ($= 3{\cdot}128 \ldots$) is greater than 3, and the latter ($= 1{\cdot}201 \ldots$) is less than $1{\cdot}29 \ldots$ —see Part I, p. 232, and Part II, p. 395, and note that the inverts are operations performed upon the appropriate critical points. In the equation $x^2 - x + a = 0$, dealt with in Part II, p. 407, the even critical point a is the subject both of the iteration and of the ascending operative invert—*both of which must therefore refer to the even and lesser root*, counting downwards. For the negative roots of Newton's equation $x^3 - 2x + 5 = 0$, we find that the odd critical point is $\sqrt{2}-$ and the even one is $\sqrt[3]{5}+$; that is the odd root should be less than the even one—but this is impossible and both roots must therefore be unreal.

For three changes of sign consider $x^3 - 16x^2 + 65x - 50 = 0$, of which the roots are 1, 5, and 10. The even critical point $65/16+$ refers to the middle root; the odd point $16-$ to the

greatest root, and the odd points $\sqrt[3]{50}-$ and $50/65+$ to the least root. In the equation

$$x^5 - 15x^4 + 85x^3 - 225x^2 + 274x - 120 = 0,$$

of which the roots are 5, 4, 3, 2, 1, the critical points derived from successive pairs of terms might appear to refer to each root in succession. The point $85/15-$, however, is separated from the root 4 by the root 5, and can therefore refer to no root at all—as indicated by the qualifying condition in proposition (e).

(4) Some more theorems on critical points will be given in Notes II and III and Figures 8 and 9. Two of these are so frequently useful for the analyses of equations that they should be mentioned here. If all the roots of an equation are positive and real, the critical points derived from successive pairs of terms must be in diminishing order of magnitude. In fact their successive ratios must be greater than the ratios of the corresponding binomial coefficients—which is indeed connected with Newton's Rule. For example, $x^4 - 2x^3 + 12x^2 - 11x + 10 = 0$, that is $(x^2 - x + 1)(x^2 - x + 10)$, has no roots. But conversely the roots need not all be real even if these conditions hold. If the equation consists of groups of terms of alternate signs, the former rule, at least, applies *to the partial roots of successive pairs of groups* (Example P).

VIII. (1) We are now able to return to the proper subject of this paper, the inverts obtained by operative division. In Sections III and IV we obtained a number of different inverts, either by ascending or by descending division, by taking $fx = 0$ in an ascending or in a descending series, and then dividing both sides of the equation by different powers of x. We have now to find in conclusion, (1) to which root each invert refers; (2) under what conditions it is convergent; and (3) what is its geometric interpretation. At the end of Section IV it would have been difficult to answer these questions, but the intervening sections will help us to make the attempt.

Let the proposed equation be

$$fx = p_0x^n + p_1x^{n-1} + \ldots + p_qx^{n-q} \ldots + p_{n-1}x + p_n = 0$$

Suppose p_0 always positive and let p_q be one of the negative coefficients. Then, dividing algebraically by p_0x^{n-q}, we have

$$x^q + p_1/p_0 \cdot x^{q-1} + \ldots + 0x^0 \ldots + p_{n-1}/p_0 \cdot x^{-n+q+1} +$$
$$+ p_n/p_0 \cdot x^{-n+q} = - p_q/p_0.$$

If X_q is the root to which this series refers, then by Part II, p. 397, descending operative division gives us

$$X_q = g - \frac{1}{q}\frac{p_1}{p_0} - \left\{\binom{1}{q}_1 \frac{p_2}{p_0} - \binom{1}{q}_2 \left(\frac{p_1}{p_0}\right)^2\right\} g^{-1} - \text{etc.},$$

where $g = \sqrt[q]{-p_q/p_0}$ for short. It is convenient to employ the notation suggested in the same section, p. 398, according to which $\psi_q x$ denotes a descending function of which the leading and highest term contains x^q; and $\phi_q x$ denotes an ascending function of which the leading and lowest term contains x^q. Then if

$$\psi_q x = -p_q/p_0, \qquad X_q = [\psi_q]^{-1}(-p_q/p_0);$$

with similar equations for ϕ_q.

If p_s, p_t, ... are other negative terms, we can also divide fx by $p_0 x^{n-s}$, $p_0 x^{n-t}$, ... and obtain similar inverts by descending operative division for X_s, X_t, ... where these roots may or may not be the same as X_q. Thus we may consider as many descending inverts as there are negative terms in fx; and may observe that the subjects of all will be positive real numbers. But if p_1 be a negative coefficient, we can obtain yet another invert from fx/x^n, namely $[\psi_{-1}]^{-1}(-p_0/p_1)$. On examining this, however, we find that it is the same as $[\psi_1]^{-1}(-p_1/p_0)$, with a rearrangement of terms.

By writing $fx = 0$ in an ascending series $\phi_0 x = 0$, putting p_n positive and dividing by it multiplied into the powers of x contained in terms which are now negative, we obtain a set of ascending inverts. If p_{n-1} is now negative, we have got another ascending series (without division of $\phi_0 x$ by any power of x), namely $[\phi_1]^{-1}(-p_n/p_{n-1})$—which is indeed the series given by simple ascending division as shown in Part I, Section III (3), p. 230. Here again, however, this proves to be the same series as $[\phi_{-1}]^{-1}(-p_{n-1}/p_n)$.

If, in any of the descending settings such as $\psi_q x = -p_q/p_0$ we put z^{-1} for x_1 we obtain ascending settings in z which give ascending inverts on the same subjects such as $-p_q/p_0$. These prove to be, by the use of the multinomial theorem, nothing but the algebraic reciprocals of the descending inverts in x—as was to be expected since the roots of z must be the algebraic reciprocals of the roots of x. Compare also Part II, Example H, p. 400, where we saw that $\phi_{-q}^{-1} \cdot \psi_q^{-1} = 1$. Similarly

all the ascending inverts of the equation in x are the algebraic reciprocals of the descending inverts of the equation in z.

If p_0 and p_n have the same sign (so that there may be an even number of real positive roots), then there will be as many descending as ascending inverts, all with positive subjects; so that there will be twice as many inverts as the number of negative terms in fx. If p_0 and p_n have different signs (so that there must be an odd number of real positive roots), the invert $[\psi_n]^{-1}(-p_n/p_0)$ will be found to be the same as $[\phi_n]^{-1}(-p_0/p_n)$; so that the total number of inverts will equal the total number of terms less one.

(2) On examining the series in Part II, p. 397, we see that each operative invert, descending or ascending, consists of a series of positive or negative *integral* powers of o operating on the appropriate *fractional* power of the subject. Indeed, the remarks under Example F, p. 399, show that

$$[\psi_q]^{-1}(-p_q/p_0) = [\sqrt[q]{\psi_q}]^{-1}\sqrt[q]{-p_q/p_0};$$

the operation $[\sqrt[q]{\psi_q}]^{-1}$ being the series of integral powers of o just mentioned. Now $\sqrt[q]{-p_q/p_0}$ is a *critical point* as defined in the previous section. Thus

Each invert obtained by operative division consists of an operation performed upon a critical point.

Hence we infer that *each invert refers to the same root of the equation as the one to which the critical point refers.*

(3) Next it remains to be shown that *each invert obtained by operative division is the algebraic expression of various infinite iterations based upon the same critical point.*

Three special examples of this were given in Part II, Section V (3)—in two of which the same invert was proved to be generated by two different iterands. We now examine the general case.

First observe that in the equation $\psi_q x = -p_q/p_0$ we have hitherto brought the whole of the absolute term to one side of the equation, so that ψ_q does not contain one. But we may if we please add any arbitrary number to both sides of the equation. We may call this a *modifier* and denote it by $\pm m$; so that if $\psi_q x$ is now supposed to contain m, the equation may be written

$$\psi_q x = m - p_q/p_0 = y \text{ (say)}.$$

This is the same thing as *splitting the subject*, as referred to in Part II, p. 401. Hence if I be the *generating iterand* and $g = \sqrt[q]{y}$, we must have by the preceding subsection

$$I^{\infty}g = [\sqrt[q]{\ }\psi_q]^{-1}g.$$

Any solution of this functional equation in I may be verified in two ways. First we must show that the iterands Ig, I^2g, I^3g, . . . constantly approximate to the series on the right. Secondly we must show that

$$[I]\,[\sqrt[q]{\ }\psi_q]^{-1}g = [\sqrt[q]{\ }\psi_q]^{-1}g\,;$$

because if $I^{\infty}g$ reaches a given value, $II^{\infty}g$ must reach the same one.[1]

I will first show that the simple or axial form of iteration, that is, of $0 \pm f/M$, as described in Section VI (3), will generate *all* the series given by operative division if the subject and the modifier be properly selected. We shall see that

$$X_q = I^{\infty}g = [0 - f/qg^{n-1}]^{\infty}g = [\sqrt[q]{\ }\psi_q]^{-1}g,$$

where $fx = 0$ is the original equation of the nth degree (in descending terms and with $p_0 = 1$), which is divided by x^{n-q} in order to obtain the setting $\psi_q x = m - p_q/p_0$, which yields the invert $[\sqrt[q]{\ }\psi^q]^{-1}g$. Note that I is a rational operation, the modifier qg^{n-1} being a constant; and that this modifier contains a power of the base g.

To save space, I will verify this proposition in a special case —when $q = 3$, say, and write the setting

$$x^3 - bx^2 - cx - m - cx^{-1} \ldots - p_n x^{-n+3} = d - m = y = g^3.$$

Then

$$I = 0 - 1/3g^{n-1}\,.\,\{0^n - b0^{n-1} - c0^{n-2} - (y+m)0^{n-3} - \ldots - p_n\}$$
$$= 0 - 1/3g^{n-1}\,.\,\{(0^n - g^3 0^{n-3}) - b0^{n-1} - c0^{n-2} \ldots - m0^{n-3} \ldots - p_n\};$$

$$Ig = g + \frac{1}{3}b + \frac{1}{3}cg^{-1} + \frac{1}{3}mg^{-2} + \frac{1}{3}eg^{-3} + \ldots \frac{1}{3}p_n g^{-n+1}\,;$$

$$I^2g = Ig - 1/3g^{n-1}\,.\,\{(Ig)^n - g^3(Ig)^{n-3} - b(Ig)^{n-1} - c(Ig)^{n-2} - \ldots - p_n\}$$
$$= g + \frac{1}{3}b + \left(\frac{1}{3}c + \frac{1}{9}b^2\right)g^{-1} +$$
$$+ \left\{\frac{1}{3}m - (n-2)\frac{1}{9}bc - (3n-7)\frac{1}{27}b^3\right\}g^{-2} \ldots$$

[1] Conversely we can find iterands by solving the functional equation. This may be done by the operative division of a given invert by itself, the *form* of the quotient I being assumed.

It will be seen that this is generating the invert given in Part II, p. 397, namely

$$[\sqrt[3]{\psi_3}]^{-1}g = g + \frac{1}{3}b + \left(\frac{1}{3}c + \frac{1}{9}b^2\right)g^{-1} + \left(\frac{1}{3}m + \frac{1}{9}bc + \frac{2}{81}b^3\right)g^{-2} + \\ + \frac{1}{3}eg^{-3} + \text{etc.}$$

$$= t_1 + t_0 + t_{-1} + t_{-2} + t_{-3} + \ldots \text{(say)}.$$

Thus Ig gives $t_1 + t_0$ correctly; I^2g adds t_{-1} correctly; I^3g adds t_{-2}; and so on. The work will be much facilitated by Table I, which gives the first few terms of the ordinary multinomial expansion of (ψ_n) and $(\phi_n)^r$.

We may verify this result by the second method mentioned above, according to which I operating once on an invert generated by it reproduces the same invert; and the work will be facilitated by the formula for $(\psi_n^{-1})'$ in Part II, p. 399. This is what was to be expected. For if the invert rightly expresses the root, whichever root it may be, then the result when f operates upon it should be zero. In fact we shall find, not only that $[\psi_q + p_q][\psi_q]^{-1}(-p_q) = 0$, as indicated in Part II, Example I, p. 400, but that more generally

$$[\psi_r + p_r][\psi_q]^{-1}(-p_q) = 0,$$

if $p_0 = 1$. This holds when $r = n$; so that $I = 0$ when it operates upon any of the appropriate inverts.

(4) But many other iterands will have the same result; and it is necessary to mention them in order to show that all the forms of iteration described in Section VI (3, 4, 5) will generate the same series if the base and modifier be properly chosen. For axial iteration, the general theorem is that

$$I = 0 - \frac{\psi_r - g^r}{qg^{r-1}}$$

operating on g will generate $[\sqrt[q]{\psi^q}]^{-1}g$. If $r = n$ and $q = 3$, this is the example just examined. Or we may take $r = q$. Whatever the value of r, Ig will be the same as when $r = n$, and I^3g will give the series right to about five major terms. But unless $r = n$, $\psi_r - g^r$ will contain negative powers of 0 which will require irrational expansions of the subject.

For Newton's form of axial iteration take $I = 0 - (\psi_q - g^q)/\psi_q'$ and iterate on g as before. Thus for the example used above, on expanding the fraction by algebraic division, we have

$$I = o - \{o^3 - bo^2 - co - (m + g^3) - eo^{-1} - \dots\}/\{3o^2 - 2bo - c + eo^{-2} + \dots\}$$

$$= o - \frac{1}{3}o + \frac{1}{9}b + \left(\frac{2}{9}c + \frac{2}{27}b^2\right)o^{-1} + \left\{\frac{1}{3}(m + g^3) + \frac{5}{27}bc + \frac{4}{81}b^3\right\}o^{-2} + \left\{\frac{4}{9}e + \frac{2}{27}c^2 + \frac{4}{27}b^2c + \frac{2}{9}b(m + g^3) + \frac{8}{243}b^4\right\}o^{-3} + \left\{\frac{5}{9}f + \frac{7}{27}be + \left(\frac{1}{9}c + \frac{4}{27}b^2\right)(m + g^3) + \frac{9}{81}bc^2 + \frac{28}{243}bc^3 + \frac{16}{729}b^4\right\}o^{-4} + \dots;$$

$$Ig = g + \frac{1}{3}b + \left(\frac{1}{3}c + \frac{2}{9}b^2\right)g^{-1} + \left(\frac{1}{3}m + \frac{1}{3}bc + \frac{4}{27}b^3\right)g^{-2} + \left(\frac{1}{3}e + \frac{2}{9}bd + \frac{1}{9}c^2 + \frac{8}{27}b^2c + \frac{8}{81}b^4\right)g^{-3} + \dots;$$

$$I^2g = g + \frac{1}{3}b + \left(\frac{1}{3}c + \frac{1}{9}b^2\right)g^{-1} + \left(\frac{1}{3}m + \frac{1}{9}bc + \frac{2}{81}b^3\right)g^{-2} + \left(\frac{1}{3}e - \frac{2}{27}bd + \frac{1}{27}c^2 - \frac{4}{81}b^2c - \frac{9}{81}b^4\right)g^{-3} + \dots;$$

which is right to four major terms. Some detail has been given in order to show the important fact that Newton's iterand generates precisely the same descending invert. (Care must be taken with the term g^3 in I.)

But we shall obtain the same result if we use only the first term or terms of ψ'_q in the denominator of the fraction in I.

For mid-axial iteration we can take

$$I = o + g - \sqrt[3]{o^3 - bo^2 - co - m - eo^{-1} - \dots}$$

$$= g + \frac{1}{3}b + \left(\frac{1}{3}c + \frac{1}{9}b^2\right)o^{-1} + \left(\frac{1}{3}m + \frac{2}{9}bc + \frac{5}{81}b^3\right)o^{-2} + \left(\frac{1}{3}e + \frac{2}{9}bd + \frac{1}{9}c^2 + \frac{5}{27}b^2c + \frac{10}{243}b^4\right)o^{-3} + \dots;$$

$$I^2g = t_1 + t_0 + t_{-1} + t_{-2} + t_{-3} + \dots;$$

which is right to five major terms. Or we may take Dary's form:

$$I = \sqrt[3]{g^3 + bo^2 + co + m + eo^{-1} + \dots}$$

$$= g + \frac{1}{3}g^{-2}(bo^2 + co \dots) - \frac{1}{9}g^{-5}(bo^2 + co \dots)^2 + \frac{5}{81}g^{-8}(bo^2 + co \dots)^3 + \dots;$$

$$Ig = g + \frac{1}{3}b + \left(\frac{1}{3}c - \frac{1}{9}b^2\right)g^{-1} + \left(\frac{1}{3}m - \frac{2}{9}bc + \frac{5}{81}b^3\right)g^{-2} + \ldots;$$

$$I^2g = t_1 + t_0 + t_{-1} + \left(\frac{1}{3}m + \frac{1}{9}bc - \frac{10}{81}b^3\right) + \ldots$$

(5) For ascending division, write the original equation

$$\phi_{-n}x = p_n x^{-n} + p_{n-1}x^{-n+1} + p_{n-2}x^{-n+2} + \ldots + p_0 = 0.$$

Dividing by $p_n x^{-n+q}$, and adding a modifier, we obtain

$$\phi_{-q}x = m - p_{n-q}/p_n = y; \qquad X_q = [\phi_{-q}]^{-1}y.$$

For simple modified iteration, we find that, after dividing by p_n,

$$I = 0 + \phi_{-n}/qg^{-n-1}$$

generates the series, where $g^{-q} = y$. This is the same as the formula for generating descending inverts by simple iteration, except that the signs of n and q are changed; and the results are also given by putting z^{-1} for x in the descending equation.

Both for ascending and for descending division, the cases when $q = 0$ lie somewhat outside these formulæ as they deal with the inverts of ϕ_1 and ψ_{-1}. But as stated in subsection (1) above, these are the same as ϕ_{-1}^{-1} and ψ_1^{-1} (except that we cannot use modifiers); and I have no space to discuss the matter here. But simple ascending division as described in Part I requires this setting and is so important that the generating iterands must be noted. Let

$$\phi_0 x = a - bx + cx^2 - dx^3 + \ldots = 0;$$

then it will easily be found that

$$X = [0 + \phi_0/b]^\infty A = A + CA^2 + (2C^2 - D)A^3 + \\ + (5C^3 - 5CD + E)A^4 + \ldots,$$

which is the operative invert when A, B, C, ... = $a/b, b/b, c/b \ldots$

Similarly for Newton's iterand $0 - \phi_0/\phi'_0$, we have by ascending algebraic division

$$I = A + 2AC0 + (4AC^2 - 3AD - C)0^2 + (12ACD + 8AC^3 + \\ + 4AE - 2C^2 - 2D)0^3 + (16ACE + 16AC^4 - 36AC^2D + \\ + 9AD^2 - 5AF + 7CD - 4C^3 - 3E)0^4 + \ldots;$$

$$IA = A + CA^2 + (2C^2 - D)A^3 + (8C^3 - 11CD + E)A^4 + \ldots;$$

$$I^2A = A + CA^2 + (2C^2 - D)A^3 + (5C^3 - 5CD + E)A^4 + \ldots$$

The result will also be the same if we use only the first two terms of ϕ'_0, namely $-b + 2c0$—which is as it ought to be.

(6) Lastly we have to enquire when each invert is *convergent*. We have seen that all the inverts are generated by various iterations—the corresponding geometric constructions of which are obvious and easy (Section VI); and may therefore suppose that the conditions of convergence will be equally clear. But it will be asked at once how this can be the case, since different iterations, even towards the same root, have different conditions of convergence. The answer appears to be briefly that most of the generating iterands are irrational, as they consist of roots or ratios developed in infinite series which themselves have conditions of convergence in addition to those demanded by the iteration. I have space therefore to deal only with the rational iterands, namely $0 - \psi_n/qg^{n-1}$ and $0 + \phi_0/b$.

An iteration may diverge either at the critical point from which it starts or at the root which it should reach—in both cases owing to the use of an unsuitable angle of iteration. We may therefore talk of *ultimate* convergence (at the root), and of *proximate* convergence (near the critical point or base).

Regarding ultimate convergence, the iterand gives immediate and definite information, concerning both arithmetical and algebraic iteration—information which cannot easily be otherwise obtained. For since the series represents the arithmetical iteration we may, I suppose, assume that the former will be ultimately convergent or divergent when the latter is so. Now by Sections V and VI, the iteration of $I = 0 - f_n/qg^{n-1}$ will succeed if $I'X$ lies between $+1$ and -1, and will be most rapid *near the root* if $I'X = 0$. That is, $1 - f'_n/qg^{n-1}$ must lie within these limits; that is, qg^{n-1} must be greater than $\frac{1}{2}f'_nX$ and should nearly $= f'_nX$.

If we do not use a modifier, then $g = K_q$, the critical point, and we may not be able to fit a convergent iteration to it. But by the use of the arbitrary modifier m we can adopt such an angle of iteration as will ensure both the arithmetical and the algebraic iteration converging at the root, and shall in fact employ the simple modified iteration described in Section VI (3), which can be always made to succeed. We find m from the equations ($p_0 = 1$)

$$g = \sqrt[q]{y} = \sqrt[q]{m + K_q^q} = \sqrt[n-1]{f'_nX/q}$$

for the quickest possible iteration at the root. Of course we do not know $f'X$ exactly; but we can always roughly estimate its magnitude sufficiently to ensure that g shall be large enough.

For the simple ascending iterand $0 + \phi_0/b$ we observe that $b = \phi'_0(0)$—the tangential of ϕ_0 at the point zero. This should be nearly the same as the tangential at the least positive root ϕ'_0X, and will be so if we move the origin sufficiently close to that root—which is the simple explanation of the rules given in Part I, Section III.

Applications are given in the Examples.

(7) Hence we may apparently be certain that if the ultimate convergence of the arithmetical iteration is assured the corresponding operative invert will really and correctly represent the required root *if the whole of the series be considered.* But this does not mean that the first few terms of the series need necessarily approximate, even roughly, to the value of the root; not even indeed when the generating arithmetical iteration does so. For in the series the terms of $Ig, I^2g, I^3g. \ldots$ are rearranged under progressive powers of g. All the terms of Ig, namely,

$$Ig = g - p_1/q - p_2/q \cdot g^{-1} - p_3/q \cdot g^{-2} - \ldots$$

(for the equation $x^n + p_1x^{n-1} + \ldots + p_n = 0$) are generally, it is true, contained in the first few terms of the invert; but if we take only these first few terms, the higher terms of I^2g, $I^3g, \ldots$ will be cut off, and the beginning of the series may not be sufficient.

It comes to this, then, that, when we wish to obtain an approximation from the first few terms only, then the value of g in comparison with the coefficients $p_1, p_2, p_3, \ldots$ should be as large as possible for descending inverts and as small as possible for the ascending ones—that is, the successive major terms should diminish rapidly. Or else the coefficients of g in the invert should do so. These matters will be further discussed in Note VI.

(8) *Conclusion.*—I have now concluded my task of endeavouring to show how each invert generated by operative division is also generated by iteration upon the proper critical point—thus indicating when each series is ultimately convergent and to which root it approximates. But, though I have occupied

much more space than was originally allotted, I am obliged entirely to abandon many important propositions—concerned with rational, equal and unreal roots, Lagrange's, Horner's, Weddle's, Gräffe's, and other methods, Tschirnhausen's process, the algebraic study of the inverts themselves, much work on partial roots, and above all the relation of the inverts (ϕ^{-1} and ψ^{-1}) to the general operative multinomial theorem (ϕ^n and ψ^n) —not to mention the solution of functional, difference, differential, and integral equations by operative division (see Verb Functions, p. 71). A few notes are, however, added at the end.

The great length of the article is due to the necessity of touching not only upon arithmetical iteration and critical points, but also upon the o-algorithm. But regarding this last matter, the article does no more than put it to the most elementary uses, and no further attempt is made to indicate the general employment of the algorithm either in Algebra or Geometry or in the Calculus.

The subject of this article is of interest, not only in connection with the Theory of Equations, but because the whole of Algebra from multiplication and involution upwards is based upon operative involution (iteration) and because our results emphasise the fact that the processes of formal or pure operative algebra divorced from subject are simply mechanical —in inversion as when direct—see Part II, p. 403.

Our principal result has been, I think, that iteration and operation division yield somewhat new methods for generating, transforming, and summing certain infinite series and for determining their ultimate convergence. How many series belong to this class remains to be seen.

I am indebted to Mr. Walter Stott and Mr. P. E. B. Jourdain for references to certain literature on arithmetical iteration (Part I); but this cannot be examined in time for this paper, and I hope therefore to deal with it in a subsequent note. My article in *Nature*, October 29, 1908, was written quite independently, and the method of solution there described has been ably set forth in Dr. W. P. Workman's *Memoranda Mathematica* (Oxford: Clarendon Press, 1912, p. 46).

NOTES

I. *The ϕ^0 Fallacy.*—It is curious that in these days when there is so much analysis of the fundamental ideas of mathematics, the fallacy that $\phi^0, \Delta^0, \Sigma^0, D^0, E^0, \ldots$ all equal numerical unity should have been overlooked—Part I, p. 221. We *define* that $\phi^0 x = x$, $\Delta^0 x = x, \ldots$, but then immediately assume that $\phi^0 x = 1 \times x$, $\Delta^0 x = 1 \times x, \ldots$ Why not suppose that $\phi^0 x = 0 + x$ or $= x^1$? We have evidently been misled by association of ideas, since, for numbers, juxtaposition implies multiplication. But for operations, juxtaposition implies that the superior element *operates upon* the inferior one, and this must still hold regarding ϕ^0, as well as regarding ϕ^h when h is infinitesimally small. Hence ϕ^0 must be, not a number, but an operator—and is in fact **o**; and the recognition of this idea will some day open a new chapter in mathematics.

II. *The Relations between Successive Roots and Critical Points* (incompletely studied in Section VII) are best indicated briefly as follows. From the algebraic point of view a rational integral function is the algebraic product of a number of linear or quadratic factors; from an operative point of view it is an operative product of operative factors—that is,

$$fx = [p_n + x\mathbf{o}][p_{n-1} + x\mathbf{o}] \ldots [p_2 + x\mathbf{o}][p_1 + x\mathbf{o}]p_0.$$

The effect of operating with $x\mathbf{o}$ upon any function, that is, of multiplying it by x, is to modify its current ordinates without disturbing either its roots or its critical points, and also to bend the curve towards the origin and thus to add a new root, zero. The effect of operating with $p + x\mathbf{o}$ is first to do this and then to shift the entire curve away from the x-axis to a distance p. And if fx is a rational integral function it results from a succession of such operations.

If $p_0, p_1, \ldots p_r$ are all positive, the resulting curve commences, when $x = 0$, at the value p_r, has only positive terms, increases for all positive values of x, and has no positive root. Call this set of terms A. Next let p_{r+1} be negative; then a positive root and a set of positive critical points are immediately introduced. If $p_{r+2}, p_{r+3}, \ldots$ all continue negative, no new positive root is introduced, but the original one is, so to speak, pushed further and further from the origin along the positive x-axis and also new critical points, all referring to this single root, are formed by each new term. Call this set of negative terms B. Next form another set of positive terms C. Then a second root between the origin and the first root may be introduced if the addition of the terms C does not push the curve too far away from the axis; and also there will be in any case another set of critical points between the C-terms and the B-terms. But if C is too large, not only will the second root not be formed, but the first one will disappear. And the repetition of these processes continues to produce the same results. Thus the number of positive roots must either equal the number of changes of sign or fall short of it by an even integer—which contains Descartes' Rule, of course.

Again, arrange fx in an ascending series so that

$$fx = A - B + C - D \ldots,$$

where $A, B, C, \ldots$ are successive sets of terms of the same sign and A now contains the absolute term. Draw diagrams of the curves A, $A - B$, $A - B + C$, etc., as in Figure 8, on the supposition that the variable possesses only positive values.

We observe first that when x is small enough A is nearly the same as the absolute term; that $B, C, D, \ldots$ which have no absolute term, are all nearly zero; and that $A > B > C > D \ldots$ But when x is large enough $A < B < C < D, \ldots$

because the powers of x in these sets are of increasing order of magnitude. Hence the equations $A-B=0$, $B-C=0$, $A-C=0$ must each have a root and, by the previous proposition, only one root—which we may denote by AB, BC, AC. . . . The curve A begins at the absolute term and, if it contains powers of x, constantly increases, reaches positive infinity, and has no root. The curve $A-B$ also begins at the absolute term; is always less than A, which it follows closely at first; but then bends downward, crosses the axis of x at AB, and finally becomes negative infinity. The curve $A-B+C$ begins at the absolute term; never again touches $A-B$; at first lies between A and $A-B$; but crosses A when $A=A-B+C$, that is, at the point BC; and finally becomes positive infinity. Obviously if C is small enough, $A-B+C$ may cross the axis of x at two points; the first of which lies near AB and the second of which lies near BC, both the points of crossing (which are the roots of $A-B+C=0$) lying between AB and BC: but if C is too large both these roots will disappear. Similarly the curve $A-B+C-D$ begins at the absolute term; never again touches $A-B+C$; at first lies between $A-B$ and $A-B+C$; but crosses $A-B$ when $A-B=A-B+C-D$, that is, at the point CD; and finally becomes negative infinity. It must therefore have one root and may have three. Similarly the curve $A-B+C-D+E$ lies between $A-B+C$ and $A-B+C-D$ until it crosses the former at the point DE. And so on.

This construction makes it obvious that if all the possible roots exist we must have roughly not only that $AB<BC<CD$, . . . but that there must be sufficient intervals between these points to give room for the roots, so to speak. Moreover, all the roots must lie between the least and the greatest of these points; and each of the former will lie somewhere near each of the latter in order. Thus $A-B+C$ cannot possibly have a real pair of roots unless $AB<BC$ by a considerable interval; and $A-B+C-D$ can have only one root unless $AB<CD$. It may be thought that if $CD<AB$, the curve $A-B+C-D$ may still be able to cross the x-axis thrice between them; but then this curve might possibly cut the curve $A-B$ thrice also—that is, $C-D$ would have three roots; which is impossible. Further study of this subject must be left to the reader. He should also consider the position of AD, AF, AH . . . and other secondary critical points, which may often be the bases of iterations, and examine Example P.

I propose to call one-change, two-change, three-change . . . equations *plenary* linear, quadratic, cubic . . . equations; and the points AB, BC, AD, . . . *plenary critical points*.

These propositions are best studied under the Arithmetical Variable—consisting only of signless numbers. Much confusion is frequently caused by the promiscuous use of the One-Dimensional and the Two-Dimensional Variables. Thus Cauchy's theorem that an equation of the nth degree has n-roots has been hailed as "the fundamental theorem of algebra," but with both the Arithmetical and the One-Dimensional Variables it is simply untrue, and Descartes' Rule should take its place. If we go outside these variables, why not use the Three-Dimensional Variable at once and put everything upon a Quaternion basis?

III. *Sterile Tracts.*—The function $Q-R+S$ can have no bend-points unless its tangential $Q'-R'+S'$ has roots; and operation upon them with any number of $p+x0$ factors will not give them bend-points or roots. Hence if fx contains such a group of terms it will have at least two roots less than the possible complement. This is the simple general theorem which, when Q, R, and S are consecutive single terms, becomes Newton's Rule (p. 583). This rule was proved but not explained by Sylvester in 1864—after many mathematicians had failed.

IV. *Analysis by Partial Roots.*—The following is a useful method of localising roots by means of the partial roots of a *single* setting. Let $\zeta x - fx = \xi x$ where fx is the given equation - of an even degree. Let ζx be an arbitrary function of the same degree and with its first two or more terms the same as those of fx, and let its roots be all double roots. Then ζx is always positive; and ξx is two degrees less than fx and its roots bear definite relations to those of fx. For example, let $(x^2-4x+5)(x^2-4x+3) = x^4-8x^3+24x^2-32x+15=0$ be a test equation. Take for ζx the arbitrary *standard* equation, $x^4-8x^3+22x^2-24x+9=0$, of which the roots are 1, 1, 3, 3. Then $\xi x = -2x^2+8x-4$; and while ζx is always positive, ξx is always negative except between its two roots $2 \pm \surd 2$—so that the roots of fx lie between those of ξx; which is evidently the case. Or let $fx \equiv (x^2-4x+5)(x^2-4x+7) = x^4-8x^3+28x^2-48x+35=0$. Take the same standard equation as before. Then $\xi x = -6x^2+24x-26$; and as this is always negative, fx has no roots. If fx is of an odd degree it may be treated in a similar manner; or may be raised one degree by multiplication by an arbitrary factor $x \pm a$. In all cases the actual roots of fx can occur only when ζx and ξx have the same sign—Section VII (1).

V. *Critical Coefficients.*—Many propositions in the Theory of Equations may be given more elegantly by using these. For example, if $x^4 - px^3 + qx - r = 0$, we write $x^4 - bx^3 + bcx - bcd = 0$, where $b = p$, $c = q/p$, $d = r/q$. The same thing can be done if the original coefficients are affected by the binomial (Newtonian) coefficients.

VI. *Remarks on the Tables.*—On inspection of Tables I and II it will be seen that the series for ψ_n^{-1} and $(\psi_n)^{-1}$ consist of the same combinations of the original coefficients $b, c, d, \ldots$ grouped in the same way, but that the numerical sub-coefficients are different; in fact the series $\psi_n^{-1}\gamma$ is the same in form as the algebraic quotient of γ^{n+1} divided by $\psi_n\gamma$ (γ is used for g in the Tables as the latter symbol is required for one of the coefficients). Now if in place of $b, c, d, \ldots$ we use weighted coefficients $p_1, p_2, p_3 \ldots$, we shall easily see that the general *minor* terms of $\psi_n^{-1}\gamma$ is (if $a, b, c \ldots$ are any integers)

$$-\frac{1}{w-1}\binom{w-1}{n}_e \cdot \frac{(-1)^e \cdot e!}{a!\,b!\,c!\ldots} \cdot p_q^a p_r^b p_s^c \ldots / \gamma^{w-1},$$

where $e = a+b+c\ldots$, and $w = aq+br+cs\ldots$ and is the combined weight of the whole group $p_q^a p_r^b p_s^c \ldots$ Hence we can always find the numerical sub-coefficient to be attached to every one of these groups, and also the power of γ to which the group belongs. Conversely if the power of γ be given, we can find the groups of original coefficients which will belong to it in the series and their numerical sub-coefficients - by the same process as the one employed for finding co-efficients in the multinomial theorem. Indeed, the sub-coefficient $(-1)^e \cdot e!/a!\,b!\,c!$ is simply the sub-coefficient of the same group in the ordinary expansion of $(\psi_n)^{-1}$; while the sub-coefficient $-\{(w-1)/n\}_e/(w-1)$ is the special one given by operative division. On further inspection of the series we see that the *middle* coefficients of γ (in small brackets) consist of groups which are all of the same *order*, e; and that the *major* coefficients of γ (in large brackets) consist of groups which are all of the same weight w. The sum of the numerical sub-coefficients of all the terms within small brackets is the binomial coefficient $(w-1)_{e-1}$; and when the products of both the numerical sub-coefficients are fractions, the denominators of these fractions consist only of powers of n.

In Section VIII (6), p. 590, it was shown that the series are ultimately convergent when the corresponding iteration is so—which usually can be easily

ascertained. For proximate convergence the terms should diminish quickly—which will generally occur when y is large or when its major coefficients become small. To study the latter case put $b = n_1$, $c = n_2$, $d = n_3$, . . . ; then all the major coefficients vanish if n is the degree of the equation, since $x = y - 1$. Or we may set the original equation and its invert in what may be called Newtonian coefficients (Table III).

The calculation of roots is much facilitated by centring the original equation by removing the second term bx^{n-1}—which abolishes all the terms containing b in the invert (see Table IV and the latter part of Table II, 1). By Part II, Example K, p. 401, linear transformations do not affect the value of the major coefficients of y, which are invariants.

If $\psi_n x$ has no negative powers of x, so that n is the degree of the function, the major coefficients of its invert based upon its absolute term are all functions of the invariants H, G, I, J, etc., as can easily be seen from Table III, for example. These functions may be most readily obtained by inverting the centred form of $\psi_n x$—as, for instance, by putting n_2C, n_3D, . . . for c, d, . . . in Table IV. The study must be left to the reader, together with that of the serial inversion of quantics in general.

It will be seen from the formula just given for the general minor term that the latter part of it may be written $y(p_q/y^q)^a . (p_r/y^r)^b . (p_s/y^s)^c$. . . . Hence the series divided by y consists of all possible combinations of these ratios. Or we may also distribute n amongst them in the same manner, and then rearrange the series in terms of the type p_q/ny^q collected according to their order. This is done in Table V. The terms may easily be written out to any extent. The major terms now consist of the quantities B, C, D, . . . with the same powers and coefficients as they possess in $(A + B + C \ldots)^e$ and with the additional coefficients $(n - w + 1)$ $(2n - w + 1)$ . . . containing $e - 1$ factors. If A, B, C, . . . are all numerically < 1, the terms will generally diminish rapidly.

If we rearrange this series in descending terms of n we shall find that the coefficients of the successive powers of n consist of infinite series of the ratios b/y, c/y^2, . . . which can easily be summed, giving the important logarithmic form

$$y^{-1}x = y^{-1}\psi_n^{-1}y^n = 1 - 1/n . \log G + 1/(2!n^2) . \frac{d}{dy}(y \log^2 G) - 1/(3!n^3 . \left[\frac{d}{dy}(y0)\right]^2 \log^3 G + \text{etc.},$$

where $G = 1 + b/y + c/y^2 + \ldots = \psi_n y/y^n$. The symbolical[1] sum of this is

$$x = \psi_n^{-1}y^n = \int \left\{ \left[\epsilon^{-\frac{1}{n}\left[\frac{d}{dy}(y0)\right]0} \right] \log G \right\} dy.$$

The differentiations here indicated give finite expressions in powers of $\log G$, which may be arranged in ascending order, or re-arranged in powers of $exp(\log G/n)$, and in other ways. All these arrangements are generated by different iterands, such as those suggested on p. 579; but I have no space to deal with them here.

EXAMPLES

I have space only for a few examples to illustrate some of the propositions in this article. Note VI and the Tables will facilitate calculations, especially with the aid of Barlow's Tables. The equations are classified according to Note II.

O. *One-change Equations*, $A - B = 0$.

(1) $x^2 - x - 1 = 0$.

By Section VII (3), p. 581, both the positive critical points $= 1$; and both refer to the only positive root and are prospective, *i.e.* less than it. Which should we

[1] Correct symbolism, however, requires a special Iteration-Operator.

select for iteration and inversion? Evidently (by trial or otherwise) $X<2$ and $f'X<f'2=3$. By Section VIII (6), p. 590, both the iteration and the invert will ultimately converge if $2qg>f'X$, where $q=1$ or 2 and $g=\gamma$. It is simpler to use no modifier if possible and to take $g=$ each critical point in turn. If $q=1$, $2qg$ may not be $>f'X$; but the condition will hold if $q=2$. We therefore iterate $0-f/2$ on the base $g=1$; or calculate X from the corresponding invert of $x^2-x=1$, which is, by Table II,

$$X=1+1/2+1/8-1/128+1/1024\ldots=1{\cdot}61816.\ldots$$

The iteration gives the same result, and by the usual solution $X=1{\cdot}618034.\ldots$ The root being ascertained, we have $f'X=2{\cdot}236\ldots$; so that if we had taken $q=1$ and inverted $x-x^{-1}=1$, we should have obtained an ultimately divergent series.

The reciprocal equation ($x=1/z$) is $z^2+z-1+0$. This has only one positive critical point ($=1$) which is $>Z$, while $Z>\frac{1}{2}$. Now $f'Z$ must be $<f'(1)=3$. We must take $q=2$; so that $2qg>f'Z$. Hence the iteration of $0-f/2$ and the invert of $z^2+z=1$ will be valid, giving $Z=0{\cdot}61816\ldots$, which will be the reciprocal of X.

Observe that if the other sign of $g=\pm\sqrt{1}$ be taken, the *negative* root of the original equation is obtained by a valid series.

(2) $x^3-x^2-x-1=0$ ($q=3, g=1, 2qg^2>f'X, X=1{\cdot}853\ldots$).
(3) $x^3-10x^2-x-1=0$ ($q=1, g=10, 2qg^2>f'(11), X=10{\cdot}1087\ldots$).
(4) $x^3-x^2-100x-1=0$ ($q=2, g=\sqrt{100}, 2qg^2>f'(11), X=10{\cdot}51719\ldots$).
(5) $x^3-x^2-x-1000=0$ ($q=3, g=\sqrt[3]{1000}, 2qg^2>f'(11), X=10{\cdot}37912\ldots$).
(6) $x^3-2x-5=0$ ($q=3, g=\sqrt[3]{5}, 2qg^2>f'X, X=2{\cdot}0945\ldots$).
(7) $x^3-x^2-81-90=0$ ($q=2, g=\sqrt{81}, 2qg^2>f'X, X=10$).
(8) $x^3-2x^2-2x+5=8$ ($q=3, g=\sqrt[3]{8}, 2qg^2>f'X, X=3$).

The reader should also examine the corresponding reciprocal equations ($x=1/z$). Equation (6) has been also dealt with on Part I, p. 232, and Part II, p. 394. In order to avoid the irrational subject $\sqrt[3]{5}$ we may add 3 to both sides of the equation and invert $x^3-2x+3=8$, as suggested in Part II, p. 401; and here also $2qg^2=24>f'X$. (The rule for finding the modifier there given applies to midaxial iteration.) Examples (7) and (8) are from p. 581. In the latter the critical points 2, $\sqrt{2}$, $\sqrt[3]{3}$ will not give convergent iterations and series, and we are therefore *obliged* in this case to use a modifier. The one suggested is 5, but 26 may be tried.

(9) $x^5-x^4-x^3-x^2-x-1=0$.

Compare Examples (1) and (2) above. All the critical points are unity and prospective; and $X<2$, and $f'X<f'(2)=31$. Thus none of the critical points will of themselves suffice for g, since we must have $2qg^4>31$, where $q=1, 2, 3, 4$, or 5. We must therefore use a modifier calculated from $2q(1+m)^4>f'X$—say $m=1$ with $q=2$. But, though all these series will be ultimately convergent, yet owing to the large number of terms in f, the first few terms of the inverts in Table II will not nearly reach the root, unless we take the invert in invariants (Note VI, p. 596). The reciprocal equation is not better; but arithmetical iteration, say of $0-f/30$ upon $g=2$, easily gives $X=1{\cdot}965948\ldots$. If we still require a series for the remainder of the root we can shift the origin by $1{\cdot}96$, say, and obtain a rapidly approximating simple ascending invert (Part I, Section III).

(10) $x^3+3x^2-2x-5=0$ (Part II, p. 395).

By p. 582 the two leading-term critical points $\sqrt{2}$ and $\sqrt[3]{5}$ are both $>X$, and $f'X$ is therefore $<f'(\sqrt{2})=12{\cdot}485$. Hence the former critical point is not suitable; and the latter one gives a very slowly approximating series ($X=1{\cdot}330058\ldots$). It it better therefore to centre the equation and proceed as in Example P (11).

P. *Two-change Equations*, $A-B+C=0$.

Here there are two possible positive roots, but they are often unreal. In Note II such equations are called Plenary Quadratics, and it is shown that if the positive root (plenary critical point) of $A-B=0$ is not considerably greater than the root of $B-C=0$, the positive roots of fx cannot be real. For example, in $3x^6+10x^5+19x^4-30x^3-105x^2+645x+119=0$, the root of $3x^6+10x^5+19x^4-30x^3-105x^2$ is less than the root of $19x^4-30x^3-105x^2=0$, which is $3{\cdot}23\ldots$; and the root of $30x^3+105x^2-645x-119=0$ is greater than the root of $30x^3+105x^2-645x$, which is $3{\cdot}25$; and as $3{\cdot}25>3{\cdot}23$, the whole equation can have no positive roots. In fact the roots of fx lie between the roots of the inner partial function $19x^4\ldots645x$, and must be unreal if these are unreal. A descending iteration based upon the superior plenary critical point must reach the superior root; and an ascending one based upon the inferior critical point, the inferior root. The latter will also be the reciprocal of the superior root of the $x=z^{-1}$ equation, given by descending iteration. If the roots are not real, both iterations will pass through the gap between the curve and the axis of x—rapidly if Newton's iterand be employed; so that the reality of the roots is easily ascertained. For the series it is generally sufficient to select the minor critical point which is nearest to the appropriate plenary critical point. Part II, Example C, p. 395, is a case, and the reader may show that the critical points there selected yield convergent series, though both series are slow.

(11) $x^3-5x+1=0$ (Part II, p. 395).

Ex. O (10) centred. The point $\pm\sqrt{5}$ gives the greater positive root $2{\cdot}1284\ldots$ and the negative root, $-2{\cdot}3300\ldots$; and the reciprocal equation gives the middle root $x=0{\cdot}2016\ldots$, all the series being valid.

Q. *Three-change Equations*, $A-B+C-D=0$.

Dealt with in the same way. If all three possible positive roots are real, the root of $A-B=0$ refers to the greatest, and that of $A-D$ either to the greatest or the least. Similarly with the reciprocal equation; and the minor critical points refer to the same—there being no leading-term positive critical points which refer to the middle root, and therefore, apparently, no series for it (without transformations).

(12) $x^3-16x^2+65x-50=0$ (p. 582).

The series $q=1$, $g=16$ converges slowly to the greatest root, 10. The series $q=3$, $g=\sqrt[3]{50}$ converges to the least root, 1, though it may not at first appear to do so, and the same root is given by simple ascending division. Put $x=50z^{-1}$, divide by 50, and compare. For the middle root, 5, centre the equation after putting $x=y/3$ and multiplying by 27.

R. *Four-change Equations*, $A-B+C-D+E=0$.

AB refers to the greatest root, AD generally to the third, ED to the least and EB to the second. By multiplying a plenary cubic by a factor $x-a$ we can raise it to a plenary biquadric and thus obtain a series for its middle root.

TABLES

Table I. Algebraic Multinomial Theorem.

$$[0^m]f_n = (0^n + b0^{n+1} + c0^{n+2} + \dots)^m = 0^{mn} + mb0^{mn+1} + \{mc + m_2b^2\}0^{mn+2} +$$
$$+ \{md + m_2 2bc + m_3b^3\}0^{mn+3} + \{me + m_2(c^2 + 2bd) + m_3 3b^2c + m_4b^4\}0^{mn+4} +$$
$$+ \{mf + m_2 2(cd + be) + m_3 3(bc^2 + b^2d) + m_4 4b^3c + m_5b^5\}0^{mn+5} + \text{etc.}$$

Table II. Descending Inverts.

1. If $\psi_n x = x^n + bx^{n-1} + cx^{n-2} + \dots = y = \gamma^n$, then—

$$x = \psi_n^{-1}y = \gamma - b/n - \{(1/n)c + (1/n)_2b^2\}1\gamma - \{(2/n)d + (2/n)_2 2bc + (2/n)_3b^3\}/2\gamma^2 -$$
$$- \{(3/n)e + (3/n)_2(c^2 + 2bd) + (3/n)_3 3b^2c + (3/n)_4b^4\}/3\gamma^3 - \{(4/n)f + (4/n)_2 2(cd + be) +$$
$$+ (4/n)_3 3(bc^2 + b^2d) + (4/n)_4 4b^3c + (4/n)_5b^5\}/4\gamma^4 - \{(5/n)g + (5/n)_2(d^2 + 2ce + 2bf) +$$
$$+ (5/n)_3(c^3 + 6bcd + 3b^2e) + (5/n)_4 2(3b^2c^2 + 2b^3d) + (5/n)_5 5b^4c + (5/n)_6b^6\}/5\gamma^5 -$$
$$- \{(6/n)h + (6/n)_2 2(de + cf) + (6/n)_3 3c^2d + \dots (b = o)\}/6\gamma^6 - \{(7/n)i +$$
$$+ (7/n)_2(e_2 + 2df + 2ch) + (7/n)_3 3(cd^2 + c^2e) + (7/n)_4c^4 \dots\}/7\gamma^7 - \{(8/n)j +$$
$$+ (8/n)_2 2(ef + dg + ch) + (8/n)_3(d^3 + 6cde + 3c^2f) + (8/n)_4 4c^3d \dots\}/8\gamma^8 - \{(9/n)k +$$
$$+ (9/n)_2(f^2 + 2eg + 2dh + 2ci) + (9/n)_3 3(d^2e + ce^2 + 2cdf + c^2g) + (9/n)_4 3c^2d^2 +$$
$$+ (9/n)_5c^5 \dots\}/9\gamma^9 - \{(10/n)l + (10/n)_2 2(fg + eh + di + cj) + (10/n)_3 3(de^2 + d^2f +$$
$$+ 2cef + 2cdg + c^2h) + (10/n)_4 4(cd^3 + 3c^2de) + (10/n)_5 5c^4d \dots\}/10\gamma^{10} - \text{etc.}$$

2. If $\psi_1 x = x + b + cx^{-1} + dx^{-2} + ex^{-3} + \dots = y = \gamma$, then—

$$x = \psi_1^{-1}y = \gamma - b - c/\gamma - \{d + bc\}/\gamma^2 - \{e + (c^2 + 2bd) + b^2c\}/\gamma^3 - \{f + 3(cd + be) +$$
$$+ 3(bc^2 + b^2d) + b^3c\}/\gamma^4 - \{g + 2(d^2 + 2ce + 2bf) + 2(c^3 + 6bcd + 3b^2e) +$$
$$+ 2(3b^2c^2 + 2b^3d) + b^4c\}\gamma^5 - \{h + 5(de + cf + bg) + 10(c^2d + bd^2 + 2bce + b^2f) +$$
$$+ 10(bc^3 + 3b^2cd + b^3e) + 5(2b^3c^2 + b^4d + b^5c\}/\gamma^6 - \text{etc.}$$

3. If $\psi_2 x = x^2 + bx + c + dx^{-1} + ex^{-2} + \dots = y = \gamma^2$, then—

$$x = \psi_2^{-1}y = \gamma - b/2 - \{2^2c - b^2\}/2^3\gamma - d/2\gamma^2 - \{2^6e + 2^4(c^2 + 2bd) - 2^3b^2c + b^4\}/2^7\gamma^3 -$$
$$- \{2^9g + 2^7 . 3(d^2 + 2ce + 2bf) + 2^6(c^3 + 6bcd + 3b^2e) - 2^4(3b^2c^2 + 2b^3d) +$$
$$+ 12b^4c - b^6\}/2^{10}\gamma^5 - \{h + 2(de + cf + bg) + (c^2d + bd^2 + 2bce + b^2f)\}/2\gamma^6 - \text{etc.}$$

4. If $\psi_3 x = x^3 + bx^2 + cx + d + ex^{-1} + \dots = y = \gamma^3$, then—

$$x = \psi_3^{-1}y = \gamma - b/3 - \{3c - b^2\}/3^2\gamma - \{3^3d - 3^2bc + 2b^3\}/3^4\gamma^2 - e/3\gamma^3 - \{3^5f + 3^4(cd + be) -$$
$$- 3^3(bc^2 + b^2c) + 15b^3c - 2b^5\}/3^6\gamma^4 - \{3^7g + 3^6(d^2 + 2ce + 2bf) - 3^4(c^3 + 6bcd + 3b^2e) +$$
$$+ 3^3(6b^2c^2 + 4b^3d) - 3^2 . 7b^4c + 7b^6\}/3^8\gamma^5 - \{h + (de + cf + bg)\}/2\gamma^6 - \text{etc.}$$

5. If $\psi_4 x = x^4 + bx^3 + \dots = y = \gamma^4$ and $\psi_5 x = x^5 + bx^4 + \dots = y = \gamma^5$, then—

$$x = \gamma - b/4 - \{4^2c - 6b^2\}/4^3\gamma - \{4^2d - 4 . 2bc + 2b^3\}/4^3\gamma^2 - \{4^5e - 4^3 . 2(c^2 + 2bd) +$$
$$+ 4^2 . 10b^2c + 30b^4\}/4^6\gamma^3 - f/4\gamma^4 - \{4^7g + 4^5 . 2(d^2 + 2ce + 2bf) - 4^4 . 2(c^3 + 6bcd + 3b^2e) +$$
$$+ 4^3 . 7(3b^2c^2 + 2b^3d) + 4 . 7 . 22b^4c + 77b^6\}/4^8\gamma^5 - \text{etc.}$$

$$x = \gamma - b/5 - \{5c - 2b^2\}/5^2\gamma - \{5^2d - 15bc + 4b^3\}/5^3\gamma^2 - \{5^3e - 5^2(c^2 + 2bd) + 5 . 7b^2c$$
$$- 7b^4\}/5^4\gamma^3 - \{5^5f - 5^4(cd + be) + 5^3 . 3(bc^2 + b^2d) - 55b^3c + 44b^5\}/5^6\gamma^4 - g/5\gamma^5 - \text{etc.}$$

Table III. Newtonian Coefficients.

If $x^3 + 3bx^2 + 3cx + d = y = \gamma^3$, then –

$$x = \gamma - b + \{b^2 - c\}/\gamma - \{2b^3 - 3bc + d\}/3\gamma^2 + \{2b^4 - 5b^2c + bd + 3bc^2 - cd\}/3\gamma^4 -$$
$$- \{7b^6 - 21b^4c + 18b^2c^2 + 4b^3d - 3c^3 - 6bcd + d^2\}/3^2\gamma^5 - \text{etc.}$$

Table IV. Centred Equations

1. If $x^3 + cx + d = y = \gamma^3$, then—

$$x = \gamma - c/3\gamma - d/3\gamma - cd/3^2\gamma^4 - \{3^2d - c^3\}/3^4\gamma^5 - \{3^2.2cd^2 - c^4\}/3^5\gamma^7 - 5\{d^3 - c^3d\}3^4\gamma^8 -$$
$$- 7\{3^2.2cd^3 - c^4d\}/3^6\gamma^{10} - 2\{3^2.5d^4 - 30c^3d^2 + 4c^6\}/3^7\gamma^{11} - \text{etc}$$

2. If $x^4 + cx^2 + dx + e = y = \gamma^4$, then—

$$x = \gamma - c/4\gamma - d/4\gamma^2 - \{4^2e - 2c^2\}/4^3\gamma^3 - 2\{4(d^2 + 2ce) - c^3\}/4^4\gamma^5 - 2\{4de - c^2d\}/4^3\gamma^6 -$$
$$- 2\{4^3.3e^2 - 4^2.3(cd^2 + c^2e) + 5c^4\}/4^6\gamma^7 - 2\{4^3.5(d^2e + ce^2) - 60c^2d^2 + 7c^5\}/4^7\gamma^9 -$$
$$- \{4^2.6de^2 - 4^2(cd^3 + 3c^2de) + 6c^4d\}/4^5\gamma^{10} - 7\{4^4.2e^3 - 4^3.2(3c^2e^2 + d^4) +$$
$$+ 40(2c^3d^2 + c^4e) - 18c^6\}/4^8\gamma^4 - \text{etc.}$$

3. If $x^5 + cx^3 + dx^2 + ex + f = y = \gamma^5$, then—

$$x = \gamma - c/5\gamma - d/5\gamma^2 - \{5e - c^2\}/5^2\gamma^3 - \{5f - cd\}/5^2\gamma^4 - \{5(de + cf) - 2c^2d\}/5^3\gamma^6 -$$
$$- \{5^2(e^2 + 2df) - 15(d^2c + c^2e) + 2c^4\}/5^4\gamma^7 - \{5^2.3ef - 5(d^3 + 6cde + 3c^2f) +$$
$$+ 7c^3d\}/5^4\gamma^8 - \{5^4.2f^2 - 5^3.2(d^2e + ce^2 + 2cdf) + 15c^2d^2 - 11c^5\}/5^6\gamma^9 - \text{etc.}$$

Table V.

If $\psi_n x = \gamma^n$ and $B = b/n\gamma$, $C = c/n\gamma^2$, $D = d/ng^3, \ldots$, then—

$$\gamma^{-1}x = \gamma^{-1}\psi_n^{-1}\gamma^n = 1 - \{B + C + D\ldots\} + \{(n-1)B^2 + (n-3)C^2 + (n-5)D^2 \ldots +$$
$$+ (n-2)2BC + (n-4)CD \ldots\}/2! - \{(n-2)(2n-2)B^3 + (n-5)(2n-5)C^3 \ldots +$$
$$+ (n-3)(2n-3)3B^2C \ldots + (n-7)(2n-7)3CD^2 \ldots + (n-5)(2n-5)$$
$$6ABC\ldots\}/3! + \{(n-3)(2n-3)(3n-3)B^4 \ldots + (n-4)(2n-4)(3n-4)$$
$$4B^3C\ldots\}/4! - \text{etc}$$

Table VI.

If $fx = \psi_n x - \gamma^n = 0$; and $H = f\gamma/n\gamma^n$, and $K = \log(\psi\gamma/\gamma^n)$, then—

1. $x = \gamma - \gamma H + \gamma[D_\gamma(\gamma 0) + n0]H^2/2! - \gamma[D_\gamma(\gamma 0) + n0][D_\gamma(\gamma 0) + 2n0]H^3/3! + \text{etc.}$
$= \gamma - \gamma H + \gamma\{2\gamma f'\gamma/f\gamma + (1-n)\}H^2/2! - \gamma\{3\gamma^2 f''\gamma/f\gamma + 6\gamma^2(f'\gamma/f\gamma)^2 +$
$+ 9(1-n)\gamma f'\gamma/f\gamma + (1-n)(1-2n)\}H^3/3! + \text{etc.}$

2. $x = \gamma - \psi\gamma/\psi'\gamma.K + \psi\gamma/\psi'\gamma.D_\gamma(\psi\gamma/\psi'\gamma)\ K^2/2! - \psi\gamma/\psi'\gamma\ [D_\gamma(\psi\gamma/\psi'\gamma)]^2\ K^3/3! + \text{etc.}$
$= \gamma - \psi\gamma/\psi'\gamma.K + \psi\gamma/\psi'\gamma.\{1 - \psi\gamma/\psi'\gamma\ \ \psi''\gamma/\psi'\gamma\}K^2/2! -$
$- \psi\gamma/\psi'\gamma\{1 - \psi\gamma/\psi'\gamma.\psi''\gamma/\psi'\gamma)^3 + (\psi\gamma/\psi'\gamma.\psi''\gamma/\psi'\gamma)^3 - (\psi\gamma/\psi'\gamma)^2\ \ (\psi'''\gamma/\psi'\gamma)\}$
$K^3/3! + \text{etc.}$ (if $\gamma\psi'\gamma/n\psi\gamma > 0$ and < 2).

FIGURES

Common Point of ζ and ξ

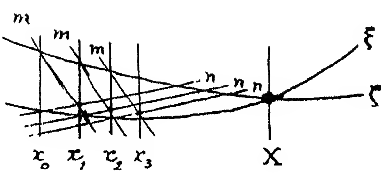


FIG. 1.—Section VI (2).

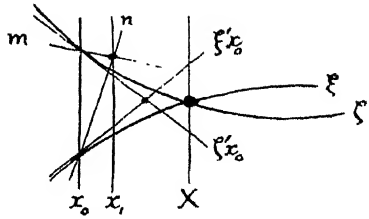


FIG. 2.—Section VI (2).

Axial Iteration

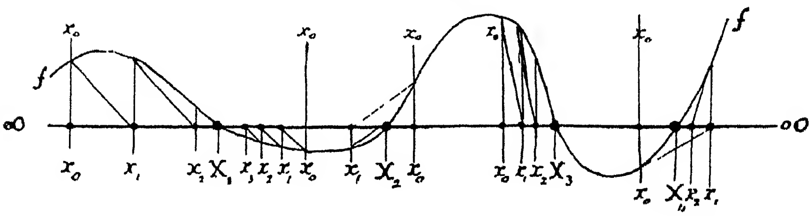


FIG. 3. $[\mathbf{O}+f]_{x_0}$. Section VI (3).

FIG. 4. $-[\mathbf{O}+f/m]_{x_0}$. Section VI (3).

FIG. 5. $[\mathbf{O}-f/f']_{x_0}$. Section VI (4).

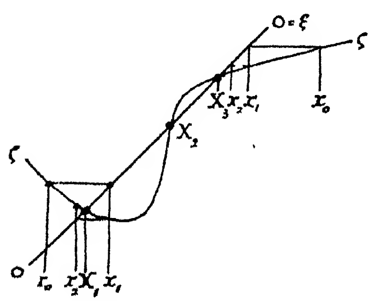


FIG. 6. $[\mathbf{O}+(\xi-\mathbf{O})]_{x_0}$. *Mid-axial Iteration.* Section VI (5).

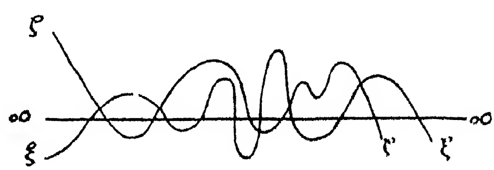


FIG. 7.—*Partial Roots and Actual Roots.* Section VII (1).

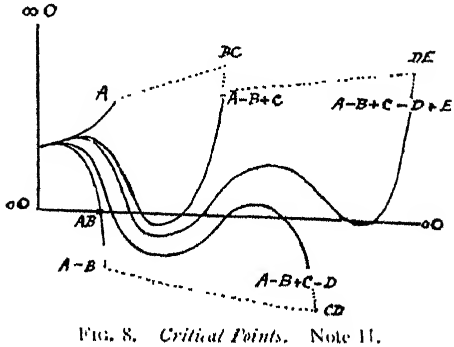


FIG. 8. *Critical Points.* Note II.

ESSAY-REVIEWS

COMBINATORY ANALYSIS—THE INTERPRETATION THEREOF, by C.: on **Combinatory Analysis**, by MAJOR PERCY A. MACMAHON, F.R.S., D.Sc., LL.D. Volume I. [Pp. xix + 300.] (Cambridge: at the University Press, 1915. Price 15*s*. net.)

COMBINATORY Analysis is well known even to students of elementary algebra, where it figures under the title of Permutations and Combinations; while in theory of equation the subject is intimately connected with symmetric functions. The formal statement of the objects of this branch of mathematics is that it includes the formation, enumeration, and other properties of the different groups of a finite number of elements which are arranged according to prescribed laws. By its subject-matter combinatory analysis is related to some of the most ancient problems which have exercised human ingenuity. It would be difficult to assign a date to such a question as the Ferry-boat problem; its historian describes it under the extensive title of mediæval, but it would surprise no one to find it enclosed in a still-unwrapped papyrus roll, or to hear that a version of it had been discovered in some ancient manuscript which described the pastimes of King Solomon's ample families. The scientific subject commences with the works of Pascal, Leibniz, Wallis, and John Bernoulli. In the hands of these mathematicians the problems do not always retain the charms of their earlier romantic setting; jealous husbands and frail wives no longer meet at ferries, and letters are no longer placed at random in their envelopes. The problems are enunciated in set and formal terms; now the chief aim is to determine the number of distributions of n objects of which p are of one kind, q of another kind, r of a third and so on $(n = p + q + r + \ldots)$ into parcels, that is unarranged classes, of which p_1 are alike and of one kind, q_1 of a second kind, r_1 of a third kind, and so on $(n_1 = p_1 + q_1 + r_1 + \ldots)$, while a similar question arises in connection with groups in which the objects are arranged. In the first sections of the book

before us these problems are considered and in certain cases solved; the theory of symmetric functions, which is intimately bound up with the theory of distribution, is developed. The section also gives an account of the laws of symmetry and of Hammond's operators which are to play an important part in the development of the subject.

Major MacMahon in his preface notes the position of the subject of Partitions in the international organisation of the subject-matter of mathematics; in this division partitions are placed in Theory of Numbers: our author pleads for it as a subdivision of Combinatory Analysis. His plea is supported by the treatment of the theory of the separations of a partition which is given in Section II, and its intimate relation to the expression of symmetric functions as well as to the part which this theory plays in other portions of the subject developed in this book. In this section the theory of distribution and the theory of symmetric function are both advanced by the notion of the unitary partitions of a number; as the author says, at one time the theory of distributions is urged on by the algebra and at another pulls the algebra after it. In Section III permutations in their most general form are discussed. This section contains a theorem which is called by the author the Master Theorem on account of the success with which he has applied it to the solution of many problems of great variety and difficulty. It is of great algebraical interest and is quoted here because it gives, as well as an extract can, some notion of the problems solved in the book. The theorem is: "Given

$$X_r = a_{r1}x_r + a_{r2}x_2 + \dots + a_{rn}e_n (r = 1, \dots n)$$

then the coefficient of

$$x_1^{\xi_1} x_2^{\xi_2} \dots x_n^{\xi_n}.$$

in the development of the product

$$X_1^{\xi_1} X_2^{\xi_2} \dots X_n^{\xi_n}$$

is equal to the coefficient of the term

$$x_1^{\xi_1} x_2^{\xi_2} \dots x_n^{\xi_n}$$

in the expansion of the function

$$\frac{1}{(1 - a_{11}x_1)(1 - a_{22}x_2) \dots (1 - a_{nn}x_n)}$$

in which the denominator is in symbolic form in such wise that on multiplication the factors $a_{11}a_{22}$, $a_{11}a_{22}a_{33}$, . . . are to be placed in determinant brackets $(a_{11}a_{22})$, $(a_{11}a_{22}a_{33})$ . . . and denote coaxial minors of the determinant of transformation." Now by selecting different types of the guiding determinant various interesting algebraic forms of the generating function are written down; the application of these enables the author to solve problems of great interest: amongst these we may cite the Problème des Rencontres, the number of circular substitution of n letters and certain cases of lattice permutations. A by-product of the theory expounded occurs in the interesting case of dealing out the cards of m packs of playing cards; it is proved that the average number of pairs met with in dealing is equal to $4m-1$. The Master Theorem is a remarkable instance of the insight of the author into the inner nature of those questions which are so well known as recreations and which have been effectively displayed by Mr. Rouse Ball and M. Edouard Lucas. The particular problems do not interest our author: he goes for the general algebraical theorem upon which they depend, and when that has been solved the particular problem falls into its proper relation. It is the same philosophical spirit at work in the sphere of number as that which Newton showed in his great dynamical discoveries; when one studies Newton one is apt to forget the magnitude and diversity of the problem which suggested his immortal speculations—so it is with the Combinatory Analysis, the infinite perplexities of the problems are reduced by Major MacMahon to order and simplicity, not by solving particular problems, but by revealing the laws which these particular cases, so puzzling in themselves and so apparently unrelated to each other, must obey.

In Section IV the theory of the compositions of numbers is treated. The simplest case is the unipartite composition of n, and by composition the author means partitions in which order is regarded; thus 4 has eight compositions—

$$(4)\ (31)\ (13)\ (2^2)\ (1^4)\ (1^2 2)\ (21^2)\ (121)$$

Various graphical representations of unipartite numbers are explained and then the general problem of multipartite numbers is entered upon, that is, the number of distributions of objects of the type $(pqr\ldots)$ into n parcels of the type

(1^n). The general problem receives extension and elucidation beyond the discussion already given. The composition of bipartite numbers is dealt with by a graphical representation. Two chapters are devoted to the following problem, which was suggested by the American astronomer Newcomb and which arose in connection with a game of "patience": a pack of cards containing p cards marked 1, q cards marked 2, r marked 3, . . . is dealt after shuffling, and so long as the cards dealt have an ascending order or are of equal value they are dealt on one heap, but when this sequence is broken, a new heap is commenced, and the process continued until all the pack is dealt; the question is, to find the probability that there will be exactly m heaps. The solution leads to new analysis and to a remarkable chain of theorems for which the curious reader must consult the book.

Problems which are connected with distributions upon a chessboard occupy Section V, a chapter on perfect partitions being prefixed. In the application of the differential calculus to the enumeration of the number of arrangements of n 1's upon a chessboard of n^2 squares, in such wise that no row or column contains more than one symbol, we have an example of the method in which in this book a complex process is illustrated by a simple instance, and also an illustration of the employment of the differential calculus in problems which deal with magnitudes so distinct from its proper subject-matter. The author writes the operation Dx^n in the form

$$1xxx \ldots x + x1xx \ldots x + \ldots + xxx \ldots x1,$$

Thus the various terms can be represented upon n lattices of n squares arranged in line, the unity occupying its proper place in the series and the squares corresponding to the x's being left blank. Each term is next taken and differentiated thus:

$$D1xx \ldots x = 11xx \ldots x + 1x1x . x + \ldots 1xx \ldots x1$$

Each term here can be represented on a lattice or rectangular chessboard having two rows of n squares, thus one unity is placed in the position it occupies in the one-row lattice and the second is placed in its proper position in the second row. Treating each term of Dx^n in this way we require $n(n-1)$ lattices of two rows to give the various arrangements of two unities one in each row, and in no case being on the same

column. Continuing the process we finally get $n!$ square lattices of n rows and n columns each containing a different arrangement of n unities, but in each arrangement one 1 occurs in each row and one in each column.

This simple example depends upon the selection of a function and an operator. A variety of cases of greater complexity is given and the relation of the whole to the differential operators of Section II with symmetric functions on operands is discussed. This leads to the discussion of the Latin Square. The author reminds us of the cognate problem (which Euler propounded in 1782) of the arrangement of 36 officers arranged in six sets of six, six being in each of six regiments and also arranged in sets of six according to rank; the disposition is required into a square in which no row or column should contain two officers of the same rank and of the same regiment. Though Euler was unable to solve this problem, it suggested to him the problem of the Latin Square, that is, the arrangement of n letters on a board of n^2 compartments so that no letter occurs twice in the same row or column. Euler recognised the difficulty of the problem in its general form, and in 1890 Cayley, after enumerating the number of possible second lines of the square when the first is given, remarked that it was not easy to see how the whole number of arrangements for the third line is to be calculated, (adding) and the difficulty of course increases for the next following lines. Now it is remarkable that Major MacMahon has solved the general case and shown how to calculate the number of sth lines that can be written down when $(s-1)$ lines are known; he has also solved similar problems of a more difficult character. The last section of the book is devoted to the enumeration of the partitions of a multipartite number.

It is impossible to give anything but a most inadequate idea in a review of the complicated and powerful analysis by which the author attains his objects. The elaboration of their methods has occupied thirty years of work; most of the results in the book are original, some have appeared in papers published by the author in the *Phil. Trans. R.S.*, the *Proceedings of the L.M.S.*, the *Quarterly Journal*, but most of them appear here for the first time. Any one who wishes to measure the magnitude of the task achieved in the book is recommended to take the article on this subject published in the *Encyclopédie*

des Sciences Mathématiques and reflect upon the changes which are now required to bring that portion of this great work into line with the subject of which it treats.

There is one regret that may perhaps be expressed, I hope, without ungraciousness; it is that the work has been done like most of our best English mathematical work in the author's study and not in the centre of a living school of mathematics. It is, I believe, a loss to England and to mathematics that Major MacMahon has not directed a great school of research; the gain to the youthful mathematicians of such a leader is obvious; they would have received an impetus which the printed page will only give to a few. Is it not possible also that the quality of work done in such circumstances may not, like mercy, be doubly blest? When one reflects on the past and realises how much Cayley might have given to those hungry students who watched him lecture in the New Museum at Cambridge in the 'eighties, and would willingly have fed on something more satisfying than a distant view of his blue foolscap manuscript, and when one reads MacMahon and thinks that he has never even fed students on the Cayley plan, it is impossible to resist the feeling that there are countries in which mathematical teaching is better organised than it is in England.

In conclusion a reference may be made to the fact that this is Volume I; it need not be said that Volume II is eagerly anticipated. In it no doubt a complete index to the two volumes will appear. The book is splendidly printed and is singularly free from errors; there is, however, one in the statement of the Master Theorem on p. 97, where two notations have been used.

MR. BALFOUR'S ARGUMENT FOR THEISM, by JOSHUA C. GREGORY, B.Sc., F.I.C.: on **Theism and Humanism**, by the Rt. Hon. ARTHUR JAMES BALFOUR, F.R.S., LL.D., Hon. Fellow Trinity College, Cambridge. [Pp. 274.] (London: Hodder & Stoughton, 1915. Price 10*s*. 6*d*. net.)

PHENOMENA, or events, present themselves, among their many possible aspects, as RESULTS. Some results are obviously intended. A codling seizes the bait on the hook and is lodged in the creel. There was design in the proceeding, for the fisher

intended to catch fish. When a result is intended or purposed it is also an END, and we conceive of the intended result as a DESIGN. Animals secure designed results as well as men; but the larger number of results in the universe proceed neither from the intentions of human beings nor from those of animals. The universe itself, with all its processes, is one vast result. The famous Argument from Design tries to show that many of these results are also intentions, and therefore point to a creative or sustaining mind.

The story of the increasing discredit of the Teleological Argument and our own reflection soon show that when we move outside the circle of human and animal endeavour the attribution of intention or design to any result whatever becomes highly doubtful and inconclusive. It is easy to perceive that human contrivances are designs, because we know from our own experience, and thence by an analogical extension to our fellows, that men have purposes and execute them. A wider extension of the analogy suggests the reasonable conclusion that many animal actions are not only results but designs. The universe into which we are born is rich in results that stubbornly refuse to disclose themselves to analysis or logic as the consequence of intentions.

It is evident that convergence on a result does not necessarily imply intention. In a recent novel an ingeniously conceived convergence of causes raises the suspicion of murder. The victim had hung a loaded and primed musket in his curio room. There was a skylight in the roof; and, at a particular time of a particular day, the sun's rays were concentrated on the priming of the musket. The owner happened to be in line with the muzzle and was shot through the head. Here was a striking convergence of causes—the mutual positions of man, musket, skylight and sun, at the only moment of the year when it could prove fatal, the loading of the piece and so on. The example is taken from fiction, but it demonstrates that convergence on a result need not imply an intention. The degree of complexity, or arresting nature of the convergence, makes no difference to the principle. We do not attribute design to the construction of an astronomical telescope only because of its convergence on the power to make stars more visible. We know that men desire to study the stars, and we recognise in the instrument the characteristic features of human inventions. A result does not

display intention simply because it is the focus of a convergence, however intricate.

Mr. Balfour admits that " from a consideration of inanimate nature alone it is difficult, perhaps impossible, to infer design." The universe is full of results—simple results, complex results, results that have contributed to man's existence and progress. But no logical link compels us to regard even these favouring results as designed. " The mere existence of natural law is not, as it seems to me, a sufficient basis for the argument ; we require also that these laws should combine to subserve an end." Theism has, therefore, endeavoured to select certain results that can be convincingly construed as ends. " They have always sought for proofs of contrivance rather among the living than among the dead." It is not, however, clear that Theism is the only alternative to the failure of mechanism to explain the organic world, as Mr. Balfour appears to think. It may be perfectly true that " no room can be found for psychical states at all," in a " strictly determined physical system," without these psychical states being necessarily the consequence of design. Mr. Balfour, indeed, seems to involve himself in a contradiction with reference to these same psychical states. " A world where all energy suffers inevitable degradation, considered by itself, appears atheistic on the face of it ; nor can even life, consciousness, or thought redeem it, if they, too, are doomed to perish when further transformations of energy become impossible . . ." he writes, and then subsequently cites the incommensurabilities of psychical states with transformations of energy and movements of matter as an indication of design. It is, at any rate, a far cry from incommensurability to design. These psychical states, including their own private intentions, may be no more than results, results compelling no demand even for " the barest creed which acknowledged that the universe, or part of it, showed marks of intelligent purpose." Theism, or design, in short, is not the only alternative to mechanism. Mr. Balfour thinks that this " barest creed " is demanded by the design manifest in certain selected results, those results, in particular, that we call psychical ; but it is so bare that he prefers to alter the incidence of the argument and " stress is laid, not upon contrivances . . . but on the character of certain results obtained." A selection is made of certain results that support Theism, not primarily because of

the design they directly exhibit, but because of the value they present. The attempt of this argument is to prove design through value. " Value (we assert) is lost if design be absent."

This line of argument seems to be even more vulnerable than the more direct proof of design. A careless Crœsus drops a sovereign and a beggar picks it up. It is a valuable find, but the beggar would scarcely argue that he was on this account intended to have the coin. His actions would show pretty plainly that he assumed the contrary. Mr. Balfour's argumentative route is to be no primrose path. Accident, as well as design, may, and frequently does, originate value. The reputed Phœnician discovery of glass, an unintentional result proving a valuable process, indicates that it may be unnecessary to look for an intention behind the values inserted into human life. Glass is a very valuable commodity; but the fact that it is so valuable is no reason for supposing that its accidental discovery by the Phœnicians really resulted from the intention of a presiding intelligence. Design is no more marked off by value than it is established by the convergence of causes on a specific result.

Mr. Balfour seems to be faintly aware of the unconvincingness of arguing from value to design when he selects from the total mass of æsthetic interests a chosen few to bear the weight of his contention. He suggests that neither those " æsthetic interests . . . roused by objects we deem relatively trivial " nor those originated " by objects which are admittedly rare and splendid " can " fit comfortably into a purely naturalistic framework," but he admits that only the latter " demand a source beyond and above the world of sense and perception." This seems very much like the argument of a beggar that the shilling he finds might have been dropped by chance, but the sovereign in the gutter was put there intentionally for his benefit. This is not a mere parody of the argument. The greater value of the sovereign does not place it in a different category from the shilling. We valuate æsthetic interests qualitatively as well as quantitatively, it is true; but it is not at all clear that we must refer our highest æsthetic interests to providential design because they possess for us the highest degree of appeal.

Nor is it at all evident that retention of value depends on whether we attribute our æsthetic interests to chance or to providential design. Even if it did we are not entitled to argue that because our æsthetic interests lose value when separated

from divine intention we must persist in refusing to admit this separation. It is quite possible that loss of æsthetic values might form part price of rational insight. Such a supposition is, however, unnecessary. Why should we cease to appreciate Homer because we discover that there was no providential design in our possession of such appreciation? Little wonder Mr. Balfour warns us "that the argument from æsthetic values is not a scientific induction or a logical inference." His method is very different indeed from reasoning. He seeks to impress us so much with the value of certain supreme æsthetic interests that we spontaneously accept the suggestion of providential design behind them. This is the rôle of the hypnotiser. We can understand how æsthetic ecstasy prompts to belief in communion with the Divine Spirit without failing to perceive that the belief receives no support from reason.

Mr. Balfour recognises that the "charm of history essentially depends" on its actuality—"upon its accuracy; or (more strictly) upon its supposed accuracy." This "supposed accuracy" is sufficient then for value to be retained. "It is also true" that we enjoy "nature at first hand, nature seen immediately, if not as she is, at least as she appears." Nature is nature, and our enjoyment and appreciation remain whether we were intended to enjoy or not.

The supplementary argument that our æsthetic values, particularly our highest æsthetic values, cannot be due to selection and must therefore belong to a design for our benefit too drastically limits the alternatives. By-products are frequently exploited when found to be valuable. Æsthetic interests may well be by-products (or after-products), that we have learned to esteem, of the evolutionary process. Just as dreams are an incidental consequence of our psychical constitution, so may it be with æsthetic interests. The argument also disposes too simply of one of its own suggested alternatives. Æsthetic interests expand and heighten the mental powers. Why summarily dismiss these undoubted effects as unoperative on the evolutionary process? Even if æsthetic interests were confined to this (relatively) indirect influence they would not be unavailable for the action of natural selection, to which Mr. Balfour, though in a somewhat extended sense, restricts the naturalistic factors of evolution. But æsthetic interests are also originally and ultimately connected with utility. The

savage warms his courage and secures vigour for battle by a war-dance. The dance passes into the higher regions of art—a utility becomes a purely æsthetic enjoyment. Utility is concerned in the genesis of æsthetic interest; this is enough to preclude the summary rejection of natural selection in favour of a special design for our æsthetic benefit.

For the purposes of his argument, Mr. Balfour describes "objects of æsthetic interest" as applying to the "most varying degrees of excellence—to the small as well as to the great. . . ." The æsthetic values depend on the "intrinsic quality of the emotions" aroused and not on ulterior purposes. Those of the highest grade compel us, according to the line of argument just indicated, to accept the Theistic alternative. He similarly extends ethics to embrace "the whole range of what used to be called 'springs of action' from the loftiest love" to the most lowly impulses, and he similarly restricts his argument for Theism based on ethical considerations to the highest moral grades. He conceives morals as concerned with ends of action, and principally with ultimate ends of action. This differentiates the ethical from the æsthetic without, however, destroying the essential parallelism between the two lines of argument.

Altruistic efforts on the lower portions of the ethical scale, such as the parental devotion among animals, have survival value and compel no appeal from selection to Theism. The higher parts of the scale find their only explanation in the latter. The argument does not gain in cogency by its transference to the ethical plane.

Among animals, we learn, there is no contrasting of "alternative ends." "In the bee-hive altruism is obeyed, but not chosen." There are many ultimate ends for man that may harmonise or clash. It seems rather curious to imply that the clashing or chaotic element should point to a divine régime. The human "ethics of reflection" are also contrasted with the "ethics of instinct" with respect to plasticity. "Instincts are (relatively) definite and stable," animal loyalties do not become nuclei of wider associations. Human loyalties show no similar fixity. Since, however, the capacity for altruistic emotions and beliefs is useful to the race it may be plausibly conjectured that it is a direct product of organic evolution, just as animal loyalties may be. But the highest altruistic ideals

cannot have been evoked by organic selection from the primitive forms of loyalty. Granting, if only for the sake of argument, that our highest ethical ideals have no survival value, Theism is not the only alternative to selection, though Mr. Balfour here, as elsewhere, reduces the alternatives to these two. The highest ethical ideals may be greatly valued; but their value in our eyes does not, any more than the value of the highest æsthetic ideals, logically include the inference that it was intentionally bestowed. There is no logical compulsion to believe that "Selection must be treated as an instrument of purpose, not simply as its mimic." The term "mimic" begs the whole question. Our valuation of a result does not make it a "mimic" of a purpose. Why, again, should our "noblest ideals lose all power of appeal" because we were not actually intended to have them? The simple fact that noble ideals do appeal to us has no obvious connection with a divine design that this appeal should exist.

After æsthetic and moral values, intellectual values. The discussion of these culminates in a subsumption of all values under the doctrine of congruity. There must be a congruous origin for the maintenance of "the value of our highest beliefs and emotions. Beauty must be more than an accident. The source of morality must be moral. The source of knowledge must be rational." A source that is rational, moral, and capable of originating beauty is—Theism.

This "Doctrine of Congruity" is simply another name for the prepossession, or indurated prejudice, of the reflective human mind that the effect cannot be superior to, or essentially different from, its cause or series of causes. Stated in one of its simplest forms, this prejudice is equivalent to the assumption that to every item in the effect there corresponds a similar item in the cause. It is a product of reflection, for in the naïveness of primary experience there may be great unlikeness between producer and produced. Thales regarded water as the parent of all, rational and non-rational. The egg is an unconscious body, while the chick is both conscious and intelligent. Rational humans develop from infants that are little more than bundles of vague feelings and sensations, and spring, in their turn, from germs devoid not merely of intelligence but of consciousness itself. There is but little apparent equivalence between the death of a man and the flight of a bullet, between

the heat of a fire and the gratefulness of its warmth or between the forest oak and the humble acorn. In the sphere of life the river appears, whatever correction may be applied by reflection and analysis, to flow higher than its source. Evolution has proceeded from protoplasm to man, and from rude culture to civilised communities. Immediate experience, so far from confirming Mr. Balfour's doctrine that reason must have a rational source, suggests that the superiority of effects to their causes is a fundamental law of the animate world.

The "Doctrine of Congruity" may serve as a working principle in the mechanical or physical sphere. A system cannot manifest more energy than it possesses or acquires from other sources. The quantity of matter with which an operation ends is usually equivalent to the matter present at the start and to any that may be introduced as the operation proceeds. Mr. Balfour mechanises the principle of growth when he argues that the end is really the same as the beginning. The human intellect is strongly inclined, from its nature and constitution, to this mechanical habit of thought. We obtain our mental grip, for the most part, by tracing likenesses and perceiving similarities; we end, through an almost inevitable movement of thought, by creating for ourselves the prejudice that there can be no real difference between the last effect and the first cause. Hence the notion that the rational must proceed from the rational, the moral from the moral, the appreciation of beauty from an original æsthetic perception. But if growth be the type of the fundamental universal processes, processes therefore in which the end may be both different from and superior to the beginning, it becomes not merely possible but probable that the rational issues from the non-rational. The "Doctrine of Congruity" requires a source similar to the final result; the "Doctrine of Growth" requires a difference between the end and the beginning. Uncorrupted experience believes in growth; mechanised thought believes in congruity. Mr. Balfour's argument for Theism depends on preferring the claims of the mechanical to those of the vital or organic.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

IT is curious to read in the list of new publications in the *Bulletin of the American Mathematical Society* (1915, **22**, 155) that another part of the French *Encyclopédie des sciences mathématiques* was published by Teubner at Leipzig in 1915. It is to be hoped that this is an indication that the truth that science is not merely a national question is by no means always denied.

Among the mathematicians who have died during the year 1915 are to be mentioned, besides Morgan William Crofton (see SCIENCE PROGRESS, 1915, **10**, 276), the American George William Hill (1838–1915), whose name is associated with dynamical astronomy and in particular the theory of the moon's motion, in which infinite determinants were introduced; William Grylls Adams (1836–1915), whose mathematical work was mainly in applications to physics; and Henry William Lloyd Tanner (1851–1915), whose original work was mainly on the theory of differential equations, the theory of numbers, cyclotomy and group-theory. Notices of all these men appear in *Proc. Lond. Math. Soc.* 1915, **14**, xxix.

History.—Prof. Gino Loria (*Scientia*, 1915, **18**, 357) gives a sketch of the conceptions of infinity and infinitesimal among the mathematicians of antiquity. It is remarkable that Zeno should be said to come, in history, *after* Democritus, and that Zeno's two last arguments, which are by far the most interesting, should not be mentioned, especially as neither the mistake nor the omission is made in Loria's *Le Scienze esatte nell' antica Grecia* of 1914. The second part of the article (*ibid.* 1916, **19**, 1), which deals with the conceptions from the middle ages to the end of the seventeenth century, is of great interest and contains some things which are new to most of us. Loria puts in a strong plea for the publication of *all* the manuscripts both of Newton and Leibniz. In connection with the subject of the first part of Loria's article, it should be men-

tioned that Prof. Florian Cajori has brought to a conclusion his painstaking and valuable researches (*Amer. Math. Monthly*, 1915, **22**) on the history up to modern times of Zeno's arguments on motion, referred to in SCIENCE PROGRESS for July, 1915 (**10**, 116). Cajori (*Monist*, 1915, **25**, 495) continues his valuable paper describing the work of William Oughtred (cf. SCIENCE PROGRESS, 1916, **10**, 431). In this concluding part the influence of Oughtred on the teaching of mathematics is investigated in great detail. Another welcome example of a detailed historical paper is a study of the geometrical work of Colin Maclaurin by C. Tweedie (*Math. Gaz.* 1915, **8**, 133). Tweedie gave a presentation in modern form of Maclaurin's *Geometria organica* in a paper read to the Royal Society of Edinburgh on December 6, 1915.

Pierre Boutroux (*Rev. de Métaphys. et de Morale*, 1914, **22**, 814; published in November 1915) makes a study of the historical significance of Descartes' *Géométrie*. It is well known that Fermat seems to have been led in his invention of a method of co-ordinates by a method sometimes used by Apollonius; but the repetition of this fact is the only thing which appears of value in Boutroux's paper.

At the end of a review by Prof. R. C. Archibald (*Bull. Amer. Math. Soc.* 1915, **22**, 125) of some books on the life and work of the late Henri Poincaré, there is a valuable list of recent memoirs on Poincaré.

Logic and Principles of Mathematics.—One of the pioneers of the science of symbolic logic, who has not been generally known in this connection, is Bernard Bolzano, the first volume of whose *Wissenschaftslehre* was reprinted at Leipzig in 1914.

Philip E. B. Jourdain (*Monist*, 1915, **26**, 633) attempts a contrast between mathematicians and philosophers in their ways of regarding logic. The comparison is not always favourable to the mathematicians, as the name of George Berkeley reminds us.

A translation of part of Gottlob Frege's *Grundgesetze der Arithmetik* is given in the same number of the *Monist* (1915, **25**, 481) and prefaced by a short account of Frege's other work on logic and the principles of mathematics. This translation is to be continued by translations of other parts of the same book which can be put into ordinary language.

C. D. Broad (*Mind*, 1915, **24**, 464) gives a very able dis-

cussion of what we can mean by the question as to whether our space is Euclidean or not, on which Poincaré and Russell have come to such different conclusions. Broad's final form of his question is: "Subject to the conditions that space is to be changeless and homogeneous and not to act on matter, and that matter is to move about in space, can we construct a system of physics which assumes Euclidean geometry for space, and enables us to deal consistently and adequately with all the data that scientists agree to be most worthy to be taken into account? . . . Of course the only way to answer such a question as this is actually to try and construct such a system of physics. If you can do it, space is Euclidean; if not, then space may not be Euclidean. . . ." In this connection the reader should refer to an admirable critical notice by Broad (*ibid.* 555) of A. A. Robb's *Theory of Time and Space* published at Cambridge in 1914.

Dr. Robert L. Moore (*Bull. Amer. Math. Soc.* 1915, **22**, 117) adds a note modifying in certain respects his results about axioms for the linear continuum obtained in the *Annals of Mathematics* for 1915 (see SCIENCE PROGRESS, 1916, **10**, 433).

Meyer G. Gaba (*Trans. Amer. Math. Soc.* 1915, **16**, 51) gives a set of postulates for general projective geometry in terms of *point* and *transformation*.

The definition of a "plane curve" in terms of the conceptions of the theory of aggregates is known to be of extremely great importance in the theory of functions of a complex variable. Eric H. Neville (*Journ. Indian Math. Soc.* 1915, **7**, 175) gives a definition of it as "a multiple perfectly connected plane set which coincides with its own edge," the conceptions used being those familiar to mathematicians from, for example, Dr. W. H. Young and Mrs. Young's *Theory of Sets of Points*. Neville's definition excludes the space-filling curves of Peano and E. H. Moore.

Theory of Numbers and Analysis.—G. B. Mathews (*Proc. Lond. Math. Soc.* 1915, **14**, 464) considers the division of the lemniscate into seven equal parts. The same author (*ibid.* 467) gives a direct method in the multiplication theory of the lemniscate functions and other elliptic functions.

S. Ramanujan (*Journ. Indian Math. Soc.* 1915, **7**, 173) has an interesting note on finding the sum of the square roots of the first n natural numbers.

Charles de la Vallée Poussin (*Trans. Amer. Math. Soc.* 1915, **22**, 435) gives a long and important memoir, which is practically a whole original treatise, on Lebesgue's integral.

Prof. M. B. Porter (*Bull. Amer. Math. Soc.* 1915, **22**, 109) proves an important theorem on a class of functions of limited variation which Vitali in 1905 called " absolutely " continuous functions.

Prof. G. Mittag-Leffler (*Sitzungsber. der Kgl. Bayer. Akad. der Wiss. zu München, Math.-phys. Klasse,* 1915, 109) gives an interesting account of his work, especially since 1898, on the analytical representation of a one-valued branch of a monogenic function, in which the existence of an analytic function for any " star " of convergence was proved by actual construction. This paper is particularly valuable for the exposition it gives of the work of contemporary mathematicians and the relation of Mittag-Leffler's ideas to theirs. An appendix to this paper contains a proof of a theorem of Marcel Riesz due to G. H. Hardy. A note to this paper is of great interest for those who are interested in the early history of the theory of functions : Weierstrass, who at the beginning of his work did not know Cauchy's theorem on the radius of convergence of a power-series, always began his lectures on the theory of analytic functions by proving an equivalent theorem.

Joseph Slepian (*Trans. Amer. Math. Soc.* 1915, **16**, 71) studies the functions of a complex variable defined by an ordinary differential equation of the first order and degree.

Under the direction of Prof. E. T. Whittaker, the Mathematical Department of the University of Edinburgh has shown great activity of late. Besides the production of a series of tracts, principally on subjects connected with the Mathematical Laboratory, which are reviewed elsewhere in the present number, a series of " research papers " has been issued by this Department. These papers are separate copies of papers lately published in various mathematical periodicals and provided with special blue paper covers. Of these papers the eleventh is an introduction by L. R. Ford (*Proc. Edinb. Math. Soc.* 1915, **33**) of what he calls " successive oscillation functions," which are derived from functions—in the most general sense of the word—of a real variable. The oscillation function of the first order is well known from the work of Baire, but here Ford considers an oscillation function of this one, and so on,

and proves two general theorems of importance about such oscillation functions.

No. 9 of the same series is an extension by Ford (*ibid.*) of some familiar theorems concerning the relations between the roots of a polynomial and those of its first derivative to the more general case of the rational function with a pole at a single point.

But most of these papers concern the subject of spherical harmonics, and here Whittaker's influence is very marked. Edward Blades (*ibid.*) finds what form the function which is the integrand in Whittaker's general solution of Laplace's equation must have in order that the solution may be a spheroidal harmonic. Whittaker (*ibid.*) continues his work of 1912 and 1914 on integral equations corresponding to Mathieu's equation and Lamé's equation, of which the latter has already been noticed in this Quarterly for July 1915 (**10**, 120) and October 1915 (**10**, 279). In a paper on a wide class of equations of the same kind (*ibid.*) Whittaker obtains the integral equation which is satisfied by their solutions.

The solutions of Mathieu's differential equation or the equation of elliptic cylinder functions have also been considered by E. Lindsay Ince (*Proc. Edinb. Math. Soc.* 1915, **33**). The general solution of this linear differential equation is known to be of a type involving two periodic functions $\phi(z)$ and $\psi(z)$, which under certain circumstances cease to be distinct, when the general solution degenerates into a single solution. Ince discovers and investigates the nature of the corresponding second solution. A. G. Burgess (*ibid.*) considers determinants which give the infinite series of relations between a and q in the coefficient of y in Mathieu's equation, when the solutions are purely periodic. Another paper by Ince (*M.N. Roy. Astron. Soc.* 1915, **75**, 436), concerned with the extension of Mathieu's equation, known as Hill's equation in G. W. Hill's Lunar Theory of 1877, gives an application of Whittaker's method of solving Mathieu's equation to obtain a general solution of Hill's equation.

Archibald Milne (*Proc. Edinb. Math. Soc.* 1915, **33**) investigates the disposition of the roots of the confluent hypergeometric functions, and exhibits the results in a graphical form. In particular the zeros of the parabolic cylinder functions are discussed.

Prof. H. M. Macdonald (*Proc. Lond. Math. Soc.* 1915, **14**, 410) gives in analytical form the argument, which demonstrates the theorems used in his treatment of diffraction in his *Electric Waves*, that the constants of the requisite series are the same for the solution of the diffraction problem and for the corresponding potential problem. Dr. T. J. I'A. Bromwich (*ibid.* 450) considers the problem of the diffraction of waves by a wedge by a generalisation of the process used in electrostatics of taking images, by replacing the sum of the effects of n images by a complex integral.

Maurice Fréchet (*Trans. Amer. Math. Soc.* 1915, **22**, 215) considers the representation of bilinear "*fonctionnelles*."

Sir Ronald Ross (SCIENCE PROGRESS, 1915, **10**, 218; 1916, **10**, 393) gives a restatement of his researches published in 1905 and subsequent years on his notation for operations and the solution of equations by operative division. The third part of his researches appears in the present number.

Prof. E. W. Hobson (*Proc. Lond. Math. Soc.* 1915, **14**, 428) proves more simply Plancherel's theorem on the convergence of a series of normal orthogonal functions; and then gives a proof of Weyl's result on the summability of such series in accordance with Cesàro's method, which is free from the intricacy of Weyl's own method.

Eric H. Neville (*Math. Gaz.* 1915, **8**, 151) gives an account of his method of solution of numerical equations which has been already referred to in SCIENCE PROGRESS for October 1915 (**10**, 279).

Geometry.—Among papers on geometry, we will mention those by Gaston Darboux on families of surfaces whose orthogonal trajectories are plane curves (*Bull. Sci. Math.*, 1915, **39**, 62), G. M. Green on the theory of curved surfaces and canonical systems in projective differential geometry (*Trans. Amer. Math. Soc.* 1915, **22**, 1), William Caspar Graustein on the equivalence of complex points, planes, and lines with respect to real motions and certain other groups of real transformations (*ibid.* 33), Virgil Snyder and F. R. Sharpe on certain quartic surfaces belonging to infinite discontinuous Cremonian groups (*ibid.* 62), E. J. Wilczynski on the general theory of congruences (*ibid.* 311), several papers by J. de Vries on the characteristic numbers of systems of curves and bilinear congruences (*Versl. Kon. Akad. van Wet., Amsterdam*, **23**, 907, 1032, 1226, 1232,

1316, 1320), and a paper on a certain linear congruence by H. J. van Veen (*Nieuw Archief voor Wiskunde*, 1915, **11**, 232).

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Dynamics.—There is a considerable degree of probability, supported by various considerations, that the light reaching us from the stars has suffered a small amount of absorption or scattering in its path, due to the existence of matter in interstellar space. The density of this matter is, of course, exceedingly small and not necessarily uniform. Recent investigations by J. C. Kapteyn (*Ap. J.* xxx. 1909, p. 284) and H. S. Jones (*M.N., R.A.S.*, lxxv. 1914, p. 4) assign a fairly definite value to this absorption, and a somewhat smaller value has been assigned by P. J. van Rhijn (*Doctor's Dissertation*, Groningen, 1915). Its smallness may be judged from the fact that in order to lose one-tenth of its original intensity, light from the stars must travel through space for from five to ten centuries. It would have been thought *à priori* that matter of such tenuity would possess merely a theoretical importance, but Dr. Louis Vessot King has shown in *Trans. Roy. Soc. Canada*, Ser. III, ix. p. 99, 1915, that if an absorption of even this small amount is substantiated by future researches, we must recast some of our fundamental conceptions. Using Rayleigh's law of molecular scattering, which seems justifiable, he calculates that the amount of matter necessary to cause this scattering is equivalent to about $1\cdot3 \times 10^8$ hydrogen molecules per cubic centimetre. The highest vacuum which can be produced artificially contains a density of matter greatly exceeding this, yet it means that in a cubic parsec there is an amount of dust whose mass is 6,300 times the Sun's mass; in the same volume, the average mass of the lucid stars is estimated at only 0·02 of the Sun's mass. Thus there is about 300,000 times as much dust as there is matter condensed into stars. The result is truly startling, and even if the scattering or absorption of light in space is considerably less than present estimates, and if there are a large number of dark stars, it would seem that this residual gas should be taken account of in stellar dynamics, for it will exert an attraction far exceeding that of the stars themselves.

A third paper has appeared by Prof. A. S. Eddington (*M.N., R.A.S.*, lxxvi. 1915, p. 37) in continuation of his important researches in stellar dynamics. In this paper some of the restrictions imposed in previous papers have been removed, and more general systems discussed. The restriction of spherical symmetry is removed, and in order to allow for a possible influence of matter distributed throughout interstellar space, as mentioned above, it is supposed that the system moves under any potential of force instead of merely under its own attraction. Many of the results obtained hold also whether the system has attained a steady state or not. Assuming the velocity distribution to be according to Schwarzschild's law, the velocity ellipsoids define three orthogonal systems of curves which are called the *principal velocity surfaces*. It is found that, even if a steady state has not been reached, these surfaces must be confocal quadrics. For a system moving under its own attraction, the only possible solution is one of spherical symmetry.

J. H. Jeans, *M.N., R.A.S.*, lxxvi. 1915, p. 70, in a paper on the theory of star-streaming and the structure of the universe has obtained some important results of a very general nature. If the law of star-streaming possesses no symmetry, then to satisfy the kinematical conditions of the system, the universe must be spherical; if it possesses one degree of symmetry, so that it is represented by a figure of revolution about an axis, then the universe must also be a figure of revolution. If the law has two degrees of symmetry, or is spherical, then the universe can have any shape. Further restrictions are, however, imposed by the dynamical conditions of the system, and an examination of these indicates that with a spherical law of star-streaming the universe must also be spherical. With the conditions prevailing in our own universe, the conclusion is reached that a steady state is impossible and that star-streaming does not represent a steady state. As was pointed out, however, by Prof. Eddington when this paper was read (*Observatory*, xxxix. 1916, p. 41), there are strong arguments for believing that a steady state gives a good approximation to what is going on in our own universe, and a consideration of steady systems may throw considerable light on what is actually happening.

Displacement of Solar Spectrum Lines.—The accurate

measurement of the wave-lengths of the Fraunhofer lines by Rowland and others, when compared with the measures of the wave-lengths of the same lines as obtained from terrestrial sources, shows that there is a general shift of the Fraunhofer lines towards the red. Various reasons have been assigned for this shift and several recent communications have dealt with the subject. The original explanation was that the effect was one due to pressure, the requisite pressure in the sun's atmosphere being about 5 atmospheres. This explanation has several strong arguments against it. Julius attempted an explanation based on his theory of anomalous dispersion in the Sun's atmosphere. If this theory is true there should be a mutual influence of the Fraunhofer lines upon one another; the theory requires that the effect should have opposite signs for a given line according as the companion line is towards its blue or its red side, and that it should be larger in the latter case than the former. Julius claimed that his theory was supported by the measures made by St. John of the displacement of the Fraunhofer lines at the edges of sun-spots. St. John (*Ap. J.* xli. 1915, p. 28) denied this, stating that systematic errors were introduced by the method of discussion and the selection of lines adopted by Julius. The question was taken up again by Sebastian Albrecht (*Ap. J.* xli. 1915, p. 333), who compared the wave-lengths of the iron lines in the solar spectrum, as given by Rowland, with the laboratory wave-lengths of the same lines on the International system. This made it possible to eliminate systematic pressure shifts. Albrecht then found that lines with close companions towards the red were shifted towards the violet and those with close companions towards the violet were shifted towards the red, the former effect being the larger. These results are in accordance with the deduction of Julius's theory, and Albrecht accordingly concluded that his results seemed definitely to establish the operation of anomalous dispersion in the Sun. J. Evershed (*Observatory*, xxxix, 1916, p. 59) regards these results as fictitious. He remarks that on Julius's theory all close double lines should show a wider separation in the solar than in an arc spectrum, where there is no question of anomalous dispersion. Photographing the Sun and arc spectra together, and selecting at random two pairs of iron lines well shown in both Sun and arc spectra,

he finds their separation not markedly different in the two cases and concludes that Albrecht's results are probably illusory. A similar conclusion is arrived at by Dr. T. Royds (*Kodaikanal Bulletin*, xlviii.), who has measured directly the sun-minus-arc displacements of different lines, using photographs of solar and arc spectra on the same plane. Comparing the sun-arc displacement with Albrecht's residuals, he finds that the former are only about one-quarter as large as the latter and concludes that the latter are fictitious. The question has been taken up from the theoretical point of view by Sir J. Larmor in the *Observatory*, xxxix, 1916, p. 103, who shows that very close spectrum lines ought to repel each other, if they represent independent vibrations. If then an iron line in the solar spectrum has a close companion due to another substance, a displacement of the solar line relative to the arc line may be looked for. If, however, the two adjacent lines are due to the same substance, as in the researches of Evershed and Royds, then no sensible difference should be expected, as regards their separation, between the solar and arc spectra. These results are an intrinsic property of the vibrations of the source, and any effects due to anomalous dispersion would be superposed on these. It seems probable, therefore, that the results so far obtained cannot be regarded as either establishing or refuting the theory of anomalous dispersion.

A Trans-Neptunian Planet.—In *Memoirs of the Lowell Observatory*, vol. i. No. 1, Percival Lowell has embodied the results of investigations, extending over many years, on the problem of a possible trans-Neptunian planet. The investigation is based upon the residuals between the places of Uranus obtained by observation, and those given by Leverrier's theory, and Gaillot's later and more accurate theory. Adams and Leverrier when searching for Neptune had residuals amounting to 133″ to base their investigations upon, whereas the residuals of Uranus do not anywhere exceed 4″·5. Such small residuals, affected as they are by errors of observation and possible errors in the theoretical positions, cannot in the nature of events furnish a conclusive proof of a new planet. The residuals are, however, decreased by 71 per cent. on that hypothesis, so that there is a reasonable probability as to its reality. The position of the planet can, however, be but vaguely

indicated. It might have been thought that it would have been better to base the investigation upon observations of Neptune; this was not done because less than half of its orbit has been described since it was first observed, whereas there are observations of Uranus dating back to 1690, when it was observed by Flamsteed (who, of course, was ignorant of its identity). Consequently the orbit of Uranus is better known.

The mass of the planet is indicated as seven or eight times that of the Earth's mass, and its stellar magnitude as about twelve or thirteen. The next step lies with observation, and the planet—if it exists—may be found either visually by its visible disc, or photographically by its trail relative to the stars.

PHYSICS. By J. Rice, M.A., Lecturer in Physics, University of Liverpool.

In the *Proc. Roy. Soc.*, December 1915, Prof. Duffield gives a description of experiments carried out by himself and some pupils on the consumption of carbon in the electric arc. One extremely interesting and very simple conclusion is arrived at, viz. that when the arc length is extremely small, the loss from the cathode of one carbon atom is accompanied by the transfer between the poles of a quantity of electricity equivalent to four electronic charges. For long arcs there is a further loss of carbon due to combustion or evaporation. The loss per coulomb for a given current density in the carbons increases with increasing arc length until a nearly constant value is reached at about 8 mm. This maximum value is to be expected, as a limit is reached when increasing the length of the arc does not augment the amount of air which by gaining access to the hot poles causes oxidation.

As is well known the anode loss is greater than the cathode loss, a fact accounting for the existence of the crater; the ratio of the two losses varies for the same length of arc with the current strength; for long arcs Prof. Duffield finds the ratio rises from 1·36 to 1·64 when the current varies from 2 amperes to 8 amperes. In long arcs the loss of material per coulomb decreases with increasing current. This result is opposite to what might have been expected, since so much more heat is generated by the larger currents; but it should be observed that it is the loss of carbon *per coulomb* of electricity which is in question; with small currents the

time taken for the passage of a given quantity of electricity is longer, so that probably the extraneous combustion which causes the loss additional to that due to the convection of the charge through the arc is greater. In short, doubling the time causes more carbon to burn away than doubling the current.

But, as mentioned above, it is the work with the short arc which leads to the most interesting result; no matter what strength of current was used, if the arc was reduced to a very short length, so that the subsidiary burning was of a small order of magnitude, the loss per coulomb *from the cathode* approached the value 3×10^{-5} gram (no regularity in the behaviour of the anode in these circumstances was suggested by the curves obtained). The coincidence of this number with the value for the electrochemical equivalent of carbon ($3{\cdot}109 \times 10^{-5}$), on the assumption that this element is quadrivalent, appears too striking to be accidental and must be closely connected with the mechanism of the arc.

Various theories of the arc have been put forward in recent years. Fleming, as long ago as 1890, suggested that "the negative carbon is projecting off a torrent of negatively electrified carbon molecules and these impinging against the positive carbon wear out a crater in it by a sandblast-like action." Thomson in his *Conduction of Electricity through Gases* puts forward the view that "the cathode is bombarded by positive ions, which maintains its temperature at such a high value that negative electrons come out of the cathode; these, which carry by far the larger part of the arc discharge, bombard the anode and keep it at incandescence; they ionise also, either directly by collision or indirectly by heating the anode, the gas or vapour of the metal of which the anode is made producing in this way the supply of positive ions which keep the cathode hot. It will be seen that the essential feature of the discharge is the hot cathode."

Prof. Duffield discusses these theories in the light of the fresh information elicited by his research. 'The first inclination is to regard the additional fact referred to above as favouring the earlier view of Fleming, viz. that in the immediate neighbourhood of the cathode the entire current is carried by carbon atoms, each of which as it leaves the cathode takes with it four electrons derived from the source of current-supply. The

objection to this view is that it offers no explanation of the fact that the molecules of the gas must penetrate to the surface of the cathode (probably conveying positive charges to it) and there interact with carbon atoms instead of waiting till the latter have proceeded some distance from the cathode, for it is well known that an arc can only with great difficulty be run in an atmosphere which does not readily form a chemical compound with the material of the pole.

Dr. Duffield regards the interaction of the gas molecules with the carbon as playing a very important rôle in the mechanism, and that, while permanent chemical compounds are probably not formed in such a high-temperature region as the arc, yet there may be momentary interaction between carbon and gas atoms rendering the electronic content of the former atoms unstable and causing them to yield up some of their store. There are of course many ways in which the yielding of the electrons could take place. Thus the atom might eject the four electrons obtained from the current supply under the influence of thermionic or photo-electric action and subsequently be liberated uncharged, or the carbon atom could be detached from the cathode by interaction with the gas and the four electrons liberated at the same instant; or in the latter case only two electrons might be liberated, and, say, one positively charged (divalent) oxygen atom might arrive at that instant and form uncharged carbon monoxide; or no electrons might be liberated and uncharged carbon dioxide formed.

Prof. Duffield inclines to the Thomson theory for other reasons also. Carbon has not been found free with four electronic charges; and further there is the fact that Duddell has located the back E.M.F. of the arc at the surface of the anode and this seems to require negative, *i.e.* electronic, emission rather than positive there. Hence the author is anxious to retain the electronic view and is at pains to point out that electronic emission in some of the ways mentioned above is just as compatible with the facts as Fleming's early view which regards the emission as being atomic or molecular.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Stoicheiometry.—In this connection Bousfield (*Trans. Chem. Soc.* **107**, 1781, 1915) has continued his investigation of the density and viscosity of aqueous solutions with special reference to nitric acid. The apparatus employed is complex, and the measurements aim at a high degree of accuracy. The results obtained are not easy to interpret, but they form an essential part of the experimental basis upon which a satisfactory theory may eventually be reared. The abnormalities observed must be attributed to the complex nature of water itself and to the changes in the constitution of water brought about by change in temperature and by the presence of the solute.

It is also necessary to draw attention to the very valuable contribution made by Applebey and Hughes (*Trans. Chem. Soc.* **107**, 1798, 1915) to the determination of the vapour pressures of saturated solutions. The vapour pressures of aqueous solutions of sodium nitrate, sodium sulphate, and thallium nitrate were measured by a static method over a range of temperature in the neighbourhood of their boiling points. The two last named salts gave results in excellent agreement with those obtained by Lord Berkeley by the dynamic method.

Chemical Kinetics and Catalysis.—An ingenious method for the indirect determination of the velocity of hydrolysis of formamide by hydrochloric acid has been worked out by J. C. Crocker (*Trans. Chem. Soc.* **107**, 1762, 1915). The principle of the method consists in having sucrose present in the solution, the sucrose undergoing inversion, its rate of change being followed by means of a polarimeter. The rate of the inversion depends upon the concentration of hydrogen ions present, which in turn affords a measure of the amount of hydrochloric acid present at any moment. The acid is being used up all the time by the hydrolysis of the amide and the rate of inversion gives a measure of the extent of the amide hydrolysis. Naturally the hydrogen ion diminishes as time goes on and therefore the rate of inversion falls off due to this cause. It is easy, however, to calculate the true velocity constant of the amide hydrolysis and very concordant values are obtained. The principle of the method is applicable to any reaction where the hydrolyst (the acid) changes in concentration during the reaction and can act as a simple catalyst on a third substance present

in solution. It thus may be employed in cases in which a direct determination would present difficulty.

A type of work which appears capable of extending very considerably our knowledge of the mechanism of the reactivity of a solvent, when participating stoicheiometrically in a reaction, has been commenced by Holmes and Jones (*Journ. Amer. Chem. Soc.* **38**, 105, 1916) under the title "The Action of Salts with water of hydration and without water of hydration on the velocity of saponification of esters." The object is to find out, if possible, whether there is any difference between the reactivity of "free" water, *i.e.* solvent, and water present in a "bound" form as water of hydration in certain dissolved salts. The mode of testing the relative reactivity is by measuring the rate of saponification of methyl acetate and methyl formate. It is found that in general the velocity of saponification is greater in the case of highly hydrated salts than with those in which hydration is absent and the reactivity is therefore due to the solvent. The conclusion is that bound water is more reactive than free water. This is a very striking result. Naturally one has to be certain that the two series of measurements are truly comparable. The authors state that the acidity produced by the hydrolysis of the salts will not account for the difference in the speed. One would wish this were more clearly demonstrated. In any event the further investigations promised on these lines will be awaited with interest.

As a problem of considerable technical importance, the catalytic bleaching of palm oil has been investigated by Sastry (*Trans. Chem. Soc.* **107**, 1828, 1915). The object is to oxidise the material as rapidly as possible, without its being otherwise attacked, and to avoid the labour and expenditure of time required if the oxidising agent has to be washed out of the oil. The author's experiments consist in blowing air or oxygen through the oil in the presence of various catalysts, both oil and air being kept at a temperature of 80–90° C. Among the catalysts employed were salts of manganese, lead, cobalt, iron, and nickel. The best catalysts were found to be the salts of cobalt and manganese, especially the borates of these metals. The larger the amount of catalyst present the shorter the time required. The bleaching effect is permanent, no colour having manifested itself after keeping for fifteen months. The

author recommends finally the use of cobalt borate as the most suitable catalyst for technical purposes.

Electrolytic Dissociation.—As is well known, the outstanding difficulty in regard to equilibrium between ions is the so-called "Anomaly of strong electrolytes." Strong electrolytes, *e.g.* salts, when dissolved in water dissociate into ions in such a manner, that, with increasing concentration the mass action equilibrium constant, in place of remaining unchanged, rises markedly and does not even exhibit the same order of magnitude. Since this anomaly occurs in those cases in which the dissociation is very extensive the two effects have been more or less associated. The work of Schlesinger and Coleman (*Journ. Amer. Chem. Soc.* **38**, 271, 1916), however, has disproved the conclusion that large dissociation *per se* is the cause of the anomaly. These authors have investigated, by means of electrical conductivity, the degree of dissociation of alkali formates dissolved in anhydrous formic acid. This solvent resembles water in being an excellent dissociating medium. The dissolved formates are therefore highly ionised, *e.g.* a decinormal solution of sodium formate is dissociated to the extent of 88 per cent. at 18° C., and similar values are obtained for the lithium, rubidium, and cæsium salts. In spite of this extensive ionisation, however, these electrolytes in formic acid obey the Ostwald "Dilution Law" with a high degree of accuracy. It looks, therefore, as if the "anomaly" of strong electrolytes may be attributed eventually to the abnormality of water as a solvent.

Phase Equilibria and Change of State.—A useful contribution to this field has been recently published by E. Jänecke (*Z. Phys. Chem.* **90**, 265, 1915) by the determination of fusion points and transition points with the help of a special electrically heated pressure apparatus. The method has been applied with success to the accurate determination of the melting points of a number of hydrated salts of sodium, calcium, barium, and copper. In connection with the hydrates of copper sulphate Jänecke infers from his results that a subhydrate (probably $CuSO_4 \frac{1}{2}H_2O$) exists. The author has also redetermined the transition points of the nitrates of silver, ammonium, and potassium, and has discovered a new modification of potassium nitrate. Finally, Jänecke has verified Cohen's work on the transition of the stable and unstable forms of metals

including tin, zinc, bismuth, cadmium, copper, silver, lead, and antimony. There can no longer be any doubt but that many metals as we know them are metastable (as has been insisted on by Cohen) and in these cases are a mixture of two allotropic forms such as are postulated in Smit's theory of allotrophy.

Quite a different type of investigation, though at the same time one dealing with change of state, has been carried out by Walton and Brann (*Journ. Amer. Chem. Soc.* **38**, 317, 1916), who studied the effect of dissolved substances upon the velocity of crystallisation of water. It is well known that a super-cooled liquid tends to crystallise. Tammann was the first to investigate this phenomenon fairly closely. He showed that it involved in general three temperature zones. The speed of crystallisation at first increases with increased super-cooling (zone A); it then generally remains constant for several degrees (zone B); and finally decreases (zone C). The present investigation refers to zone A. The velocity of crystallisation was determined at 9° below zero for forty-five substances. It was found that in all cases the velocity was retarded. Solutions of equi-molecular concentration show different retarding effects. The effect is therefore more or less specific. For substances with more than eight atoms in the molecule there is a rough relation between the number of atoms and the retardation of the velocity; the greater the number of atoms, the slower the speed. That these effects cannot be explained by capillary effects is evidenced, according to the authors, by the fact that the sugars which are inactive in a capillary sense are very active in retarding the rate of crystallisation. Colloidal substances such as gelatine, ferric hydroxide and certain dyes inhibit the rate. Further, the stability of the super-cooled solutions towards spontaneous crystallisation varies with the solute. Decimormal solutions of hydrochloric acid are characterised by being exceptionally stable. As regards the mechanism of the retarding influence in general, it appears that the effect is connected with a shift in the equilibrium which exists between the different polymers of (H_2O) which constitute liquid water. The idea is that, ice being mainly trihydrol ($(H_2O)_3$), any decrease in the quantity of this substance present in the *liquid* state would correspondingly diminish the rate at which crystallisation would take place. This is an interesting hypothesis,

as it indicates that hydration of the solute is a general phenomenon, a conclusion which has been reached on other grounds.

INORGANIC CHEMISTRY. By C. SCOTT GARRETT, D.Sc.

Water of Crystallisation.—As we have already pointed out in these quarterly reports, the whole subject comprising what is somewhat loosely termed " water of crystallisation " is one which so far has not received the attention which it merits. Chemists are prone to be satisfied with merely stating the empyrical number of molecules of water or other solvent occurring in a compound without trying to differentiate between them or allotting to them specific structural positions. The solution of these and other problems in this field, besides being interesting, will furnish considerable aid in advancing our knowledge of structure and reactivity in inorganic chemistry.

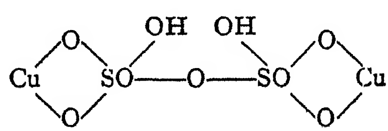
Some work in this connection has recently been published by Guareschi (*Atti R. Accad. Sci. Torino*, 1915, **50**), who has made measurements of the amount of water driven off from different types of hydrates over various temperature ranges and under various conditions.

In the case of dihydrated calcium sulphate, for instance, both the natural variety and that obtained by precipitation dehydrate at 93–94°, or in a current of dry air at 81–82°, but whereas the former dehydrates readily, the latter loses water very slowly. And, as is well known, a moderately stable semi-hydrate is obtained which, in the absence of further data, probably possesses a composition represented by the formula $Ca_2S_2O_8.H_2O$. Sodium nitroprusside crystallises with empyrically two molecules of water, and when dehydrated it loses water readily at 98–99° up to an amount corresponding to one and a half molecules of water. The final half molecule of water is only separated slowly and with difficulty, and for this reason the hydrate is probably correctly represented by the formula $[Na_2Fe(NO)(CN)_5]_2.4H_2O$. Moreover, the reactivity of the hydrate molecules in this salt would seem to vary with the conditions under which the hydrate has been formed, for when the salt has been dehydrated and allowed to rehydrate itself, it then can be dehydrated again much more easily.

Many so-called dihydrates lose their water of hydration

in too well-defined stages and the formula of the hydrate is then, probably, correctly represented by the usual formula. On the other hand, hydrates such as the above require at least the double formula for correct representation, and in some cases even more multiple formulæ may be required. Thus, dihydrated cadmium acetate loses up to one and three-quarters molecules of water easily in a steam oven, whilst the last quarter requires a further and more elevated stage of heating, indicating that the hydrate contains probably eight molecules of water and ought to be represented by the formula $(CdAc_2)_4.8H_2O$.

With hydrates or solvates where the number of solvent molecules exceeds two, the problem immediately becomes much more complex. The classic case of pentahydrated copper sulphate will serve as an example. Over calcium chloride at 21–23° two molecules of water are lost, and the pale sky-blue trihydrate remains. This hydrate gives up a further two molecules of water at 60°, whilst the last molecule is lost only on heating at 206°. If the pentahydrate is heated at 206–207° it loses four molecules within half an hour, whereas the final molecule is lost only very slowly and in two half-molecule stages, the latter of which is the most difficult of all to carry out. From such considerations as these Guareschi suggests that the monohydrate has a constitution represented by the double formula :



Preparative.—In view of the importance, at the present time, of the conversion, on the industrial scale, of calcium nitrate into ammonium nitrate for explosives, the recent work of Le Chatelier and Bogitch (*Compt. Rend.* 1915, **161**, 475) on the subject is of considerable value. In practice the synthetic Scandinavian calcium nitrate, nitrolim, is converted into ammonium nitrate by double decomposition with gas works ammonium sulphate. The difficulty of the process is the filtration of the pasty precipitate of calcium sulphate which is obtained.

These investigators find that this difficulty can be overcome

by using equimolecular proportions of the two reacting salts dissolved in their own weight of water, and heating at 150° in a closed vessel. The calcium sulphate so obtained is a sandy crystalline mass readily susceptible of washing by decantation with hot water, and from which practically the whole of the ammonium nitrate can be obtained, free from calcium sulphate, in two washings.

Of more scientific than industrial interest is the method of preparation of chlorites discovered by Bruni and Levi (*Gazzetta*, 1915, **45**, 161). Pure barium chlorite can be obtained by the action of chlorine dioxide and carbon dioxide which have been freed from chlorine, on barium peroxide suspended in hydrogen peroxide solution. The sodium salt can be prepared from the barium salt by double decomposition with sodium sulphate, and a red basic mercuric salt $3Hg(ClO_2)_2.HgO$ is obtained when mercuric nitrate is used. Other insoluble salts are obtained in a similar manner, but the ammonium and hydroxylamine bases are exceptions. The mercuric salt is explosive in the dry state and with concentrated sulphuric acid all the solid chlorites deflagrate more energetically than the corresponding chlorates. The soluble salts do not give a precipitate with mercuric chloride, but the red precipitate is formed when mercuric nitrate is added to a neutral and not too dilute solution. The mercurous salt is yellow and unstable and rapidly oxidises, to the red salt in the air.

Analytical.—A rapid and accurate method of estimating chloride and bromide in presence of each other has been devised by Meyer (*Chem. Zeit.* 1915, **39**, 708), which ought in future to find a place in analytical text-books. A known volume of the mixed halide solution is titrated with N/10 silver nitrate solution. An equal volume of the solution is completely precipitated with silver nitrate solution and the mixed AgCl and AgBr precipitate is collected in a Gooch crucible, washed, dried, and weighed. The titration gives the weight of silver in this precipitate and the difference of the two weights gives the weight of chlorine and bromine together. From this latter the amount of each can easily be determined by a simple calculation.

Constitution.—A lengthy paper by Weitz (*Ann. der Chem.* 1915, **410**, 17) on the nitrogen compounds of gold contains some data which is of importance from the point of view

of constitution and stability in inorganic chemistry. Amongst many other nitrogen-containing derivatives of gold obtained by reaction between auric chloride or auric hydroxide and ammonia or ammonium salts—compounds of unsettled or indefinite composition—the author has obtained a well-defined tetra-ammineauric nitrate, $Au(NH_3)_4.(NO_3)_3$. This compound crystallises out in glistening needles from a weak suspension of ammonium nitrate in saturated ammonium nitrate solution containing a little auric chloride. The salt is decomposed on heating, but is stable in solution and gives double salts with potassium, sodium, and ammonium nitrates. With soluble salts of other acids, double decomposition takes place and precipitates are obtained where $Au(NH_3)_4$ functions as a specific trivalent metallic radicle. Thus, indicating this new radicle by R, are obtained such salts as the following :

Phosphate . .	$RPO_4 . H_2O$	Oxalate perchlorate	$R.(COO)_2.ClO_4$
Oxalate nitrate .	$R.(COO)_2.NO_3$	Iodate . . .	$R.(IO_3)_3$
Perchlorate . .	$R.(ClO_4)_3$	Sulphate nitrate .	$R.SO_4.NO_3.H_2O$
Sulphate perchlorate	$R.SO_4.ClO_4.2H_2O$	Chromate . .	$R_2.(CrO_4)_3$

Now these compounds afford another example of a phenomenon which is to be found in other inorganic compounds. The trivalent radicle Au is somewhat amphoteric and has not strong enough basic characteristics to form stable salts with the above series of acidic radicles. When, however, its basic character is reinforced by the inclusion within the complex of three molecules of a basic group—in this case NH_3—it becomes capable of forming stable salts with the stronger acidic groups, or in some cases with relatively weak acidic groups, especially if these latter are assisted by the presence of a strongly acidic group, as in the case of the oxalate nitrate.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School.

The proof of the equivalence of the four valencies of the carbon atom which was originally furnished by Henry (*Bull. Acad. Roy. Belg.* 1886, Classe des Sciences, **12**, (111), 644, 1888, **15**, 333 and 1906, 722) can no longer be regarded as rigid in view of the possibilities of intramolecular rearrangement on the lines of the so-called Walden inversion. Fischer and Brieger

(*Berichte*, 1915, **48**, 1517) have accordingly taken up this question once more, making use of a series of reactions in which no substitutions are called into play and which all take place at low temperatures, thereby reducing the chances of a Walden inversion to a minimum. Starting with the optically active half amide of ethyl isopropyl malonic acid

$$\begin{matrix} C_2H_5 & & CONH_2 \\ & \rangle C \langle & \\ C_3H_7 & & COOH \end{matrix}$$

they have obtained by the action of nitrous acid the optically inactive ethyl isopropyl malonic acid, thus demonstrating the equivalence of the two carboxyl groups. Similarly, dextrorotatory allyl propyl cyanacetic acid

$$\begin{matrix} C_3H_5 & & CN \\ & \rangle C \langle & \\ C_3H_7 & & COOH \end{matrix}$$

on reduction has yielded the inactive dipropyl cyanacetic acid which proves the equivalence of the two alkyl groups. It is proposed next to prepare the optically active form of vinyl ethyl cyanacetic acid

$$\begin{matrix} CH_2 = CH & & CN \\ & \rangle C \langle & \\ C_2H_5 & & COOH \end{matrix}$$

and to reduce a portion of it to the inactive diethyl cyanacetic acid and also if possible to oxidise some of it to ethyl cyanomalonic acid

$$\begin{matrix} COOH & & CN \\ & \rangle C \langle & \\ C_2H_5 & & COOH \end{matrix}$$

If these latter reactions are successfully accomplished the equivalence of all the four valencies will have been established.

The intricate question of the assimilation of carbon dioxide by the plant forms the subject of an interesting paper by Willstätter and Stoll (*Berichte*, 1915, **48**, 1540). The authors' method of experimenting was to pass a regular stream of air containing a known amount of carbon dioxide through a small illuminated glass vessel immersed in a constant temperature water bath and containing from 5 to 20 grams of leaves. By estimating the amount of carbon dioxide in the issuing gas and the amount of chlorophyll in the leaves they determined

the ratio between the number of grams of carbon dioxide assimilated in one hour and the weight of chlorophyll concerned, to which ratio they gave the name of assimilation number. Experiments with normal, autumnal, and etiolated leaves showed that the assimilative effect is not always proportional to the chlorophyll content, which is explained by assuming that the process of assimilation is to some extent effected by an enzyme, probably acting at the surface of contact between the chloroplast and the plasma. The fact that in leaves rich in chlorophyll an increase in illumination produces no effect on assimilation, whereas a rise in temperature brings about increased assimilation is explained by the accelerating effect of a rise of temperature upon the enzyme action. In the case of leaves deficient in chlorophyll a rise of temperature has but little influence, whereas increased illumination has a very marked effect. The explanation offered in this case is that there is more than sufficient enzyme present for the chlorophyll present, but that the greatest assimilative effect can only be attained when all the chlorophyll is exerting its maximum activity. All attempts to bring about assimilation by means of chlorophyll isolated from the leaf failed, in all probability owing to absence of enzyme. The removal of the epidermis from the undersurface of the leaves had no deleterious effect on assimilation, whereas only a slight pressure applied to the leaves brought assimilation to a complete standstill.

The effect of plant nutrition upon chlorophyll production has been investigated by Pollacci and Oddo (*Atti. R. Accad. Lincei*, 1915 [v], **24**, ii, 37). Comparative experiments with seeds of *Zea mais* grown in nutrient solutions composed of calcium and potassium nitrates, ammonium sulphate and potassium dihydrogen phosphate with or without the addition of a small quantity of the magnesium salt of pyrrole carboxylic acid showed that the seedlings grown in the presence of the magnesium salt had their leaves at least three times as well developed as the others and their colour was normal, whereas the leaves of the others were ill developed and deficient in chlorophyll.

A new method for the classification and evaluation of caoutchoucs is described by Ostromisslenski (*J. Russ. Phys. Chem. Soc.* 1915, **47**, 1374). According to this author the elastic property of caoutchouc and its property of vulcanising

when treated with sulphur is not confined to natural caoutchouc but should be regarded rather as a state of matter acquired by some amorphous substances of high molecular weight which are called by the author "resinoids." The elastic state is only able to persist within certain temperature intervals; thus caoutchouc loses its elastic properties when cooled to − 20° C. and becomes leathery. The temperature at which a substance acquires elastic properties is called the "temperature of elasticity," while the temperature at which elastic properties are completely lost is called the "fatal temperature." These two temperatures form two new characteristic constants for resinoids, comparable with the melting-point and boiling-point of a crystalline solid, and may be employed for identifying or classifying caoutchoucs. These temperatures may be determined in colloidal solutions, by means of viscosity surface tension, or density measurements at various temperatures, all three methods giving identical results.

Attempts on the part of malingerers to simulate jaundice by taking repeated small doses of picric acid have led to the publication of three papers (*J. Pharm. Chem.* 1915 [vii], **12**, 228, 350 and 366) dealing with the detection of picric acid and the methods employed in distinguishing between pathological jaundice and the condition artificially induced by picric acid, and there appears to be no difficulty in distinguishing between the two conditions by analysis of the urine.

In conclusion one other paper dealing with analysis may be quoted. According to Vintilesco and Popesco (*J. Pharm. Chem.* 1915 [vii], **12**, 318) the rancidity of a fat is due to the absorption of oxygen which may be liberated by a peroxidase and detected by tincture of guaiacum. To ascertain by chemical means whether a given fat is rancid, 10 grams of the sample are melted and shaken for one minute with 5 drops of diluted blood, 10 drops of tincture of guaiacum, and 10 c.c. of water. If the fat is rancid the resulting emulsion is coloured blue. The reaction is given by rancid fats even after they have been heated for a few minutes to 120°.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Dynamical Geology.—The renewed discussion on coral-reefs is continued by Prof. R. A. Daly, who elaborates in detail the glacial-control theory of their origin proposed by himself in 1910

(*Proc. Amer. Acad. Arts and Sciences*, November 1915). Instead of crustal subsidence, a rise of sea level, due to the restoration of water previously locked up in the great Pleistocene ice-sheets, is invoked to explain the observed phenomena. The platforms of uniform depth upon which the reefs grow are supposed to have been formed by wave action during the preceding cold, rough period of general lowering of the ocean level.

The phenomena of the repose periods of Vesuvius, Etna, Stromboli, and Vulcano have been described in an interesting paper by H. S. Washington and A. L. Day (*Bull. Geol. Soc. Amer.* 1915, **26**, 375). This study was incidental to a larger investigation, now proceeding, of the gases, salts, and rocks collected from these volcanoes, with a view to determining the composition of the magmatic gases, their possible exothermic interreactions, and correspondence with the general chemical characters of the erupted rocks. A further object was to confirm the presence of water in the unaltered volcanic gases, as has already been done in the Hawaiian volcanoes, in refutation of Brun's recent hypothesis of the anhydricity of volcanic action.

The *Summary of Progress of the Geological Survey of Great Britain for* 1914 (1915) briefly records the work done in many parts of the country. A volcanic phenomenon of great interest is described from Mull as the remains of a great caldera, with thick marginal breccias, and an internal series of lava-flows 2,000 to 3,000 feet in thickness, which frequently exhibit pillow-structures. This is believed to indicate the former existence of a lake in the caldera; but recent work by Lewis has shown that it is no longer necessary to postulate extrusion into or under water in order to explain pillow-structure. Remarkable vertical intrusions of circular outline, described as ring-bosses or ring-dykes, are believed to have arisen along faults bounding circular subsidences, similar to the well-known cauldron subsidences of Glen Coe and Ben Nevis.

Stratigraphical and Regional Geology.—The debated question of the relative ages of the Moine Gneiss and Torridon Sandstone of the Scottish Highlands has been advanced a stage by Prof. J. W. Gregory's discovery of pebbles of rocks indistinguishable from typical Moine gneisses in the Torridonian conglomerate of Little Loch Broom (*Geol. Mag.* October 1915). The identification is supported by Dr. J. J. H. Teall and Dr. J. Horne.

A re-examination of the north-western part of Charnwood Forest has convinced Prof. T. G. Bonney that he formerly assigned a pyroclastic origin to too many of the rock-types, as at Bardon Hill (*Geol. Mag.* 1915 (6), **2**, 545). The structures formerly interpreted as pyroclastic are now regarded as due to flow-brecciation in the lavas.

At the other end of the geological time-scale, the Danbury (Essex) gravels have been investigated by Prof. J. W. Gregory (*Geol. Mag.* 1915 (6), **2**, 529), who remarks on the abundance of quartzite pebbles foreign to the district, and on the absence of materials such as Jurassic sandstones, large unworked flints, and basalt, which are common in the Essex glacial gravels. These facts support the hypothesis of fluviatile origin and the Pre-Glacial age of the Danbury deposits.

In a paper on the Carrara marble district, Dr. du Riche Preller (*Geol. Mag.* 1915 (6), **2**, 554) re-affirms the Mesozoic age of the marbles, in opposition to the views of Prof. Bonney. They form an integral part of the Lower Mesozoic rocks of the Apuan Alps, and are in no way associated with, or contemporaneous with, the older schists of the district.

The Cordilleran geology of British Columbia is greatly furthered in recent *Memoirs of the Canadian Geological Survey*, which, whilst primarily dealing with economic geology, also describe the general geology of these interesting mountain areas (*Memoir* 58, Texada Island, B.C., by R. G. McConnell; *Memoir* 68, A Geological Reconnaissance between Golden and Kamloops, B.C., along the Canadian Pacific Railway, by R. A. Daly; *Memoir* 56, Geology of Franklin Mining Camp, B.C., by C. W. Drysdale; *Memoir* 76, Geology of Cranbrook Map-Area, B.C., by S. J. Schofield; and *Memoir* 79, On Deposits of the Beaverdell Map-Area, by L. Reinecke, 1915). The primary feature in all these areas is the prevalence of great batholiths of granitoid rocks, with which the mineralisation is frequently connected. The same type of country, but farther to the north-west, is dealt with in two publications of the *Canadian Geological Survey* (*Memoir* 50, Upper White River District, Yukon; and *Memoir* 67, The Yukon-Alaska International Boundary between Porcupine and Yukon Rivers, both by D. D. Cairnes). The *United States Geological Survey* has also been active in Alaska, as witnessed by the recent publication of two bulky memoirs (*Professional Paper* 87,

Geology and Ore Deposits of Copper Mountain and Kaasan Peninsula, Alaska, by C. W. Wright; and *Bulletin* 587, Geology and Mineral Resources of the Kenai Peninsula, Alaska, by G.C. Martin *et alia*).

Glaciology.—Dr. Nils Olof Holst, late of the Geological Survey of Sweden, furnishes an excellent synopsis of the Ice Age in England from a Continental point of view (*Geol. Mag.* 1915 (6), **2**, 424, 434, 504).

The form of the ice in the frozen tundras of the north coast of Alaska is discussed by E. de K. Leffingwell (*Journ. Geol.* 1915, **23**, 635). In contradiction of the view that the ice forms horizontal beds of considerable lateral extent, he shows that it exists chiefly as a network of vertical wedges, thinning downward, and surrounding isolated bodies of the tundra formation. The latter is often much disturbed and contorted in the neighbourhood of the ice; and these observations may help to explain similar phenomena in Pleistocene glacial deposits.

The existence of two glacial stages in Alaska is demonstrated by S. R. Capps (*Journ. Geol.* 1915, **23**, 748) by the discovery in the White River basin of a 3,000 feet thick series of glacial tills interstratified with outwash gravels, assorted sediments, and a few lava-flows. This series can be proved to antedate by a considerable period of time the last great ice expansion, which was probably contemporaneous with the Wisconsin continental glaciation.

Petrology.—A. Scott has described the field relations and petrography of the well-known Crawfordjohn (Lanarkshire) essexite (*Geol. Mag.* 1915 (6), **2**, 455). It is an elongated plug or small boss which has no connection whatever (as formerly thought) with the Tertiary north-west dykes, but must be regarded as an outlying member of the Late Carboniferous alkaline series of Scotland. Analyses of the essexite and of its monchiquitic contact-rock are provided.

Two of the differentiated nepheline-syenite laccoliths of Ontario are described and discussed by W. A. Foye (*Amer. Journ. Sci.* 1915, **40**, 413). He indicates the close association of granite with these rocks, and believes that the nepheline-syenite has arisen by the reaction of limestone with granite, and that gravity has been the controlling factor in the arrangement of the various types within the laccoliths.

A long and important paper by N. L. Bowen on "The Later Stages of the Evolution of the Igneous Rocks" may receive brief mention here, but is deserving of a much more extended notice (*Journ. Geol.* 1915, **23**, Supplement, p. 91). The chief conclusions are that assimilation is relatively unimportant as a factor in the production of the diversity of igneous rocks; and that while differentiation is controlled entirely by crystallisation, the arrangement of the separated units is effected under the influence of gravity.

C. K. Leith and W. J. Mead, in continuation of their "Metamorphic Studies" (*Journ. Geol.* 1915, **23**, 600), develop the view that the formation of slates, schists, and some gneisses by rock flowage, requires a convergence, both chemical and mineralogical, towards a few columnar or platy minerals, which give these rocks their characteristic lamellar structure.

J. Johnston discusses the available experimental evidence as to the function of pressure in the formation of rocks and minerals (*Journ. Geol.* 1915, **23**, 730), and comes to the conclusion that its effect on the equilibrium of a polycomponent chemical system is not especially marked unless one or more of the components is volatile.

ZOOLOGY. By C. H. O'Donoghue, D.Sc., F.Z.S., University College, London.

Protozoa.—Bentham has examined "Some Protozoa from Fishes occurring in the Vicinity of Cullercoats, Northumberland" (*Ann. and Mag. Nat. Hist.* November 1915). He finds Hæmosporidia parasitic in *Cottus scorpius, Scomber scomber*, and *Raia balis*, and also a species of Trichodina on the gill-rakers of the first named.

Invertebrata.—Three new species of Alcyonaria and a Stylaster from the west coast of North America are described by Hickson (*Proc. Zool. Soc.*[1] October 1915). A collection of Land Planarians was made by members of the British Association in Australia and has been described by Dendy (*ibid.* November 1915). Three species all new were obtained from West Australia, and of the six species from Tasmania two are new. The first Land Nemertean, a new species, to be

[1] In the case of papers in the *Proceedings of the Zoological Society* the date given is that on which the paper was read and not the date of publication.

found in West Australia, is recorded by Dakin (*ibid.* November 1915). "Two new Species of Monhystera inhabiting the gill-chambers of Land Crabs" have been discovered by Baylis (*Ann. and Mag. Nat. Hist.* November 1915). Beddard contributes a paper "On *Tænia struthiones*, Parona, and Allied Forms" (*Proc. Zool. Soc.* October 1915), and he defines a probable new species of *Davainea* from the Ostrich (*Struthis masaicus*).

Stebbing contributes notes "On some Enigmatical Names in Conchology and Pycnogonology" (*Ann. and Mag. Nat. Hist.* October 1915).

The Homoptera are treated in "Rhynchotal Notes" (*ibid.* October 1915) by Distant in continuation of a series of previous notes.

Ricardo concludes a previous paper of "Notes on the Tabianidæ of the Australian Region" (*ibid.* October 1915) and records four new genera, *Cænoprosopon, Denroplatus, Pseudotabanus*, and *Pseudopangoina* as well as various species of *Tabanus*.

Turner continues his "Notes on Fossorial Hymenoptera" by describing new Ethiopian species (*Ann. and Mag. Nat. Hist.* October 1915) and the Australian species of Bembex (*ibid.* November 1915). The same author in conjunction with Meade-Waldo and Morley supply "Notes and Synonymy of Hymenoptera in the Collection of the British Museum" (*ibid.* October 1915). Two contributions on Coleoptera come from Arrow (*ibid.* October 1915). The first is "Upon a Remarkable new Genus of Lamellicorn Beetles from Borneo" and the second "Upon the Beetles of the Melolonthid Genus Rhopœa found in the Fiji Islands." "On the Lepidoptera collected in 1913–14 by Herr Geyr von Schweppenburg on a Journey to the Hoggar Mountains" is the title under which Rothschild gives a descriptive list of the species taken (part i. *Ann. and Mag. Nat. Hist.* October 1915, part ii. *ibid.* November 1915). "Descriptions of Three new Neotropical Butterflies" are given by E. M. Bowdler-Sharpe.

The genus *Palæomonetes* is recorded for the first time in Australia by Dakin's description of a new species of prawn-like Crustacean from West Australia (*Proc. Zool. Soc.* November, 1915).

Vertebrata.—Gilchrist contributes some interesting "Obser-

vations on the Cape *Cephalodiscus* (C. gilchristi) and some of its Early Stages" (*Ann. and Mag. Nat. Hist.* October 1915). The eggs are enclosed in a capsule and the larva uniformly ciliated. The adult lives on rocky ground in shallow water.

Various species of fish from the Okhotsh Sea are treated by Schmidt in "On the Pacific Species of Hippoglossoides" (*ibid.* October 1915), and Regan describes the morphology of the Cyprinodont Fishes of the sub-family Phallosthethinæ (*Proc. Zool. Soc.* November 1915). In "A Note on the Parasphenoid of a Palæoniscid" (*Ann. and Mag. Nat. Hist.* October 1915) Day states that his observations point strongly to the Tetrapoda being derived from a primitive Teleostome as against a Dipnoan as is generally maintained.

A "Description of a New Tree-Frog of the Genus Hyla" discovered by Mr. A. E. Pratt in the Arpak Mountains, Dutch New Guinea, is given by G. A. Boulenger (*Ann. and Mag. Nat. Hist.* November 1915). Nicholls calls attention to certain features in which the Bull-Frog *Rana tigrina* differs from its European allies (*Proc. Zool. Soc.* November 1915). The fossil Amphibia receive attention from Moodie, who in "Coal Measure Amphibia and the Crossopterygia" (*Amer. Nat.* October 1915) seeks to establish a phylogenetic relation between these two groups. Evidence of a transitional type of limb is not yet forthcoming.

The observations of E. G. Boulenger on the feeding of snakes in captivity (*Proc. Zool. Soc.* October 1915) seemed to show that generally snakes that refused to accept dead animals would also refuse them if offered alive. A. G. Boulenger (*Proc. Zool. Soc.* November 1915) furnishes "A List of the Snakes of East Africa, North of the Zambesi and South of the Soudan and Somaliland, and of Nyassaland," and "A List of the Snakes of North-east Africa, from the Tropic to the Soudan and Somaliland, including Socotra," giving keys to the identification of the genera and species. He also adds "Descriptions of a new Amphisbæna and a new Snake discovered by Dr. H. G. F. Spurrell in Southern Colombia."

"Pattern-blending with reference to Obliterative Shading and Concealment of Outline" is the record of a series of laboratory experiments which are compared with actual patterns of animals by Muttram (*Proc. Zool. Soc.* November 1915). In the same place is a statistical inquiry "On the

Distribution of Secondary Sexual Characters amongst Birds, with relation to their Liability to the Attack of Enemies."

Roberts has been able to breed and rear the young of the Tasmanian Devil (*Sarcophilus harrisi*) in captivity (*ibid.* October 1915). Eastman has traced the "Early Portrayals of the Opossum" (*Amer. Nat.* October 1915) and finds the earliest reference as far back as 1504.

Thomas continues his output of papers on the smaller mammals: Bats are dealt with in "A Special Genus for the Himalayan Bat known as *Murina grisea*," "A new Genus of Phyllostome Bats and a new Rhipidomys from Ecuador" (both from *Ann. and Mag. Nat. Hist.* October 1915) and "A new Bat from Northern Nigeria" (*ibid.* November 1915); Rats in "Further Notes on Asiatic Bamboo Rats" (*ibid.* October 1915). In continuation of his previous notes Dollman writes "On the African Shrews belonging to the Genus *Crocidura*" (*ibid.* October 1915). Notes "On some of the External Characters of the Genus *Linsang*, with notes upon the Genera *Poiana* and *Eupheres*" and "On some External Characters of *Galidia*, *Galidictis* and related Genera" are given by Pocock (*Ann. and Mag. Nat. Hist.* October 1915).

In "On Specimens of Cuvier's Whale (*Ziphius cavirostris*) from the Irish Coast" (*Proc. Zool. Soc.* October 1915) Harmer supplies evidence of two individuals of this species being stranded in just over two years, so showing it may undoubtedly be claimed as an inhabitant of British seas. Kloss describes a collection of over 500 mammals he made on the coast and islands of South-east Siam (*ibid.* November 1915). One species and twenty-two sub-species were described as new. "New Genera and Species of Mammals from the Miocene Deposits of Beluchistan" are made the subject of a preliminary notice by Forster-Cooper (*Ann. and Mag. Nat. Hist.* November 1915).

General.—In the *American Naturalist* for the period under review there are a number of papers dealing with various experiments in breeding and inheritance.

Pearl describes "Seventeen Years' Selection of a Character showing Sex-linked Mendelian Inheritance" (*Amer. Nat.* October 1915) and concludes that selection can alter the composition of the population with respect to genetic determiners by a process of sorting over what is already there and rejecting

some. In "Specific and Varietal Characters in Annual Sunflowers" (*ibid.* October 1915) Cockerell points out that variations arise quite continuously. These new variations repeat themselves in various species, indicating that they represent common deep-seated tendencies. The work on "Inheritance of Doubleness in Matthiola and Petunia" (*ibid.* October) by Frost extends that done by Saunders (cf. Bateson). "Variability and Amphimixis" were studied by Walton (*ibid.* November), who indicates (1) that variability is greater in small and isolated populations; (2) progressive evolution results from factors arising through cumulations; (3) characters once thus formed produce by fluctuations, etc., the diversity of organic life. Mice furnish subjects of a paper on "Genetic Studies of Several Geographic Races of Californian Deer Mice," by Sumner (*ibid.* November). "On Hooded Pattern of Rats," by Castle (*ibid.* December), is used also to reply to Pearl's paper given above, and it is pointed out that "sorting over what is already there" implies that there is no change in the germ plasm, an assumption for which there is no evidence. "Inheritance of black-eyed white spotting in Mice" is treated by Little, while Laughlin gives "A Description of Mechanical Charts for illustrating Mendelian Heredity in each of three well-known cases of blending Inheritance in the 1st hybrid generation."

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc., Public Museum, Gloucester.

THE *Journal of the Royal Anthropological Institute* for the first half of 1915 (vol. xlv.) has now been received. The first article is the presidential address of Prof. Arthur Keith, and this deals with "The Bronze Age Invaders of Britain." Prof. Keith reaches no very definite conclusions with regard to these people. As is well known, the Bronze Age Race, or as it is otherwise called the "Round-Barrow Race," was markedly different from the Long-Barrow Race which inhabited Britain during the greater part of the Neolithic Age. The Long-Barrow people were dolichocephalic and were very small in stature; the Bronze Age people were brachycephalic and were large and powerfully built, especially in the case of the women. They are also believed to have been blond, and

perhaps the most important point insisted upon by Prof. Keith is the distinction between this Bronze Age race and the "Alpine" race of west-central Europe, which is also brachycephalic, but is brunet and short. The Round-Barrow people (who reached this country shortly before the end of the Neolithic Period) have been identified by most writers with the Goidelic Kelts, and Prof. Keith appears to accept the view that the two round-headed types were probably two branches of the Kelts, and he places the original home of the British Round Heads in the mountainous region of Central Europe. The Teutons belonged to a different type—the tall blond long-headed type—and it is worth noticing that this variety of European is not the dominant type in Germany to-day. Indeed, there is every reason to believe that the English as a whole are more Teutonic in race than the modern Germans as a whole, although in certain parts of Germany—Hanover, Oldenburg, and Holstein—the people are very purely Teutonic, no less so than the Norwegians and Swedes. Prof. Keith digresses into this subject, which is worthy of a good deal of consideration. It has long been notorious that the Prussian kingdom contains a great admixture of German and non-German people, although the common statement that the ancient Prussians were Slavs is an error. The old Prussians were Lithuanians. A large percentage of modern Prussians are brachycephalic. The point is of interest, because the physical characteristics appear to be reflected in some regions of national psychology. It has often occurred to the present writer that in some respects the traditional Germanic mentality has been much better preserved in England than in Germany. The keystone of the ancient German polity was the freeman, or, as we should call him, the elector. Among the ancient Saxons the moot was greater than the king. The aggrandisement of the monarchy in Prussia, with the concomitant subordination of the commonalty, is certainly not a Teutonic trait. Consider, for instance, the franchise for the Prussian Parliament, which is not even intended to be democratic. The polity of such liberty-loving nations as the Dutch, the Danes, the Norwegians, and (in a lesser degree) the Swedes, is much more truly Teutonic. For some unknown reason, Prof. Keith fails to point out the sharp distinction between the two types of long-headed Europeans, the tall

blond long-headed race and the short brunet long-headed (or Iberian) type. Another aspect of these questions is dealt with by H. Peake and E. A. Hooton in a paper entitled "Saxon Graveyard at East Shefford, Berks," in the same number of the *Journal*. The remains of twenty-seven individuals were found. From the differences between the male and female skeletons, the authors conclude that Saxon men had married Romano-British women, and they speculate that this was the general rule throughout the country, but such a wide generalisation founded upon so narrow a basis is not to be accepted. The *Journal* also contains an article on "Stone Implements from South African Gravels" by Major E. R. Collins and Mr. Reginald Smith.

In the *American Anthropologist* for the third quarter of last year (vol. xvii. No. 3) there are some interesting papers. The longest contribution is an article entitled "Notes on the Archeology of Salvador" by H. J. Spinden, of the American Museum of Natural History. This is an admirable and extensive study and should be widely read. Mr. R. B. Bean has a short paper on "The Growth of the Head and Face in American (White), German-American, and Filipino children." Some perfunctory observations are collected together here, but the article is almost worthless, for in view of the mixture of races the terms German-American and White-American are almost meaningless anthropologically. A paper entitled "A Study of Nebraska Crania" is contributed by C. W. M. Poynter of Nebraska University. The skulls in question came from the Nebraska Loess, and whatever may be their antiquity, they closely resemble those of the existing American Indians. The book-reviews are usually excellent in this periodical, and attention must be drawn to W. B. Babcock's long and informing review of William Hovgaard's engrossingly interesting work, *The Voyages of the Norsemen to America*.

In the last number of SCIENCE PROGRESS I referred to a singularly ill-informed article by M. du Caillaud in *Man*, dealing with what he alleged to be the racial identity of the British and French nationalities. In the December number of *Man*, Mr. H. R. Hall replies to the Frenchman's untenable thesis in a manner which is at once crushing and amusing. The November *Man* contains some excellent articles, notably one on "Queensland Stone Implements" by Dr

Ronald Hamlyn-Harris, the energetic director of the Brisbane Museum. The same number contains a report of the meetings of the Anthropology Section of the British Association last September, whereat Prof. Elliot Smith, Sir Arthur Evans, and others discussed the highly topical subject of the distribution of nationalities in the Balkans. They reached no conclusions that were not already known to persons who had studied the questions, but it is worth recording that according to this (possibly over-rated) principle of nationalities, part of what was in September Serbian Macedonia ought to go to Bulgaria, whilst on the other hand Bosnia, Herzegovina, Dalmatia, Croatia, Slavonia, Carniola, together with most of Istria and other minor districts, are overwhelmingly Jugo-Slav. The Italian claims to Dalmatia were dismissed as worthless, but the importance of the differences of religious culture among the Jugo-Slavs themselves does not appear to have been sufficiently emphasised.

Even enthusiasts have now had nearly enough of the discussion on the Piltdown discovery, but a paper by an American scholar must be noticed. This is entitled "The Jaw of the Piltdown Man," and will be found in the *Smithsonian Miscellaneous Collections* (vol. lxv. No. 12), and is by Gerrit S. Miller. The author holds (with Prof. Waterston) that the jaw and skull do not belong to one another, and that the jaw is that of a chimpanzee. The case for the dissociation of the cranium from the mandible and canine tooth could hardly be better stated than it is in this paper, and nobody can reasonably deny that some doubt exists. There is not space to discuss the matter adequately here, but I may mention that Mr. Miller decides to establish a new species of chimpanzee, *Pan vetus*, based on the jaw, and is apparently content to leave the skull with the name *Eoanthropus dawsoni*, although the cranium without the jaw would certainly not be entitled to generic distinction from *Homo*.

CORRESPONDENCE

To the Editor of "Science Progress"

LOGIC AND ILLOGIC

I. From Dr. C. A. Mercier, F.R.C.P., F.R.C.S.

Sir,—There are many inaccuracies in Mr. Winter's letter: I will point out a few. It is inaccurate to say that I assailed Miss Stebbing. She attacked me with some asperity, and I defended myself with the meekness and gentleness that are becoming towards a lady, but that must not be expected by Mr. Winter.

It is inaccurate to say that no form of words is an argument unless it contains a conclusion that is not specifically implied in a premiss. This is exactly the reverse of the truth, and can be made punctually accurate by omitting the word *not*. Deductive argument consists in nothing but discovering other meanings, besides that explicitly stated, that are implied in a proposition. "If you tell us that A is greater than B, you are not giving us any fresh information by saying that B is less than A"—P. This is in one sense true: in another sense false. The second proposition is implied in the first, but is not the same as the first. It compels us to look at the same fact in a different light. It introduces us to a new aspect. It regards the matter from a different point of view. Just so, when we move round a house from the south side to the north, we are looking at the same thing, but we see it from a different aspect, we have seen more of it than we saw when our view was limited to one aspect. This is what deductive reasoning does, and this is all it ever does or can do. It is precisely what is done when "from Some German subjects are Poles, we infer that Some Poles are German subjects"—Q. If Q is an argument, as Mr. Winter says it is, then P is an argument. If P is not an argument, as Mr. Winter says it is not, then Q is not an argument.

"Logic takes no cognisance of the truth or falsehood of a premiss." This is true of deductive Logic only. To inductive Logic the truth of the premiss is vital.

I pass Mr. Winter's examination of my arguments. It does not touch their validity in the least, as any competent reasoner can see for himself. I am indebted to him for one discovery, however. Until I read his rejoinder, I was convinced that professional logicians are the very worst reasoners in the world. I must now revise this opinion, for it is evident that Mr. Winter has very little acquaintance with Logic "as she is taught."

I am, Sir, your obedient servant,

Chas. A. Mercier.

II. Reply: By W. H. Winter, B.A. (Camb.), M.A. (St. Andrews)

Sir,—I am obliged for your courtesy in sending me a copy of Dr. Mercier's reply to my criticism.

A considerable part of it is taken up with representing me as saying precisely the contrary of what I did say. Example Q I gave as an illustration of immediate inference as distinguished from argument. Neither P nor Q is an argument. An argument when stated in its complete form will be found to consist of two premisses and a conclusion. It is true that in practice one premiss is frequently suppressed, especially if it is regarded as a matter of common knowledge. This was the case in Dr. Mercier's horticultural example. The minor premiss, No geranium or viola is an aster, was not expressed, but tacitly taken for granted as a known fact. It is none the less vital to the reasoning, for, if either the geranium or the viola were a species of aster, the conclusion would obviously be invalid. Dr. Mercier quotes my test for an argument that it should contain a conclusion which is not specifically implied in a premiss, and characterises it as the reverse of the truth. Surely this is the limit of absurdity. Unless you gain some fresh point when you argue, what is the use of arguing? If Dr. Mercier really wants to know what an argument is, let him renew his acquaintance with Euclid's Elements. He will there find a book full of arguments from beginning to end, and I believe on investigation he will discover every one of them

to be a syllogism. It is comical beyond words then to assert, as he practically does in his letter to you, that the whole deductive argument consists in nothing but discovering other meanings besides those explicitly stated in the definitions and axioms.

I accept the correction about inductive Logic, but I take it that in the present controversy we have been dealing exclusively with deductive Logic.

Dr. Mercier says that he passes my examination of his arguments. In this he is probably wise, as I am confident that the "competent reasoner" will agree with me that his arguments have been annihilated.

I am, Sir, your obedient servant,

W. H. Winter.

SCIENCE AND MODERN AND ANCIENT LANGUAGES

From Prof. H. A. Strong, M.A., LL.D.

Sir,—Allow me to congratulate you as an old colleague on the letter which you and other men of science have published relative to reforms necessary in our educational system. I am, as a veteran teacher of Classics, no less interested than the distinguished men who have signed that letter, and I venture to ask you to consider my views, which are based upon a wide experience.

(1) It is my opinion that at least 50 per cent. of the students who came under my teaching would have done much more wisely if they had turned their attention to some other subject, as they plainly had no taste for the subject and it could not in any degree serve to develop their intellects.

(2) So far am I from wishing to depreciate the Classics as an object of study that I wish to see a very much higher standard for matriculation and degree introduced into all our universities. The standard for the pass degree is so low that it cannot be a real test of the capacity of really good scholars. The standard for matriculation for medical students was so ridiculously low that I am glad to see that Latin has for this purpose been made an optional subject, and I trust that the standard for those who present Latin as a subject will be considerably raised.

(3) I trust that while science will be taught in all schools, we shall after this war see that some of our larger and more important schools shall make a speciality of this important subject.

(4) I trust also that in any scheme of reform that may be introduced into our schools attention shall be paid to the more thorough instruction of the pupils in modern languages. I have talked with many officers both before the war and since its outbreak, and they are unanimous in lamenting the ignorance of French and German wherewith they left their schools. The boys educated at the German gymnasia and the Belgian athénées know, on leaving their schools, much more English than our boys, on leaving theirs, know of French.

(5) French should be taught in connection with the Latin of which it is a daughter. This is rarely done in England.

(6) The masters in our schools should insist that science and modern languages should be regarded as equally important as classical teaching and the rewards for proficiency gained in their study should be as great.

(7) The most practical way to ensure that science and modern languages shall be properly taught in our schools is by endeavouring to procure the foundation of as many scholarships and prizes at the universities for these subjects. The colleges might aid in this, and wealthy benefactors might be invited to found scholarships for these purposes. Such scholarships should be likewise attached to schools, and the headmasters of secondary and public schools should more frequently be chosen from the ranks of scientific teachers.

(8) More importance ought to be paid in the selection of candidates for the Indian Civil and Home Civil Service to proficiency in science and modern languages.

(9) In the education of women there is room for improvement which might be effected by studies enabling them to concentrate their thoughts and enable them to form proper judgments. Mathematics should be carefully taught in their schools, and where possible some amount of Logic. Much more attention should be paid than at present to the training of women who wish to learn business methods, so that they may be able to aid in the management of business houses, as their French neighbours do.

Of course all these remarks might be indefinitely expanded,

but I have set down briefly the main improvements which suggest themselves as desirable in the reform of our educational system, from the point of view of a teacher who desires that the ancient classics shall be taught more efficiently than ever, but only to those fitted to receive their teaching. And I may add as one who has felt most bitterly the lack of instruction in any branch of science, that it is my sincere desire that the absolute and immediate necessity for the teaching of science may be recognised by the parents of the pupils at our schools and colleges; if this necessity is once recognised the school authorities will promptly accept the wishes of the public

I am, Sir, yours very faithfully,

Herbert A. Strong, M.A., LL.D.,

Emeritus Professor of Latin, University, Liverpool.

BACTERISED PEAT

I. From Sir Sydney Olivier, K.C.M.G., B.A., LL.D.

Sir,—We are a little perplexed by the reference in the current issue of Science Progress to the relations of the Board of Agriculture and Fisheries with Prof. Bottomley, which appears to us quite inappropriate to the facts of the case; and as misapprehension on this subject is somewhat general, I should like to give you the facts.

For the last two years we have been giving King's College a grant in support of Prof. Bottomley's investigations on bacterised peat, and have been following his experiments with great interest. Facilities for some of these experiments were also granted at Kew, which, as you know, is under the Board's control. In the autumn of last year Prof. Bottomley, in a public lecture, announced that the Board had refused to take advantage of his offer to waive his patent rights and this statement formed the subject of comment in the Press at the time. As a matter of fact, no formal offer had been made to the Board nor, if it had been made, could we have accepted it, in view of Prof. Bottomley's admission that no experiments on a commercial scale had been carried out successfully—and, I may add, of the doubts that many of his scientific colleagues still entertain as to the value of his discovery. We have now made arrangements with Dr. Russell of Rothamsted to conduct a

careful investigation on the whole subject, and if his report is favourable we shall then consider whether any use can be made of Prof. Bottomley's offer.

I think you will agree that it would be impracticable for a Government Department to encourage the sale of a substance as to the merits of which considerable doubt existed: that there is still room for doubt whether bacterised peat can be used agriculturally on a commercial scale is sufficiently evident from the report on the experiments at Wisley which appears in the current number of the Journal of the Royal Horticultural Society.

Yours very truly,
SYDNEY OLIVIER.

BOARD OF AGRICULTURE AND FISHERIES,
4, WHITEHALL PLACE,
January 31, 1916.

II. FROM PROF. T. B. WOOD AND PROF. R. H. BIFFEN

SIR,—In the last issue of SCIENCE PROGRESS, No. 39, January 1916, page 489, under the heading of "Economy," the writer of "Notes," referring to Prof. Bottomley's bacterised peat, states that its discovery "has made it possible to double the production of our food supply," and then proceeds to criticise the Board of Agriculture for not "availing itself of this well-timed boon." Any discovery which is capable of doubling the production of food deserves serious consideration at the present time, and we therefore hope that you will insert in SCIENCE PROGRESS the following remarks on the possibility that such an achievement has been realised.

In claiming for bacterised peat that it is capable of doubling the production of food, we take it that the author of "Notes" means that its use will double the yield per acre of the staple crops of the farm. Yet we can only find the scantiest evidence to indicate that any attempt has been made to find out whether bacterised peat is of any practical use under farming conditions. This evidence is contained in Chapter XIV of *The Spirit of the Soil* (by G. D. Knox, published by Constable), and, lest any doubt should be thrown on its accuracy, it should be noted that this is vouched for in the foreword written by Prof. Bottomley himself.

Turning now to this evidence, we find that bacterised peat

has been tried on four important farm crops ; barley, potatoes, mangolds, and wheat. The trials on barley are described as "very promising." They consisted of laboratory tests only, and the facts emphasised are that the treated plants were more deeply coloured, and tillered more freely, than the controls. If these results hold in the field, the use of bacterised peat may confidently be expected to ruin the barley crop, for high nitrogen content as indicated by the deep green colour, and the uneven ripening caused by abundant tillering, are the last features a practical man wishes to see in his barley.

The experiments with potatoes have the advantage that they were made in the open field, but on land which had not been under cultivation for nine years. The yields from plots treated with bacterised peat are compared with those from plots treated with farmyard manure and with artificial manures, and with the yield from the untreated soil. Bacterised peat is credited with having produced large increases, but it is quite impossible to form any judgment of the value of the results, as all details as to size or number of plots, or quantities of manures, are ignored.

In an appendix, however, a more detailed account is given, apparently of this experiment, from which it appears that the results recorded were obtained by weighing the produce of single plots each of about $\frac{1}{160}$ acre. The dressing of bacterised peat was 9 oz. per square yard, which is approximately $1\frac{1}{4}$ tons per acre, the cost of which is consequently about £12 per acre.

The appendix also records under the heading "Results of which less exact details are available" that the produce of a row of mangolds 9 yards long was increased by 50 per cent. by treatment with an unspecified amount of bacterised peat.

The trials with wheat are still more inconclusive. They were made on market garden soil, obviously unsuited for the purpose, for even the control plots were so badly "lodged" that harvesting was impossible. The one fact they appear to establish is that the use of bacterised peat resulted in a highly undesirable increase of growth in straw and foliage.

Further study of this chapter and the appendix leaves little doubt that if more conclusive evidence of the benefits which bacterised peat can confer on the farmer had been available it would not have been omitted. Yet on this inconceivably flimsy foundation, statements are made that our food supply

can be doubled by the use of bacterised peat! We may be pardoned for expressing our doubts whether such statements are justified, especially in view of the fact that in trials carried out near Cambridge with mangolds, wheat, and barley, bacterised peat has failed to produce any increase whatever in the yield.

Further, it must be pointed out that the farmer farms for profit, and that increased yields may be purchased at a cost which may be altogether prohibitive. The price of bacterised peat appears to be about £10 per ton. It has given its best results in pots when mixed with the soil at the rate of 10 per cent. An acre of soil 9 inches deep weighs over 1,000 tons. To reproduce on the farm the conditions which have proved successful in pots would therefore cost about £1,000 per acre. To add even so small a proportion as 1 per cent. to the soil would cost £100 per acre. The value of a good crop of wheat is about £10 per acre in normal times, at present it is seldom more than £15.

But the farmer is advised to use only 5 to 10 cwt. per acre, *i.e.* 0·025 to 0·05 per cent. of the weight of his top soil, at the cost of £2 10*s*. to £5 per acre, without a shred of evidence to show that such proportions may be expected to produce in his fields results comparable with those obtained by the use of 10 per cent. in a flower-pot.

In view of these considerations we submit that the strictures on the Board of Agriculture indulged in by the writer of "Notes" are quite uncalled for, especially as the Board has "availed itself of the well-timed boon" to the extent of arranging for a complete official examination of bacterised peat and its effects on farm crops at the Rothamsted Experimental Station.

Finally there is another side to the question to which we cannot refrain from referring. The popular press continually reminds us that the farmer is the least progressive of mortals and the last to avail himself of the results of modern research. This imputation is infinitely less true than most people realise. In many cases the farmer's caution—for it does not amount to more than that—is justified by his experience. He has in the past been all too willing to accept statements made in a form which appeals to him as "scientific," and his faith has too often been badly abused. Many farmers of our acquaintance, for example, expended considerable sums on nitrobacterine, a former much-advertised "discovery" made by Prof. Bottomley.

They now heartily endorse the statement on page 156 of *The Spirit of the Soil* that "from the standpoint of the practical man nitrobacterine was a failure." Such experiences are not readily forgotten. Every repetition of them makes the farmer more and more reluctant to adopt the results of scientific research. This in itself is a severe handicap to the efforts now being made by a considerable body of scientific men throughout the country to improve the position of British agriculture, for the farmer does not possess the means of distinguishing between results which have been thoroughly established by conscientious trials and much-advertised "discoveries" whose practical bearing on crop production rests on no surer foundation than a few haphazard trials carried out under unsuitable conditions by experimenters who do not possess sufficient acquaintance with farming to enable them to interpret the meaning of their own results. For this reason we feel compelled to protest when a periodical of the standing of SCIENCE PROGRESS lends itself to the publication of a statement that an untried discovery has made it possible to double the production of food in the country.

T. B. WOOD.
R. H. BIFFEN.

SCHOOL OF AGRICULTURE, CAMBRIDGE,
February 28, 1916.

P.S. (*March* 8).—The latest report of the Field Trials of the Midland Agricultural and Dairy Institute shows that dressings of 7 cwt. per acre applied to wheat and seeds hay "produced no result in either crop." In a trial with potatoes the results were:

	tons.	cwts.	lbs.
No artificial manure	8	4	32
" " " + "humogen" . .	7	13	64
Standard artificials	12	17	16

T. B. W. AND R. H. B.

*** The Note referred to was inserted during the Editor's absence, the statements in it being based entirely upon statements made at a lecture delivered at the Royal Botanic Society on October 18, 1915, by Prof. W. B. Bottomley, as reported in the *Times* on the following day.—*Secretary*.

NOTES

Great Science and Little Science

We men of science are apt to complain of want of recognition of our work; but, to be frank, most of our work is really of little value. Amongst the enormous output of scientific literature, by far the largest proportion of the papers are of a petty nature—records of hypotheses, records of a few haphazard observations, inconclusive, mere lumber to be thrown into the dusty attics of knowledge. Such works bear to the great works of science the same relation that the artist's pot-boiler bears to the classical picture. Do not let our hearts be too uplifted with pride in stuff of this type. We think that when we have published our records we have given the world something to remember. But the world, like the individual, cannot remember every trifling occurrence; and the trifles of science are like the trifles of our daily life—to be forgotten to-morrow. When the man comes who shall finally cultivate the whole field in which we have been picking daisies, he will probably do so without troubling even to consider our little excursions. The great work by Major MacMahon so ably reviewed by "C." in this number should chasten our pride. It is a monument of science which required thirty years in the making. After all, what is the difference between high science and low science? The one is the solution of difficult problems, the other the record of isolated observations. The latter is sometimes useful indeed—sometimes even leads to great discoveries; but, successful or not, it does not merit the honour which we give to the former. Here we have a work which is really an event in human history, if men were only intelligent enough to recognise it. Great problems once solved are always solved; and the solutions are milestones in the life-story of the race.

Brainless Britain

Much agitation has been produced among men of science and learning in general by the proposal of Government to close

the museums in order to effect war economies. Quite possibly there may be reasons for this action which are more cogent than the ones which have been given to the public; and we fully recognise that if this is the case the museums should be closed. But the impression left by the controversies on the subject which have appeared in the press suggest merely that in closing the museums our political rulers are following their usual course of penalising the higher branches of human effort because those who are engaged upon such work are few and politically powerless. On February 10 a deputation from the National Art Collections Fund, the Museums Association, the Royal Asiatic Society, the Hellenic Society, the Art Workers' Guild, and the Imperial Arts League waited on the Prime Minister with a strong protest against the closing of the museums; but Mr. Asquith gave only two small concessions in reply. He stated that the closing was necessary on grounds of economy and added that the Government would shortly produce a list of the economies which they hoped shortly to effect. Mr. Asquith demurred to the suggestion that when the Government had to look about for economies they selected the museums as the first victims. Nothing was further from the truth. In fact, the Government had not accepted the whole proposal of a Committee appointed to make recommendations upon the subject and had decided to keep open the National Gallery and the Victoria and Albert Museum. Mr. Asquith was also good enough to declare himself in favour of keeping open that part of the Natural History Museum which is popular—the parts in which exhibits of animals and birds were kept, and which were very much resorted to by colonial visitors and wounded and convalescent soldiers and sailors! What a characteristic decision! The parts of the museums which are useful for the higher needs of science and art are to be closed, but those parts which amuse the public for an idle half-hour are to be kept open. The fact is that science and art are of no value to our politicians because they do not provide votes. Nothing is to be done for the intellectual side of life, everything for the non-intellectual. We see here merely the unconscious cerebration of the politician. He knows that if the mind of the mob were to be raised to a higher standard he and his meretricious vocation would cease.

The Punishment of Science

Nothing is more valuable to the State than long-continued work in out-of-the-way tracts of nature, but nothing is more injurious to the individual who undertakes it. He is often obliged to devote years of his life to labours which, even if they succeed, bring him no profit whatever, while he is thus losing time which might be spent much more profitably for himself. It is indeed this fact which demonstrates how very inefficient British methods for encouraging science really are—since the State makes no effort whatever to find a remedy for this principal evil. What are we to think of the following case? One of the most meritorious of such workers in Britain—a man who has been toiling for years in a tract which will yield no profit to himself though much profit to science in general—recently sent us a valuable paper. We noticed that his name was not followed by the usual letters, and wrote to inquire why. His answer was that he could not afford to subscribe to the learned societies, the membership of which was denoted by those letters! Can anything more shameful be imagined? In most civilised countries the State, recognising the value of learned societies, gives them such endowment as frees them from demanding subscriptions from their members; and indeed in some states members of academies are actually paid—and rightly so, because men of exceptional learning are or should be of value to the whole nation. But in Britain (if we may venture to call ourselves civilised) this point is not recognised. Our "statesmen" and Members of Parliament, whose services to the State are often supposed to be almost nil, extract a quarter of a million pounds a year out of the Exchequer, but the British Empire cannot afford to pay a few pounds a year in order to relieve learned societies from taxing the workers. The fault is a gross and inexcusable one and is a further proof that the average intellect of the mass of the British public of to-day, including that of their rulers, is very much below par.

The Progress of the War

The war continues its predestined course, and reminds one of a game of chess between a Professional Player on the one side, and an Old Lady after dinner with port wine on the other

side. The former evidently knows all the gambits and has calculated beforehand the effect of every move upon the next dozen moves; while the latter is anxiously and excitedly concerned in taking the first pawn of her adversary which she thinks has incautiously exposed itself to capture. Fortunately war is a longer game than chess—long enough, in fact, often to enable non-expert players to become expert before it is finished; and even the Old Lady who appears to be conducting the strategy on the one side of the war may learn something about the game before it is finished. Up to the present the partial success of the Professional Player appears inexplicable to the ignorant onlookers, but it has been really due to the simple commonsense understanding of the fact that difficult undertakings are not successfully carried out unless they are preceded by a long period of careful forethought and organisation. On the other hand, the ignorant attribute the failures of the non-expert player merely to her misfortunes. The world, however, is beginning to see through this plea, and to ask whether the Amiable Party ever really knew anything of the game before she started to play the Professional. She has hitherto lived all her life in the rosy light of comfort (and port wine); has believed in liberty, virtue, and kindness; has disbelieved in such wicked things as calculation and science; thinks that all forethought is a kind of evil plotting; and is much dismayed when she suddenly loses a bishop or a castle. Now war is a very difficult science. There is, in fact, an old military saying that an army of asses led by a lion will defeat an army of lions led by an ass. To which side of the war each of these metaphors will apply we cannot, or are unwilling, to say. Fortunately, the parable breaks down at this point. The Wicked Professional has made some monstrous blunders in spite of his expertness, and the Old Lady has many strong sons who will see her " through the game."

Experts and Official Experts

Those who have the misfortune to spend their lives in toiling for humanity in subterranean burrowings into nature are often surprised and perhaps pained to see suddenly in some newspaper that Government has appointed as its expert Mr. So-and-So, " a distinguished authority " upon the very subject

in connection with which the burrower has been working. Such appointments are of course often quite good—but at times grotesque. The gentleman who has had the good fortune to be declared an expert by Government often turns out to be a much greater expert on other matters, such as after-dinner speaking, haberdashery, waistcoats, and the general art of getting on in the world. Our rulers, who are themselves distinguished professors in these latter lines, seem to have very curious opinions about what constitutes expertness. We once heard a great statesman, who is also a philosopher, inveighing for half an hour against experts in general. On other occasions we heard no less than two Secretaries of State and also, we regret to say, a President of the Royal Society, talking with enthusiasm about the great discoveries in an important field of science of a man who as a matter of fact had never done any researches on the subject at all and had never even pretended to have done any but had merely written up the researches and thoughts of others in a pretty picture-book. On yet another occasion, when our worthy Government did at last make up its mind to spend money on certain investigations and had determined to appoint a paid committee for superintending them, the world was astonished to find among the members of this Committee the names of gentlemen who had done no work of any importance in the special line concerned. On inquiring how these gentlemen came to be appointed, we were informed that an official letter had been sent to the principal learned societies with a request that they should nominate members for the Committee. Now the writer of this note was at the time on the Council of one of these learned societies, but he heard nothing whatever of this circular letter; and it transpired that the nominations had been made by the officials of the Society without reference to the Council. That is the way things are done; and we as a nation still hope to continue to predominate in the struggle for existence between the crowded peoples of the earth. Now we know that Government really has some very able experts at its disposal, and we do not wish to disparage them; but in this matter, as in the whole matter of public appointments, there is undoubtedly much jobbery in this country. We doubt whether there is much direct bribery going on in British administration, but nepotism and a curious way of doing things

by the private nudgings and winkings of members of Boards and Committees are rife. That is how the expert with the wonderful collars and sleeve-links comes into the expert's office—and it may be imagined what is the result of his advice in connection with highly technical difficulties. Was it for this reason that our all-highest politicians have neglected to provide sufficient defence against aerial attacks, or pooh-poohed submarines, or ridiculed machine guns, or thought that our army was going to fight the Germans without shells? Probably so; because for years past we have made Ignorance a god and Pretence a demi-god. But war is a terrible revealer, and we are now finding the clay feet of the one and the sawdust anatomy of the other.

The Incompetence of Novelists, Poets, Philosophers, and Theologians

We have another criticism to make of British modes of thought. The *Times* in its issue of February 17, in rightly condemning the methods of Government on the question of official publicity in connection with the war, remarked, in connection with the organisation for this purpose, that "The Government have unfortunately regarded it rather as a means of sustenance for out-of-work politicians than a definite weapon in the national armoury. Endless pains have been devoted to propagating the fatuous reflections of popular novelists. The poets, the philosophers, the theologians have all been enlisted in the service. There have been any number of mysterious missions and private political intrigues (disguised as 'secret service'), but there has been no real attempt to organise and co-ordinate the frank distribution of honest news to the people who want it and in the form in which they want it." What a curious fancy this is of ox-brained John Bull, that achievement in any walk of life incapacitates for achievement in the fields of government. We wonder that the *Times* did not add men of science, men of business, men of commerce, travellers, inventors, and such like despicable people to its list. And indeed it may be said that few men of any real distinction in any walk of life have ever belonged to British Governments or have even sat in Parliament—except perhaps lawyers. We can recall for recent years one serious philosopher and one literary critic. Who are the people then who govern us?

To be perfectly frank, none but the Political Adventurer and his advertiser the Daily Journalist. The amazing stupidity of the British public regarding the people whom it elects to govern it is perhaps the most remarkable fact in the modern history of Britain. Our opinion is diametrically the opposite of that of the *Times*. We think that if by some such system as that of the Proportional Representation Society Parliament and Government were to be filled, not by those who have never done anything in the world—the professional talkers, the men who are out for getting on in the world, the Men of Principle, the Cuffs-and-Collars Men, the Sniffers, and the Younger Sons—but by those who have previously demonstrated their ability by good work actually done, the State would no longer be afflicted by such obtuseness, want of forethought, ignorance of administration, and indifference to all the highest interests of life as have been exhibited in the management of it for many years past. The world would be better governed by those who have a reputation to lose than by those who have a reputation to make. Why a popular novelist, a poet, a philosopher, or a theologian should not be able to achieve work of any kind as well as the demagogues of the hustings or the anonymous compilers of dignified logomachy in the press it is impossible to understand. When one looks round at the Governments, not only of Britain but of the Colonies, one asks what on earth have these men ever done to justify the selection of them for their posts—the chief Departments of State managed by persons who do not possess a grain of knowledge upon the subjects which the Departments have to deal with, and our Colonies ruled by the poorer scions of our nobility. Behind it all the incessant, garrulous, and cacophonous frog-chorus of the political fen of Journalism!

Party Impolitics

We are asked why so many journals, including SCIENCE PROGRESS, are at present attacking party politics, and, if party politics are abolished, what form of government we should propose. This reminds one of the story of the man who when a large tumour was removed from him complained that he did not recognise himself; and our questioners evidently confuse the patient with the disease. Party politics do not constitute

government by the people, but are a disease of government by the people. When the French poets and philosophers inspired the first and better part of the French Revolution and formally established the fundamental principles of government by the people, their chief argument was that, whereas despotism often placed noncompetent princes and their favourites at the head of affairs, government by the people should result in government by the best people. The principal complaint against party politics is that they have destroyed this ideal by substituting an oligarchic wire-pulled machine for government by the best people. Britain is ruled to-day by carefully organised, subsidised, paid, and, we believe, wholly vicious party caucuses which return to Parliament persons of their own order, and thus exclude the best brains in the country. Our argument is that a man who consents to belong to a political party thereby abdicates his right of free judgment, and at the same time shows that he is not intellectually or morally a man fit to rule a great Empire. This may seem to be a severe criticism, but careful reflection will prove the justice of it ; and we propose to return to the subject at greater length another time. In answer to the second question mentioned above, namely what do we suggest in place of party government, we say that this question will answer itself on removal of the disease—the first thing to do is to excise the monstrous tumour. All party organisations whatsoever, all subscriptions to party funds, all personal canvassing for votes, all election expenses, and indeed all legalised explicit division into parties should be prohibited by law, just as bribery is now prohibited ; and a great reduction in the number of members of both Houses would automatically raise the standard of ability in them by excluding many of the local wire-pullers and other unsuitable persons who now find sufficient numbers of dull voters to elect or allow them to manage affairs which are evidently beyond their ability to deal with.

Retaliation

We must really object in the interests of the nation to the monstrous namby-pambyism prevalent in this country. On January 18 Sir Arthur Conan Doyle wrote a letter to the *Times* in which he argued that, if we could not otherwise check the destruction of our civil population by German air-raids, we should retaliate in kind in Germany. Immediately a number of persons raised an outcry that retaliation is wicked—just as similar people argue that experiments on animals

and vaccination are immoral. Now what is the truth—as any man of science capable of considering a problem in its entirety will easily see? Suppose that A and B are engaged in a contest, either in sport or in a duel, and suppose that before the contest they had agreed upon certain rules which should be maintained during the contest. If A infringes one of these rules, as, for instance, by drawing a pistol when it was arranged that the contest should be one of fists only, what is B then entitled to do? If the "ring" is kept by onlookers who are capable of enforcing the original conditions, B will at once appeal to them, and if they think that A broke the convention they will punish him and B will be considered the victor.

Suppose, however, that there is no one to keep the ring (and the neutral nations have signally failed to do so in the present war), then if A breaks the rules B has the right of retaliation given to him from immemorial times, and by the universal consent of humanity. That is, he may employ against A, without blame, exactly the same trick as that which A attempted against him. He, too, may draw his revolver or strike under the belt; and the guilt will rest, not in any way upon the retaliator, but upon that one of the combatants who first infringes the pre-ordained rules of the combat. And this is, without a shadow of doubt, a just law. For what else is B to do? If he does not retaliate, A will simply repeat the same foul stroke, and in a serious combat B may thus lose the fight altogether. Of course, it does not follow that B is always obliged to retaliate in kind. We simply state that he has the right to do so. Were it otherwise the villain would be rewarded for his villainy, and the magnanimous would perish. It is a law of nature—and a just law. Those who do not possess sufficient imagination or capacity for reasoning to see this clear truth, and yet who write to the papers endeavouring to tie the hands of the authorities in a war of life and death, are, in our opinion, worthy of censure. Whether it is good strategy or not to retaliate is another question which must be decided by the authorities themselves; but about the right to retaliate against an enemy which has infringed every rule of honourable combat there is no question at all. The whole law of punishment for crime and misdemeanour depends upon the *Lex Talionis*: and one might as well argue that the State has no right to hang a murderer or to imprison a thief.

The Neglect of Science

On February 2 a very important Memorandum appeared in the press, signed by a number of leading men of science and educationists, including Sir Clifford Allbutt, the Earl of Berkeley, Sir William Crookes, Sir E. Ray Lankester, Sir Henry Morris, Sir William Osler, Sir Charles Parsons, Sir William Ramsay, Lord Raleigh, Sir Ronald Ross, Dr. Shipley, Sir Edward Thorpe, Sir William Tilden, and Sir John Williams. The Memorandum complained of "a lack of knowledge on the part of our legislators and administrative officials of what is called 'science' or 'physical science.' . . . Not only are our highest Ministers of State ignorant of science, but the same defect runs through almost all the Public Departments of the Civil Service. It is nearly universal in the House of Commons, and is shared by the general public, including a large proportion of those engaged in industrial and commercial enterprise." The Memorandum cites cases in which this ignorance has been harmful to the nation during the course of the war, and attributes it to the "vested interests" in education which tend to oppose the teaching of science in the schools and universities. For example, "At Cambridge but four colleges are presided over by men of scientific training; at Oxford not

one. Of the thirty-five largest and best-known public schools, thirty-four have classical men as headmasters. Science holds no place in the list. The examinations for entrance into Oxford or Cambridge, and for appointments into the Civil Service and the Army, are among the greatest determining factors in settling the kind of education given at our public schools. . . . For entrance into Woolwich, Science has within the last few years been made compulsory, but for Sandhurst it still remains optional. This college is probably the only military institution in Europe where science is not included in the curriculum. The result of this system . . . is a neglect of the study of the natural sciences, and to some extent an indifferent, not to say contemptuous, attitude towards them." The Memorandum ends by urging a reform in this matter "which is vital to the continued existence of this country as a great power," and is supported by a strong memorial from the Imperial College of Science and Technology.

We regret that we have no space in this number of SCIENCE PROGRESS to deal with this all-important matter, but hope to do so in the next number. In the meantime we should add that the Educational and the Science-and-State Committees of the British Science Guild are combining to take action with regard to the whole subject. The feeling in the country on this matter is undoubtedly strong, and the specialised press has not been silent. For example, the *Financial News* for February 3 says that "every man who wants to see his country great, progressive, and prosperous . . . should back the scientists with every ounce of energy he possesses. If, otherwise, he wishes to see her mean, petty, retrogressive, squalid, and contemptible, let him support a return to our debasing party strifes, with their concomitant triumph of the political schemer and all the host of parasites whom he enriches out of public money." We have ventured sometimes to criticise our party politics; but this extract will show that we do not stand alone in this criticism. And our contemporary, *Nature*, continues its series of powerful leading articles on matters connected with science and the State—articles with which we are always in strong agreement.

The Memorandum referred to above, and a considerable correspondence connected with it, are published together in the Educational Supplement of the *Times* of March 7, 1916, together with excellent leading articles precising the points at issue. It looks as if the grammarians are getting the worst of it, and indeed we are glad to know that several Headmasters, such as Mr. Nowell Smith of Sherbourne, are more or less in favour of the proposals to introduce more science into British education. We invite the reader's attention to the excellent letter by Prof. Strong in this number of SCIENCE PROGRESS, with its brief but comprehensive list of much-needed educational reforms.

The British Science Guild

The British Science Guild is constantly increasing its activities on behalf of science and men of science. We may note that since September last it issues a publication called *The Journal of the British Science Guild*, detailing some of its activities. The Preface furnishes a succinct summary of the objects of the Guild, which may be put into the two words Organisation of Science. The Preface adds: "The immediate need of the British Empire is the organisation of its vast resources and their scientific application to the colossal struggle in which we are engaged. To these ends we must for the time being apply all our energies. But we must not lose sight of the future." The first number of the journal contains the proceedings at the ninth annual meeting, the President (the Rt. Hon. Sir William

Mather, P C., LL.D.) in the chair. At this meeting Sir William Ramsay gave a very important address, which we referred to in our issue of October 1915. The second number of the journal contains an important letter by the President to the Prime Minister; the Report of the Microscope Committee, which recently met (and contained members of the Guild, of the Royal Microscopical Society, medical men, and the leading microscope manufacturers); a list of universities, education committees, and schools which support the proposal to use only British-made chemical glass apparatus for a period of three years after the war; a paper by Dr. Howard S. Wilson on "Organisation and Education," and one by Prof. R. A. Gregory on the "Introduction of the Metric System"; with an Obituary of the late Prof. Meldola, whose death all will deplore.

We are convinced that it is the duty of every man of science to belong to the Guild. Unless the Guild has the full weight of men of science behind it in this country it cannot exercise the full influence in the councils of the nation which science and the workers upon science require. More than this, we are prepared to say from our knowledge of the working of the Guild that it is by no means one of those bodies which stagnates in the history of its past, but that it has before it the whole interests of science and of those who endeavour to advance science. The thoroughness of its working will be understood from the fact that it possesses no less than nine committees in addition to its Executive Committee.

The International Defence League

Mr. Maclagan, the Founder of this League, gave three lectures on January 20, 27, and February 3, at the Queen's Hall, London, in order to create a public opinion in favour of his scheme for the future elimination of war. Before he began to expatiate on this scheme, however, he made his audience clearly understand that neither he nor any member of the League held pacifist views, but that all were entirely and whole-heartedly with our own Government and with the Allies in their determination to fight this fight to a finish.

The scheme he now lays before the world is based on universal co-operation, the initial difficulty consisting in the formation of a plan of action to which all nations will agree. The reason for the failure of all other ideas hitherto mooted, Mr. Maclagan declares to be the absence of all effort to discover the fundamental interest common to all nations. This common idea, he says, is *Defence*. What nation in the whole world from the most civilised to the most savage does not desire primarily to defend itself from aggression? That this is fact and not surmise is proven by the presence in our midst of Civil Law. If to-day a private person attacks another private person with the intent to destroy life he is summarily dealt with by the law; and so powerful is the mere idea of its presence that there are few persons now who dare to risk its enforcement. And yet this idea, which has proved so beneficent when applied within the boundaries of any country, remains in abeyance when nation treats with nation. This League, therefore, now proposes to extend the idea of civil order to embrace international peace. Treaties such as the Hague Convention have proved useless for the simple reason that their clauses were based on the legality of war. War being permissible, it must be waged on such-and-such lines; but if an International Council were formed on the understanding that war under *no* circumstances is permissible, something might be done. A Council composed of representatives from all nations is to be formed for the one and only purpose of preventing war. All armies, for the immediate future at any rate, are to be kept up to their present working efficiency; and

because all nations have agreed that defence is an essential element of their welfare, any nation that attacked another would have to meet the combined forces of the world solely on the ground that the aggressive nation had attacked, and quite apart from her motive in doing so Then, when the army of the recalcitrant nation had been driven back within its own borders, a complete blockade would be put into operation, only to be raised when a suitable indemnity had been paid. Nations, of course, would still have disputes, which they could bring before the Council for a decision; but, if the decision is not accepted, it would not be enforced by the Council. But Mr. Maclagan prophesies that the nations would gradually come to look on this Council as the highest and best authority, and to abide by its decrees; and, in any case, no matter what quarrelling might ensue, and no matter how long it would take for the world to find a satisfactory means of settling its differences, surely anything is better than the insensate slaughter of millions of human lives. This method would do away with the fetish of the Balance of Power, which has always been the chief factor in the precipitation of war, and also abolish the necessity of neutrality which this present conflict is proving to be untenable.

The next question raised is ot what elements shall the Council be composed, which is simply answered by the proposal that each nation is to be represented on the basis of her exports and imports. As nation only comes into contact with nation by means of commerce, the measure of the exports and imports would be the measure of her interest with the outside world, and the measure of her title to representation on an International Council. The larger the exports and imports the greater the number of representatives—an elastic representation which would expand or shrink automatically with the increasing or decreasing prosperity and importance of any one country.

According to the lecturer this scheme not only solves the problem of war in Europe and America but solves also the problem of the Yellow Peril, which, if unchecked, will assume greater proportions in the near future than it has done in the past, owing to the fact that the East is arming itself on the European plan—the very plan that has produced the European disaster. Once get the Eastern nations to recognise that this International Council defends them as well as Europe, and they would also subscribe to its enactments.

One possible objection might be put forward—the objection that by this mode of procedure the present map of the world is thereby stereotyped, and that frontier expansion would become impossible. But if war becomes a thing of the past Mr. Maclagan doubts that frontier expansion would be considered a necessity. And if it be temporarily considered a necessity, human intellect is not so barren that it cannot eventually hammer out a plan whereby each race shall satisfactorily amalgamate and be autonomous.

The British Constitution Association

Prof. W. M. Flinders Petrie, F.R.S., President of this Association, was in the chair on February 22 at one of its meetings held at the Council Chamber, Denison House, Westminster, when Mr. Harold Cox and Mr. C. J. Stewart gave addresses on "The Effect of War on Public and Private Extravagance," followed by a general discussion. Although Prof. Petrie, in his opening remarks, put in a plea for a simpler standard of living which was vital to the country, he yet showed the false economy of being niggardly with education in general and science in particular. In his opinion, if we derived our recreation from the cultivation of the

mind, we would no longer desire to spend so much on bodily pleasure. Mr. Cox touched the subject of economy only from the point of view of public expenditure. He emphasised the necessity of a definite policy in regard to what the State ought to be expected to provide for the people and what expenditure should come from the private purse of the individual—a line of demarcation which has hitherto not been clearly laid down. He gave the Post Office as one illustration of this point, the letter postage being self-supporting, while the parcels post was assisted out of the public funds; and the education of the people as another, which, at one time partly self-supporting, was now entirely free. After giving many reasons for his opinion that too much financial support was expected from the State, he recommended a return to older methods. He showed the leakage of money caused by maladministration, instancing the Old Age Pensions and the National Insurance Fund—both good schemes in themselves, but schemes which, because of their having been framed so hastily and carried out so carelessly, had become a heavier burden on the country than they should have done. In the case of Old Age Pensions, Ireland, who looks on England as a cow to be milked, is allowed to give these benefits broadcast to those who have means of support as well as those who are deserving. And he showed how the National Insurance Fund overlapped other societies, many cases being relieved by more than one institution. He closed his address by speaking very firmly of the necessity of retrenchment in the matter of Government salaries. Mr. Stewart, the Public Trustee, spoke only on the subject of private economy, and made a strong appeal to individuals to spend only on absolute essentials. Other points that were brought clearly to the front were that the need for economy would be permanent so far as our generation is concerned; that it is imperative for us to think in terms of labour instead of money; and that we should individually resolve to help in causing the existing markets for luxuries to be overstocked, thereby freeing much labour to be ultimately diverted into more necessary channels. Only by strenuous and united exertion along these lines will it be possible for the Allies to win the war. Germany, it was stated, is now counting on the collapse of British endurance when the financial strain touches the people individually; therefore it is only the people individually who can prove that what Germany considers a certainty will not occur.

Research for Honours at Oxford

The *University Gazette* of February 23 announces a statute which will be welcome—to the effect that research carried out under the Professors of the University of the Faculty of Natural Science will constitute a part of the examination for Honours in Chemistry in the University. With this change, the examination will consist of two divisions, of which the first would be an examination on the present lines, and the second part would require records of experimental investigations. We do not know whether the proposal will receive ultimate sanction; but it is apparently already given effect to in several other universities. For example, the Vice-Chancellor of Bristol University states (*Times*, February 26) that the last of the three years of Honours study is mainly devoted to individual researches, and that it is proposed to extend this requirement to the pass degree as well. He adds that "The University Colston Society . . . has for the past five years raised a sum averaging upwards of £500 a year, which is solely and entirely applicable to defraying the expenses of researches carried on in the University's laboratories. This form of university benefaction, which is, I think, unique in England, is greatly to the credit of our city."

Recapture and Expansion of British Trade

We have received from the International Correspondence Schools Ltd., Kingsway, London, W.C., a series of pamphlets of about thirty-five pages each called by the above title, and dealing with (1) Internal-Combustion Engineering, (2) Mechanical Engineering, (3) Textile Industries, (4) Electrical Industries, (5) Chemical and Allied Trades, (6) The Commercial War and Expert Selling, (7) Coal-Mining Industry, and (8) The Building Trade. Those interested in such pursuits will find these pamphlets a valuable and practical aid, and their small compass renders it possible for the busiest person to give them careful study. In each of them a comparison is made between English and German methods, pointing out where Germany has excelled in the particular trade under discussion, and giving practical suggestions for remedying British shortcomings. While the technical details in these pamphlets necessarily differ, in all of them one or two dominant notes are struck, such as the vital necessity of an adequate encouragement of scientific research, which has often been the means of bringing certain industries to birth, the need of co-operation between employer and employed, and the responsibility which rests with every individual to put forward intelligent and conscientious effort for eventual success.

The Athenæum Subject-Index to Periodicals

We have been very glad to receive the 1915 numbers of this valuable Index issued by the *Athenæum*, Bream's Buildings, Chancery Lane, London, E.C. One of these (on Science and Technology) consists of nearly eighty pages. It commences with articles in almost all periodicals published during the year arranged according to subjects, so that any one who is interested in a particular subject can at once look up the references. At the end of the publication there is an eight-page index of authors' names. The price is only 2*s.* 6*d.* In addition to the annual volume the *Athenæum* publishes a series of Class Lists of the kind mentioned above dealing with the following:

A. Theology and Philosophy.
B. History.
B1. The European War.
C. Geography, Anthropology, and Folk-lore.
D. Sports and Games.
E. Economic and Political Sciences. Law.
F. Education.
G. Fine Arts and Archæology.
H. Music.
I. Language and Literature.
J. Science and Technology.
K. Preventive Medicine and Hygiene.

The publication of monthly indexes will commence this year, and the annual subscription for the whole series is £2 10*s.* The Index has been brought out at the request of the Council of the Library Association. Our only criticism is that it is not made perfectly clear on the cover that each volume is only a class list, and the purchaser may possibly be misled on this point.

The Scientific Australian

No. 2 of the twenty-first volume of this useful periodical contains two special articles, one on "A New Theory on Australia's Rainfall," by V. G. Anderson, and the other "Australian Steel Foundry Practice," by A. M. Henderson. In addition to these articles on technical subjects is an interesting description of Wireless Telephony recording the fact that a conversation was conducted and clearly heard between two persons separated by a distance of 2,100 miles, and a discussion on the vexed question of the Canals of Mars and whether or not these are produced artificially. Dr. F. H. Campbell's article on "British Chemical Industries and the War" shows the interest taken in Australia in things British, and if we in England take the trouble to keep ourselves abreast of scientific affairs in the Colonies, it would certainly be one factor in maintaining the British Empire as an undivided whole.

Street Nuisances (the Editor)

Many complaints have recently been made regarding the nuisance caused in London by whistling for taxicabs, especially in the neighbourhood of hospitals and convalescent homes; and indeed several wounded officers have written to the press describing the suffering which such whistling at late hours at night causes them. As the result of a discussion in the *Times*, which was joined in by Sir Henry Morris and myself among others, I wrote to the Marylebone Borough Council asking them whether they could do anything. The Town Clerk very kindly put the matter before one of the Committees of the Council, but this Committee came to the conclusion that they were unable to take action in the matter. This is, I understand from elsewhere, due to the fact that Parliament has not passed sufficient legislation to cover such nuisances in a proper manner. The Town Clerk informs me that "at the Marylebone Police Court on the 1st inst. (February) a man who persisted in whistling for taxicabs near to a nursing home was summoned for an infringement of the Council's bye-laws, copy of which I sent to you on the 7th ult., and was fined 10*s*. It will be seen, therefore, that in certain cases the police are able successfully to take action under our bye-laws."

REVIEWS

PHILOSOPHY

Life and Human Nature. By SIR BAMPFYLDE FULLER, K.C.S.I., C.I.E. [Pp. xi + 339.] (London: John Murray, 1914. Price 9*s*. net.)

THE distinguished author of this book claims to derive special qualifications for his task from the fact that he has travelled widely, come in contact with many different races of mankind, and spent many years of his life in the government of men. Yet it is certainly true that there exists in the ordinary affairs of life something of an antithesis between theory and practice. Sir Bampfylde Fuller would probably be of the opinion that a scholar who had passed his life in laboratories and libraries would not thereby become fitted for the practical work of government. And it may, perhaps, be permitted to a scholar to doubt whether a large experience of practical affairs is of much value in the region of theory. We do not expect from a politician any important contributions to the theory of government; nor do we expect from a prostitute much new light on the psychology of love. These people do, indeed, come in contact with certain kinds of facts, and they learn more or less unconsciously that certain things are commonly associated together. But their information is quite empirical and unsystematic. It is not lit up by analysis and synthesis: it is superficial, sufficient for *their* purpose, but not truly rational. A whole multitude of convergent facts may not suffice to an uninstructed man to reach a generalisation; whereas to a trained mind the one-hundredth part of those observations would issue in a clear and definite conclusion.

These reflections are to a large extent borne out by a perusal of the present book. The *ideas* which underlie the book are not in any case original, but just such as are commonly held by the average well-informed man, who has spent the greater part of his life doing other things. The *facts* which illustrate those ideas are, on the other hand, often interesting and sometimes novel; but it may be pointed out that illustration would, in any case, present no difficulty, for abundance of familiar facts might be named in support, or apparent support, of all these ideas. If Sir Bampfylde had limited himself to a mere exposition of his special experiences he would probably have been more successful than he has been in his efforts at analysis. But he did not do so: on the contrary, the book is dominated throughout by ideas derived from a course of reading, which appears to have been of somewhat uncritical character. Most prominent among these are the theories of Bergson, and the old story of a conflict between life and matter. A doctrine of human nature founded upon a metaphysical theory is inevitably like a house built upon the sand. This is still more true when the writer has only had time for the study of science and metaphysics in the spare moments of a busy life. Those studies cannot, in these days, be adequately dealt with as the secondary hobbies of a man of affairs; they demand the exclusive time and attention of a mind developed by intense cultivation, and a knowledge infinitely wider than can be embraced within the first-hand experience of any single man.

Thus we find statements of a somewhat elementary character in this book.

Here is one: "The coal beds by which modern industry subsists are a store of energy that was accumulated by plant life in ages gone by: in fixing it the plants exerted as much power as we now obtain from the coal by burning it." Any tyro among mere theorists would know that the second of the two propositions here enunciated is altogether untrue. But Sir Bampfylde goes on: "Coal is popularly described as 'stored sunlight,' and it is true that the plants needed light in order to produce it. But so does a steam-engine need water for its functioning. Yet we do not credit the water with the power that is developed." Experience in the government of men is plainly no protection against the most palpable of false analogies in physics. Light is the source of energy incarcerated in the organic molecules of the plant; but water is not the source of energy to a steam engine, it is not the fuel. Yet upon sentences such as these Sir Bampfylde endeavours to base the doctrines of his book.

Nor in biology are his illustrations any more fortunate. "We do not see because we have eyes," says Sir Bampfylde, "but we have eyes because we have an impulse to see. This statement may appear paradoxical at first sight:" [and also I may add at second sight] "but on further consideration its truth becomes self-evident; for how, indeed, could a minute fragment of protoplasm develop an eye unless there was within it an impulse to do so, or unless it was constrained by an impulse from outside?" Yes, but what do you mean by impulse? that is the whole question. Words, words, words: Bergsonism in the thinnest of disguises: no true attempt at explanation or addition to knowledge; just an instance of the habit of "elucidating" an obscure problem by setting it in a jumble of language still more obscure.

Often Sir Bampfylde Fuller seems to travel through every kind of controversial question without apparent recognition that there is any controversy about it. Yet on many of these he is certainly well informed. "We admire our political leaders," he says. Well, some of us may, perhaps. This is not a partisan Review; so I will merely observe, with as little Bergsonian ambiguity as possible, that others do not. And now, having criticised freely, let me admit that the book does contain much matter of interest, and is certainly creditable to its author. If it cannot take first rank among other works on this subject, the reason (I venture to think) will be found in the circumstance that the laborious and lifelong researches of professional students cannot be set aside by the pastime of the leisure hours even of an able man of affairs.

HUGH ELLIOT.

What is Adaptation? By R. E. LLOYD, M.B., D.Sc. (Lond.). [Pp. xi + 110.] (London: Longmans, Green & Co. Price 2*s*. 6*d*. net.)

THIS work belongs on the whole more to the sphere of metaphysics than of science. Like so many other inquiring minds at the present time, the author is puzzled how to fit the conception of purpose into a mechanical scheme of the universe. By an interesting simile he shows how difficult it is to separate the motives of men from those of lower animals, and to affirm that the one is purposive and the other not. From this difficulty of drawing a dividing line, he infers that there is no such line; and, in short, that purpose is not a special characteristic of human beings, but runs through nature at large. The author justly insists that natural selection is a theological doctrine, for it shows how by survival of the fittest an *end* or *purpose*, viz. adaptation, is achieved. Yet he hardly appears to recognise the wide difference between this and human or intelligent purpose. For natural selection is a wholly mechanistic doctrine. The aim of adaptation was not consciously sought,

but resulted from the interaction of blind mechanical forces. Admitting that an organism is an adapted, and in so far a purposive, existence, natural selection showed how purpose could be represented in purely mechanical terms, altogether apart from conscious desire.

Now if we were to go so far as to accept Prof. Lloyd's view that there is no essential difference between human and non-human purpose, two possibilities would then confront us. *Either* purpose may be looked upon as running through the entire universe; *or* human purpose may also be expressible in mechanistic terms by some mode of statement, in the same way that natural selection expresses adaptation in mechanistic terms. Prof. Lloyd does not consider the second alternative; he discusses only the first, which leads him straight into metaphysics, where few men of science will care to follow him. If we assume that his basis is sound, which we are not yet prepared to do, then in the arguments founded upon it he appears to have got hold of the wrong end of the stick: and, indeed, to be almost unconscious that the stick in question has any other end at all.

In smaller details he is sometimes equally difficult to follow. He says, for instance, that science has "two distinct aims. The one is towards explanation of life or cosmogony; the other is towards knowledge of the manifestations of life, with a view to control." Now I for one would deny both propositions. Science has only one aim, and that the discovery of truth. It does not specially seek an explanation of life: indeed any such explanation seems so remote at present, that few but metaphysicians inquire into the question. Nor does it work "with a view to control," though that knowledge may be a very happy result of science. But we may surely claim for science an aim much higher than the merely utilitarian. Is it not the highest motive for any man to seek to know truth, to accumulate new knowledge? And that without any *arrière-pensée* of advantage to be derived from its applications. In justice to the author it must be added that his own work bears every sign of being inspired by that ideal, even though we may not agree with its conclusions.

HUGH ELLIOT.

The Differential Essence of Religion. By THEODORE SCHROEDER. Reprinted from the *New York Truth Seeker*, October 31 and November 7 and 14, 1914. [Pp. 28.]

THIS pamphlet is reprinted from articles of the author published towards the end of 1914 in the *New York Truth Seeker*. The author's purpose is to discover some factor common to all varieties of religion, such factor being then considered as the element of truth from which all error has been shorn away. In the pursuit of this purpose he discusses in turn the various factors which religions might be supposed to have in common; and makes short work of those which would be most readily suggested. Belief in outward manifestations is rejected: even belief in God is rejected as a non-essential element in religion, for no such belief is entertained by Buddhists: belief in personal immortality is also inessential; morals are not in the essence of religion; and religion is "always non-scientific." We begin to wonder what is left; and finally we learn that religion is "a subjective ecstatic experience" of a particular character further defined.

The method of procedure recalls Herbert Spencer's attempt to find a common factor between science and religion. That attempt failed, because when he produced his common factor divines were unable to recognise in it anything that they understood by religion, while men of science in not a few cases failed equally to detect in it any resemblance to science. Like most compromises, it satisfied

nobody. Probably the same fate will attend the present pamphlet. A religion which need not comprise the idea either of God or of personal immortality or of morals will scarcely be received with enthusiasm by the dignitaries of the Church; and since we are informed that it is always non-scientific, we are perhaps absolved rom offering any further criticism upon it. HUGH ELLIOT.

The Master-Key: A new Philosophy addressed to Psychologists, Scientists, Theologians, Christians, Jews, Agnostics, Spiritists, Ascetics, Orientalists, and educated persons generally. By DAVID BLAIR. [Pp. vi + 118.] (Wimbledon: Ashrama Agency, 96, High Street, 1914.)

THE above title indicates the extensive audience to which this book is addressed. We do not know what may be thought by the numerous categories of gentlemen named above, to which we do not belong. But speaking for those one or two categories to which we do belong, we must confess to a complete inability to understand the author's meaning. Nine-tenths of the book is unintelligible; and the remaining tenth traverses every belief that we have been accustomed to hold. Part I of the book, occupying six pages, settles the question of Man's Place in the Universe. Part II deals with Life on Nature, whatever that may be. Part III is Noumenoidal Life, which was such a terrifying name that we skipped it. Part IV says that the first religion was closely connected with Fourth-sphere experience. Never having had such an experience, we passed on to Part V, where, by mischance, we landed straight in the middle of the Fourth Sphere again, and were not much relieved on learning that "the worst things" living in this region "are ugly and terrifying noumenoids." Fearful of meeting such a monster, we fled into Part VI, which informed us that Jesus was a Jewish Buddha, "quite impossible in Jewish Society," and that "Messianic Christianity" is "all moonshine." Part VII brought us among the Ascetic philosophers, who certainly seem so disagreeable that we were almost forced back upon the author's own philosophy of "Monadism." But that philosophy we were still unable to comprehend. HUGH ELLIOT.

MATHEMATICS

Edinburgh Mathematical Tracts. (London: G. Bell and Sons, Ltd., 1915.)

1. *A Course in Descriptive Geometry and Photogrammetry for the Mathematical Laboratory.* By E. LINDSAY INCE, M.A., B.Sc. [Pp. viii + 79. Price 2*s*. 6*d*. net.]
2. *A Course in Interpolation and Numerical Integration for the Mathematical Laboratory.* By DAVID GIBB, M.A., B.Sc. [Pp. viii + 90. Price 3*s*. 6*d*. net.]
3. *Relativity.* By A. W. CONWAY, D.Sc., F.R.S. [Pp. viii. + 43. Price 2*s*. net.]
4. *A Course in Fourier's Analysis and Periodogram Analysis for the Mathematical Laboratory.* By G. A. CARSE, M.A., D.Sc., and G. SHEARER, M.A., B.Sc. [Pp. viii + 66. Price 3*s*. 6*d*. net.]
5. *A Course in the Solution of Spherical Triangles for the Mathematical Laboratory.* By HERBERT BELL, M.A., B.Sc. [Pp. viii + 66. Price 2*s*. 6*d*. net.]
6. *An Introduction to the Theory oj Automorphic Functions.* By LESTER R. FORD, M.A. [Pp. viii + 96. Price 3*s*. 6*d*. net.]

THESE tracts are the first six Numbers of a new series which is, for the most part, intended to respond to the need in education in higher mathematics caused

by a tendency that students may have towards a hasty satisfaction with results, such as existence-theorems, that are complete solutions only theoretically. Such theorems are nearly quite unconnected with the practical needs that are felt in every application of mathematical reasoning for numerical and graphical calculations. Of the present tracts, all, with the exception of Nos. 3 and 6, are concise guides to such practical applications. The tracts are very well printed on good paper; and the covers are fairly thick paper and of a pleasant dark green colour. It would be a good thing if the example of the more modern numbers of Ostwald's *Klassiker* could be followed in that the covers were made still more stiff. The fault is one which is shared by the Cambridge *Tracts*, and must, to a certain extent, interfere with the usefulness of an excellent series.

There are, broadly speaking, two main directions in which the teaching of science should be reformed. In the first place, we may require, on what seem to be conclusive grounds, that the subjects should be made more interesting and more readily grasped by the student, by a very free use of historical and critical methods. In the second place, a tendency has grown up of late years, even in pure mathematics, to make practical work of modelling, drawing, visualising, and calculating play a great part in education. All intelligent teachers will welcome efforts in both directions. Both directions are followed in these tracts, although the historical method is much less emphasised in them than the practical. Indeed, even some space is devoted to describing a thorough systematisation of the practical laboratory conveniences for drawing and calculating. Yet several valuable historical notes and aids to historical study will be found in them. The originality in these tracts is to be sought rather in the choice of subjects and method of treatment than results.

It is important to develop the student's ability for visualisation so that he can form easily a mental picture of three dimensional systems, and for this purpose the best thing is a course in descriptive geometry—a subject which has been unduly neglected in Great Britain. In No. 1, in which the influence of Monge is naturally very pronounced, there is a discussion of the purpose and methods of descriptive geometry; of the straight line, plane, surfaces, and curves of double curvature, in orthogonal projection; and of perspective. An admirable account of the evolution of descriptive geometry is given (p. 13); and the final chapter is devoted to photogrammetry—the method of obtaining from ordinary photographs correct metrical details about three dimensions.

No. 5 is an account of various methods—numerical and graphical—of solution of spherical triangles. The subject is important in applications to astronomy and navigation, and develops the power of dealing with questions in the geometry of position.

No. 2 is devoted to the practice of interpolation and numerical integration, and contains chapters on some theorems in the calculus of finite differences; formulæ of interpolation; the construction and use of tables; numerical and integration. There are a large number of examples for the student to work out.

No. 4 is also mainly concerned with practical applications, and the *theory* of Fourier's series is left on one side. Numerical and graphical methods in harmonic analysis are also dealt with; and a new feature in text-books is "periodogram analysis," in which we have to find the incommensurable periods of periodic terms whose sum *may* represent a given not purely periodic graph.

Prof. Conway's four lectures, which are contained in No. 3, give an historical sketch of the theory of relativity down to the stage in which it was left by Minkowski. From the student's point of view it is a piece of good fortune that this theory is as yet so new that no teacher has yet dared to turn it into the dead form of a "subject" as usually taught, so that students can still have the stimulus of an historical treatment.

No. 6 is the first text-book in English on the theory of automorphic functions. The fundamental concepts and theorems of the theory are thoroughly treated, and certain important developments of the theory—the applications to non-Euclidean geometry and the uniformisation of analytic functions—are treated in less detail. Perhaps the most valuable part of the Tract is the very full bibliography of works from 1881 to 1913, which will be of the utmost value to those who wish to make a deeper study of the theory.

PHILIP E. B. JOURDAIN.

Contributions to the Founding of the Theory of Transfinite Numbers. By GEORG CANTOR. Translated and provided with an Introduction and Notes by PHILIP E. B. JOURDAIN, M.A. (Cantab). The Open Court Series of Classics of Science and Philosophy, No. 1. [Pp. vii + 211.] (Chicago and London: The Open Court Publishing Co., 1915. Price 3*s.* 6*d.* net.)

THIS book is No. 1 of the Open Court Series of Classics of Science and Philosophy. It would have been impossible to make a better choice of an initial volume. The work consists of an Introduction by Mr. Jourdain consisting of 82 pages, and then comes Cantor's contribution; the volume closes with a few pages of notes and an index. The work of Cantor was published in the *Mathematische Annalen*, 1895–7. Perhaps it is the most strikingly original mathematical theory of the last twenty-five years. The author begins his first article with the assertion, *Hypotheses non fingo*; it marks the firmness of the foundation upon which he knew he was building. His assertion has been verified; there are, it is true, points in the theory upon which there is no certain knowledge, but the theory is built upon a rock, and will stand as long as the human mind exercises its highest functions. English students have had to wait nearly twenty years for a translation of this important book into their language; French readers in 1899 had before them M. Mariotte's translation, after indeed it had been published apparently in the *Mémoires de la Société des Sciences physiques et naturelles de Bordeaux*. And now at last this edition comes from an American publisher. Unfortunately this occurrence is so common as to excite no surprise; it is part of the pleasant fiction that English mathematicians are sufficiently accomplished to read mathematics in any language in which the Latin or Gothic script is used.

Mr. Jourdain has done his work of translation well, but it is in his long Introduction that he has established his surest claim to our gratitude; while many could have discharged the duties of translator, few could have given such a vivid historical and philosophical sketch. We have in these pages an account of the great French and German analysts to whom we owe so much in modern analysis; it is not, however, a general history. The author has selected as his thread the arithmetical theories which have rendered possible the theory of transfinite numbers, and from Fourier and Cauchy to Weierstrass and Cantor he has traced for us the true apostolic succession of modern analysts.

C.

The Science of Mechanics: a Critical and Historical Account of its Development. By ERNST MACH. Supplement to the Third English Edition, containing the Author's Additions to the Seventh German Edition. Translated and Annotated by PHILIP E. B. JOURDAIN, M.A. (Cantab). [Pp. xiv + 106.] (Chicago and London: The Open Court Publishing Co., 1915. Price 2*s*. 6*d*. net.)

THIS book is in its essence a complement to Mach's *Mechanics*, the English translation of which by Mr. McCormack was published in 1895. It consists of 57 pages of additions and alterations made by the author in the seventh German edition, two pages of corrections to be made in the *Mechanics*, and 38 pages of notes on the original text made by Mr. Philip E. B. Jourdain; there is also a very full index. From the publishers' preface we learn that this rather ambiguous plan of supplementing the English translation has been adopted by them after much thought and consultation with Prof. Mach; while in a preface by Mr. Jourdain to his notes he states that "the author's wish that no changes shall be made in the original text is binding, not only for personal reasons, but also because the work is now a classic." The notes which follow the preface have been approved by Prof. Mach. It is in hearty sympathy wlth the publishers and with Mr. Jourdain that we express our regret that the present form has been adopted. We cannot think that it is wise in a living author to stereotype his work; apparently if the literal interpretation is to be taken of the words quoted above, even "Leibniz" is to remain "Leibnitz," and "Appelt" must retain the double p. The result is either that students of the English translation are to read the work in Mr. McCormack's translation with their eyes wandering over the three sections of this book, or that they must mark down in their copy corrections, additions, and alterations, an unsatisfactory process to a bibliophile. To Prof. Mach every student of mechanics owes a deep debt of gratitude; it would have been increased if he had allowed a new edition to incorporate the additions and alterations made in the seventh German edition. In mathematics there is a sense in which it may be said there are no classics. Euclid's Elements would not have influenced mathematical thought for two thousand years if the author and his editors had regarded the text as inviolable.

The various parts of the book, whether due to Prof. Mach or to Mr. Jourdain, contain excellent matter, interesting historical facts, and acute philosophical criticism. The printing of the book is admirable.

C.

A Treatise on the Theory of Invariants. By OLIVER E. GLENN, Ph.D., Professor of Mathematics in the University of Pennsylvania. [Pp. x + 245.] (Boston, New York, and London: Ginn & Co., 1915. Price 10*s*. 6*d*. net.)

THE object of this book is, firstly, to present in a volume of medium size the fundamental principles and processes and a few applications of invariant-theory with emphasis upon both the non-symbolical and the symbolical methods; and, secondly, to emphasise a logical development of the theory as a whole and to amalgamate the methods of British and continental mathematicians. Although the original memoirs have been consulted extensively, the author has considered it expedient to give only a few references in the text, and the well-known Report of Meyer and his article in the *Encyklopädie* are referred to for other references and fuller details of the historical development. The present reviewer has not seen a more stimulating introduction to the theory. It is seldom that writers of text-books have found it possible to awake the student's interest in a branch of

mathematics and sustain it, but that is done in this book. The book consists of nine chapters, and each chapter is divided into sections. The first chapter is on the principles of invariant-theory, and the sections are concerned with illustrations of the nature of an invariant; terminology, definitions, and transformations; and special invariant formations. The second chapter deals with properties of invariants, and the sections are on the homogeneity of a binary concomitant; index, order, degree, and weight; simultaneous concomitants; and symmetry and the fundamental existence-theorem. The third chapter is on the processes of invariant-theory, and the sections deal with invariant operators; the Aronhold symbolism and symbolical processes; reducibility and elementary irreducible systems; concomitants in terms of the roots; and geometrical interpretations. The fourth chapter is on reduction, and the sections deal with Gordan's series and the quartic; theorems on transvectants; reduction of transvectant systems; syzygies; Hilbert's theorem; Jordan's lemma; and grade. The fifth chapter is on Gordan's theorem; the sixth chapter is on fundamental systems; the seventh chapter is on combinants and rational curves; the eighth chapter is on seminvariants, modular invariants, and covariants; and the ninth chapter is on invariants of ternary forms. At the end of the book there is an appendix of good and graduated exercises and theorems, and an index.

PHILIP E. B. JOURDAIN.

ASTRONOMY

Collected Papers on Spectroscopy. By G. D. LIVEING and SIR J. DEWAR. [Pp. xvi + 566, with numerous diagrams and tables.] (Cambridge: at the University Press, 1915. Price 30*s*. net.)

SCIENTISTS in general and spectroscopists in particular will be under a debt of gratitude to the Cambridge University Press for making possible, by undertaking almost the whole of the expense, this republication in collected form of the spectroscopic papers by G. D. Liveing and Sir James Dewar. The volume is handsomely produced, and in all respects worthy of the best traditions of the Cambridge Press. Such reproductions of collected papers as this fulfil a very definite aim; no doubt, as the authors remark in their preface, all the contributions to knowledge contained in these papers have already been culled and classified in Kayser's great work, the *Handbuch der Spectroscopie*; but whilst such a record is useful as an encyclopædia of knowledge, to the investigator who is seeking to retrace the steps by which the authors obtained their results, and to follow the train of their thought and argument, it is useless. For such a purpose it is essential to refer to the original papers, which generally involves the necessity of having to refer to papers scattered amongst many different volumes of the proceedings and transactions of various societies, to which oftentimes it is difficult or impossible for the investigator to obtain access. The collection of papers together in this manner avoids this necessity and at the same time enables the author's work to be viewed in a proper perspective.

The papers in this volume extend in time over a period of twenty-seven years —from 1877 to 1904. The collaboration of two scientists extending over so long a period is in itself remarkable. Their work is too well known for it to be necessary to refer in any detail to the subject-matter of these papers. A glance through the subject index makes apparent at a glance the extraordinary diversity of the subjects touched upon. At the present time, when the existence of series relations amongst the lines in the spectra of many elements is assuming so prominent a place in theories of the structure of the atom, it is interesting to note that Liveing

and Dewar were amongst the earliest investigators in discovering the existence of homologous series of lines in spectra, and of groups of lines alternately sharp and diffuse, results which foreshadowed the later developments. Then there are important investigations on wave-lengths of lines in the ultra-violet in the spectra of many elements, and on the reversal of spectral lines. A few of the other matters dealt with are origin and identity of spectra, the nature and temperature of sun-spots, the influence of pressure on spectra, problems of the atmosphere, the separation of the inert gases in the atmosphere, and studies of their spectra.

The papers are arranged—with but few exceptions, the reason for which is not quite clear—in chronological order. A supplementary and hitherto unpublished paper is added, "On the separation of gases by electric discharges with various electrodes." The classified subject index at the end, "the work of an amateur," is very well compiled, the references being full and complete, and will enable the work done on any particular point to be referred to at once. An authors' index has also been added.

The woık of Liveing and Dewar is a striking example of scientific collaboration, and this volume is a fitting tribute to the value of their united labours.

H. S. J.

Resolution of Planck's Constant and Rydberg's Constant, and the Origin and Inter-relations of Spectra Series. By JAS. STEWART. [Pp. 16.] (Paisley: J. & R. Parlane, 1915. Price 1*s*. net.)

THE author claims in this pamphlet to have established relations between the mass and charge of an electron, the velocity of light, Planck's constant h which occurs in the law of black-body radiation, and Rydberg's well-known universal constant, N, which enters into his formulæ for spectral series. From these relations, if two of these quantities are known, the others may be calculated, and assuming the accepted value of the electronic charge and Curtis's determination of N, the author proceeds to calculate the other quantities, and obtains results in fair agreement with experiment. Equally good agreements, however, have been obtained previously in other ways, and in the present instance they must certainly be regarded as fictitious, being based upon hypotheses and suppositions for which no justification is given except that the results obtained by using them are the correct ones. In addition to this, the equation of the motion of the rotating electron upon which the theory is based is incorrect, and altogether neglects the existence of a magnetic field around the electron.

The second part of the pamphlet, dealing with the origin and inter-relation of spectral series, possesses no more solid foundation than that of the first part, and is not worthy of serious consideration.

H. S. J.

PHYSICS

(*a*) **Numerical Examples in Physics.** By H. S. JONES, M.A. [Pp. xii + 332.] (London: G. Bell & Sons. Price 3*s*. 6*d*.)

(*b*) **Statics. Part II.** By R. C. FAWDRY, M.A., B.Sc. [Pp. 159–305.] (London: G. Bell & Sons, 1915. Price 2*s*. net.)

(*c*) **A Manual of Mechanics and Heat.** By R. A. GREGORY and H. E. HADLEY, B.Sc. [Pp. viii + 309.] (London: Macmillan & Co. Price 3*s*. net.)

(*a*) THE average student of Physics called upon periodically to face examination papers which contain numerical questions will find this book a veritable storehouse of questions of all types relating to the various parts of the subject. The

exercises are grouped according to their difficulty, some are as direct as possible and bear on main principles, others are similar to those carried out in the most elementary experiments, and others bear on more advanced practical methods and recent work. There are also a number suitable for more advanced students. The collection is very complete, and the questions are concrete and real. Frequently they suggest to the intelligent student methods of practical work which are not referred to in the average text-book. Short preparatory paragraphs on theory and a number of worked examples precede each section of the book.

(*b*) This excellent little book ought to be in the hands of every teacher. While the simple principles are introduced and treated in a logical and consistent manner, the problems are not of that abstract and remote type so common to the general class book of Mechanics. There is no reason why the graphical methods which the engineer uses to solve his problems should not be used by even a beginner. This book shows how such methods can be applied in a simple way to those questions which every experience of our daily life raises. When a student sees how graphical methods can be applied with ease to the roof of the house in which he lives, the bicycle on which he rides, and the bridge over which he walks, he forms a much more correct idea of the utility of Mechanics than he does after the usual school course in which he slips down "perfectly smooth" planes or drowns in "perfect" liquids.

This second part extends the application of the principles discussed in Part I. Frameworks are treated by graphical and analytical methods, Bow's notation being introduced and explained. This leads to a short account of chains and suspension bridges. The treatment of couples is illustrated by bending moments and shears. Nothing indicates the tone of the book better than the fact that such an everyday occurrence as the jamming of a drawer is used to exemplify the laws of friction. Last, but not least, the student realises that the world has three dimensions, and he is no longer compelled to live "in the plane of the paper" following innumerable "uniformly accelerated" bodies along their interminable straight paths.

(*c*) This book is substantially Parts I, II, and III of the *Class Book of Physics*, by the same authors, with many of the sections extended and a number of additions. It is written to supply the need for a single text-book on Mechanics and Heat suitable to the standard required in certain public examinations in which these two subjects are taken together.

The book is up to the standard already attained by previous works of the authors, and is adequate for the purpose referred to. The text is clearly written and covers considerable ground, while the examples are representative and well chosen. It seems a pity that a more complete account of curvilinear motion is not introduced into Chapter VIII; graphical methods can be applied with such ease to curvilinear motion that the omission of a full treatment of it from the average elementary text-book forms a serious defect. Many a boy can solve the most intricate puzzles on "falling bodies" and yet be perfectly ignorant on the subject of walking round a street corner.

J. RICE.

The Mathematical Analysis of Electrical and Optical Wave-Motion on the Basis of Maxwell's Equations. By H. BATEMAN, M.A., Ph.D. [Pp. vi + 159.] (Cambridge: at the University Press. Price 7*s*. 6*d*. net.)

THE average student who graduates in Physics has, as a rule, sufficient mathematical equipment to follow the deduction of the equations of the electromagnetic

field and the derivation of the equation of wave propagation. There the usual text-book leaves him after, perhaps, an application to the limited cases of a plane wave or a spherical wave. To those who are desirous of pursuing this part of the subject further from a mathematical point of view, Dr. Bateman's book will provide excellent guidance. Even readers with a limited mathematical equipment can easily follow the development, which is original and suggestive. A nodding acquaintance with Spherical Harmonics and Bessel Functions, such as is obtained from one of the usual text-books on Differential Equations, and with the notation of modern Vector Analysis, will serve very well.

Maxwell's Equations are reduced to two in number by the introduction of the complex field vector, and two very general types of solution of the wave equation are obtained, one of which includes the well-known solution for the Hertz dumbbell oscillator as a particular case. Chapters on transformation to polar and cylindrical co-ordinates follow, with a very full discussion concerning the problems of scattering by spherical obstacles, and of propagation of waves along a wire. The pressure of radiation and electrical vibrations on variously shaped bodies are dealt with, and Sommerfeld's multiform solution of the wave equation is applied to diffraction past a straight edge. Solutions, using ellipsoidal and toroidal co-ordinates, are also obtained, and there are short accounts of the method of solution due to Stieltjes and the method due to Green. A very suggestive chapter on the singularities of wave functions and their bearing on the problem of the structure of the ether concludes the book.

Within the limits of 160 pages the author has compressed a considerable body of work of the highest value, and ample references compensate for the necessarily brief treatment of certain parts. A good many results are given in the form of questions, and offer the adventurous student ample opportunity of trying his 'prentice hand on this most fascinating branch of Mathematical Physics.

J. Rice.

An Introduction to the Mechanics of Fluids. By E. H. Barton, D.Sc., F.R.S.E., F.P.S.S. [Pp. xiv + 249, with Diagrams and Examples.] (London: Longmans, Green & Co. Price 6*s*. net.)

There is a good deal more in this book than its title would convey. It begins with three chapters on fundamental mechanical ideas and principles, followed by a chapter on the summation of certain series, which enables the author to dispense with the notation of the calculus in subsequent chapters, and forms an introduction to the calculus, thus simplifying the work for the beginner and involving a minimum of change to those who pass to the calculus at a later stage.

Two chapters are devoted to liquids in equilibrium and flotation, and contain a very complete elementary account concerning centre of pressure, metacentre and hydrometers. A chapter on Hydrokinetics treats of Bernouilli's Theorem, Torricelli's Theorem on the velocity of efflux, the Vena Contracta, and the surface of a rotating liquid. A desirable addition here would have been a few pages on viscosity, with reference to the important problems of the motion of a solid body through a viscous fluid, and the flow of a viscous liquid through a pipe. This omission is the only serious defect in an otherwise excellent treatise. Some fifty pages are devoted to a very good account of the behaviour of gases, of Hygrometry and Barometry; and a further forty pages contain descriptions of many types of illustrative apparatus from siphons to compression pumps, Geryk and Gaede pumps, turbines and ejectors, forming a very valuable addition to the preceding theoretical discussion.

There are about 500 examples with answers and, in certain cases, solutions and hints.

The book can be recommended to students trying for entrance scholarships, and especially to technical and engineering students.

J. RICE.

CHEMISTRY

Catalysis and its Industrial Applications. By E. JOBLING, A.R.C.Sc., B.Sc., F.C.S. Reprinted from *The Chemical World.* [Pp. viii + 120, with 12 illustrations.] (London: J. & A. Churchill, 1916. Price 2*s.* 6*d.* net.)

THE phenomena which it is usual to group under the term Catalysis have long exercised a peculiar fascination for the chemist. The variety of processes which can be catalysed, and the variety of substances which function as catalysts, render the investigation of catalysis a subject of prime importance, both for chemical theory and chemical application. To attempt to present within small compass the main features of a field so wide and so varied in detail is not easy. Mr. Jobling, however, has been singularly successful, and any one who wishes to become familiar with the possibilities of catalysis in industry cannot do better than read this little book.

The first chapter deals with terms and definitions. To give an exact definition of what Catalysis stands for is practically impossible. One is forced to deal with the matter arbitrarily, and in this connection Mr. Jobling's classification will be found useful. Chapter II is devoted to the problem of the manufacture of sulphuric acid by the chamber process and the contact process, the latter being particularly well described. Chapter III deals with the industrial production of chlorine by the Deacon process, the manufacture of salt-cake (Hargreaves-Robinson process) and sulphur recovery (Claus-Chance process). The fourth chapter takes up the great problem of the fixation of atmospheric nitrogen. As stress is laid on the catalytic side of the subject, the author has necessarily restricted himself to Haber's process for the synthesis of ammonia, Ostwald's process for the oxidation of ammonia to nitric acid, and Serpek's aluminium nitride process. In Chapter V, surface combustion and its applications are considered. Chapter VI deals with catalytic hydrogenation. Many illustrations of the applicability of Sabatier's method are given, including, of course, the commercial hydrogenation of oils. The industrial applications of dehydrogenation, oxidation, dehydration, and hydrolysis are discussed briefly in Chapters VII and VIII.

The best chapter is that which deals with the fixation of atmospheric nitrogen, and more especially with Haber's process. One has only to read Mr. Jobling's description to realise the lessons it has for the chemists and manufacturers of this country. The introduction of modern physico-chemical methods is the secret of success as far as Haber's own part in the work is concerned, whilst the far-seeing policy of the Badische Anilin- und Soda-Fabrik in the support which it accorded to Haber, and the extension of the "pure" research work by the firm's own chemists, is surely an object-lesson for British manufacturers.

In dealing with a heterogeneous subject, one is anxious to know if any generalisation exists which may serve to link together apparently disconnected phenomena. In the case of catalysis our knowledge of the processes involved is still too slender to afford just grounds for comprehensive generalisation. The only attempt at unification may be summed up in the hypothesis that catalysis is essentially due to the formation of labile intermediate compounds of polyvalent

elements. This view is pretty definitely taken in Mr. Jobling's book, and there is considerable evidence in its favour. It seems reasonably well established that metals like platinum and copper owe their catalytic properties to the fact that they can give rise to at least two series of compounds in which the metals possess different valencies. Similarly the activity of the oxides of metals such as thorium, titanium, and manganese may be attributed to the polyvalent nature of the metallic element. In the same connection the idea that the instability of the metallic hydride, oxide, or salt is essential, has been borne out in numerous instances, the classical case being that of cupric chloride (in the Deacon process), whose catalytic property had been inferred by Hurter from a comparative study of heats of combination. That the mechanism of catalysis in general is obscure need scarcely be insisted on. Thus in the Claus-Chance process of sulphur recovery, in which sulphuretted hydrogen is burnt with the required amount of air, it has been found that ferric oxide is an excellent catalyst. During the process the ferric oxide is changed into pyrites, but the pyrites so formed "has some peculiar property attached to it which ordinary pyrites does not possess, for the latter is quite useless as a catalyst." Or again, in surface combustion, it has been found that the activity of the surface is affected by previous contact with the combustible gases. "Thus in most cases exposure to the combustible gas enhances the catalysing factor, whereas contact with oxygen lowers it, though to a smaller degree." An equally mysterious effect is that of "poisoning" the catalyst, the remarkable fact being the disparity between the effect produced and the minute quantity of poison required. Mr. Jobling instances the Haber ammonia process in which iron is the catalyst and sulphur one of its poisons. "The iron used as catalyst becomes quite 'dead' when it contains $\frac{1}{10}$ per cent. of sulphur, and is of little use with $\frac{1}{100}$ per cent. impurity even"

Many other problems are raised and discussed, such, for example, as negative catalysis, the function of promoters, the mechanism of induced reactions, and numerous specific effects observed in particular processes. There is not space to deal with these, but sufficient has been said to indicate the scope of the book.

There are only one or two slight misprints. On p. 10 Haber's name is twice spelt Heber, and on p. 45 (six lines from top) "pressure" occurs in place of "presence."

W. C. McC. Lewis.

The Theory of Valency. By J. N. Friend, D.Sc., Ph.D., F.I.C. (Textbooks of Physical Chemistry, Edited by Sir William Ramsay, K.C.B., F.R.S.) [Pp. xiv + 192. Second, revised edition.] (London: Longmans, Green & Co., 1915. Price 5*s*. net.)

This second edition is, to a large extent, identical with the first. Additiona matter has been introduced into Chapter V, viz. the question of the anomalous position occupied by tellurium in the Periodic System, the valency of the rare-earth elements, and the radio-elements, including a reference to Soddy's concept of isotopes. In treating the inert gases, mention is made of Mathew's attempt to connect valency with cohesion, and the conclusion to be drawn from the solubility of the rare gases in water. Chapter IX on the valency of the elements of Group I has been largely rewritten. In a later chapter, the account of Werner's theory is extended by a brief discussion of auxiliary valencies. In the most important chapter of the book, namely, that dealing with electro-chemical theories of valency, an attempt is made to bring the account up to date by a short review of modern views on atomic structure and the electronic theory of valency. One or two

omissions may be referred to. In dealing with the valency of the mercurous atom, no mention is made of Ogg's well-known electro-chemical method. Further, in connection with subvalent atoms Bose's name is omitted, and in the chapter on electro-chemical theories no place is found for Stark's views on the electro-dynamics of the atom. In general the treatment of the subject, especially the electro-chemical aspect, suffers from too brief presentation. It must be recognised that valency has become essentially a physical problem inseparably bound up with the electro-dynamics of the atom. In the reviewer's opinion, therefore, considerably more space ought to have been devoted to Rutherford's atom-model and Bohr's extension of it, not because we have yet reached a clear idea of valency on these lines—for as the author very truly remarks, our ideas about valency are in the melting-pot—but because it seems probable that along these lines the ultimate explanation of valency is to be found.

W. C. McC. Lewis.

The Fixation of Atmospheric Nitrogen. By Joseph Knox, D.Sc., Lecturer on Inorganic Chemistry, Aberdeen University. Chemical Monographs, No. 4, edited by A. C. Cumming, D.Sc. [Pp. vii+112, with 7 Illustrations.] (London: Gurney & Jackson, 1914. Price 2*s*. net.)

Dr. Cumming has performed a very useful work in undertaking the editorship of the series of Chemical Monographs to which this volume belongs. Each of these monographs deals with one specific theme in a fairly complete manner, and as the volumes are small and inexpensive, they can easily be kept up-to-date by the frequent issue of new editions.

This plan has everything to commend it from the point of view of professional chemists generally, and especially of technological chemists.

For advanced and honours students the volumes will save a large amount of time which might otherwise have to be spent in looking up numbers of papers, and, as copious references are included, they will serve as a good groundwork for those engaged in research work.

The present volume deals with the more important technological processes for the fixation of atmospheric nitrogen, and more especially with the theory on which they are based. The author has rightly confined himself to those processes which are in actual operation, or which show promise of assuming technical importance in the near future.

Chapter I. deals with fixation through nitric oxide, and gives a very clear explanation of the relative importance of electrical and thermal effects in such processes. The important work of the Haber school in this connection receives the attention which it merits. The Birkland-Eyde, Pauling, and Schrònherr processes are described in some detail. Attention is paid to important points, such as the oxidation to nitrogen peroxide, the absorption of the oxides by water and by alkalies, and the yields. In the following chapter, syntheses of atmospheric nitrogen to ammonia and ammonium compounds receive treatment, and here, necessarily, there is extended reference to the brilliant work of Haber. In the final chapter, the author introduces the methods based on the production of metallic cyanides, cyanamides and nitrides, especially Serpek's aluminium nitride process.

The book is thoroughly interesting, and the author has done full justice to the subject in the space at his disposal. We do not, however, agree with his indiscriminate use of chemical formulæ in the text to designate substances. Fig. 2,

page 47, is not sufficiently explained. It should be omitted altogether or else fully described in the text.

The book closes with the ominous sentence, "No process of any kind for the fixation of atmospheric nitrogen has yet been established in Britain." Happily, we may add, the signs are not wanting that at last this standing reproach to British industry is to be in some measure abated.

C. S. G.

Quantitative Laws in Biological Chemistry. By SVANTE ARRHENIUS, Ph.D., M.D., LL.D., F.R.S. [Pp. xi + 164.] (London: G. Bell & Sons, 1915. Price 6*s*. net.)

THIS book is founded on three Tyndall Lectures delivered before the Royal Institution in the summer of 1914. The author is convinced that biological chemistry cannot develop into a real science without the aid of physical chemistry, and considers that the aversion to exact methods exhibited by biochemists is due to their inability to appreciate such ideas as "experimental errors" or "probable errors." Physical chemists have found that the biochemical theories which are still accepted in medical circles are founded on an absolutely unreliable basis and must be replaced by other notions agreeing with the fundamental laws of general chemistry.

The first chapter, which is introductory, contains, amongst other things, an account of the graphical methods of presenting experimental results. The second chapter is devoted to the discussion of the reaction velocity of a number of biochemical enzyme actions, such as peptic digestion, the decomposition of vibriolysin and so forth. It is shown that Schutz's Rule, according to which the action is proportional to the square root of the quantity of enzyme, holds good until about 50 per cent. is digested, but after that the amount changed falls short of the value calculated according to this rule. The author's experiments have refuted the idea that the obedience to this law distinguishes organic ferments from ordinary catalysers by showing that when a great excess of ammonia acts on ethyl acetate exactly the same rule holds good until about 50 per cent. of the ammonia is used up by the formation of ammonium acetate. The third chapter deals with the influence of temperature on reaction velocity, and here again it is shown that processes in which enzymes, bacteria or other living cells are concerned follow the same laws as ordinary chemical processes; the existence of an optimum temperature is conditioned by the thermolabile nature of the substances concerned. The author refuses to accept Van 't Hoff's theory as to the reversibility of enzyme reactions, and subsequently in a rather lengthy chapter on chemical equilibria as applied to toxins and antitoxins, he develops the thesis that anti bodies do not act as enzymes or catalysers, but in reality take part in the equilibrium.

Although the book deals almost entirely with reactions of great importance in medicine and physiology, it is of too technical a nature to appeal to either doctors or physiologists, but it will convince the physical chemist that biological phenomena are subject to the same fundamental laws as ordinary chemical phenomena.

While it is undoubtedly a very creditable achievement for the author to have written this book in English, it cannot be denied that both the style and the clearness would have gained in places by a little revision.

P. H.

GEOGRAPHY

Geographical Aspects of Balkan Problems, in their Relation to the Great European War. By MARION T. NEWBIGIN, D.Sc., Editor of the *Scottish Geographical Magazine.* [Pp. x + 244, with a coloured map of South-eastern Europe and sketch maps.] (London: Constable & Co., 1915. Price 7*s.* 6*d.* net.)

THE artificial character of modern political boundaries has made it increasingly difficult to correlate States with natural regions. The problems of the Balkans, however, are bound up with topography, and their geographical aspects should be known to every one who seeks to understand the events of recent years. The reader of Miss Newbigin's very timely book will enjoy not only an excellent piece of literature, but a comprehensive view of the war-worn peninsula that will not readily be forgotten.

The absence of national life in old times among the seaward-stretching spurs and scattered isles of Greece, when a colony was the lost child of the community that sent it forth, is due to the subsidence of the Ægean area and the breaking up of former land. The central lowland of Sparta is the mere relic of a lost valley, in which a wider range of interests might have nurtured wider aspirations. The average Greek city, without a hinterland, was merely a trading port, whose true country was the sea. The mountainous masses of Albania and Serbia, belonging respectively to the Dinaric crumplings and the ancient block caught in between the Alpine folds, have been so greatly dissected as to show no guiding lines. Folk have settled in adjacent valleys, armed with suspicion rather than knowledge of their neighbours, and in many cases, such as the route from Niš to Sofia, or from Skoplje to Salonica, the roads have taken to the uplands because they could not find room in the ravines.

The great valley of Serbia, on the other hand, opens broadly to the north, but it has offered merely an inlet from hostile lands. The Morava flood-plain on the south side of the Danube has proved an incentive to attack, and Belgrade wisely set its back against the western hills. For anyone who possesses Niš, the way is fairly clear over to the Marica valley; but even this, with its cornlands and its rose-gardens, does not lead directly to Constantinople. The river on which Philippopolis and Adrianople stand turns sharply southward to an indifferent Ægean port. Constantinople is still a city to be visited from the sea.

The Balkans, again, across which a railway now climbs connecting the Danube and the Marica, that is, connecting Bukarest with the imperial east, divide the two lowlands of Bulgaria, and they are cut off so sharply on the eastern shore that it is best to take to the sea between Burgas and the northern port of Varna. According to the laws laid down by geographers, the Balkan range should have separated Rumania from Bulgaria. The indifference of mobile armies to natural regions has arranged things in quite another fashion, and the extraordinary distribution of the ethnic group known as Vlachs or Rumanians has added many problems for the statesman.

Miss Newbigin has had access to a large amount of modern literature, and makes special use of Prof. Cvijić's numerous memoirs in German periodicals. We note, by the by, that the printers ingeniously reproduce the accented c by knocking out, with varied success, the horizontal stroke of an é. Elsewhere, the author employs phonetics, which seem quite permissible in transliteration from Cyrillic characters. The trouble arises when we try to expand the letters of Romanised Serbo-Croatian. Can we then really discriminate between the two

accented types of c? Geographical studies are now encouraging linguistics, and we run considerable risks when reading the excellent maps published in Gotha or Vienna, where German versions of our Philippopolis or Janina stare us in the face. We do not like the author's Anglo-German compromise in her plural "polyen," for the Croatian "polja," or the English "poljes" or "polyes." Let us be content, however, with our new knowledge of where towns and poljes lie, and of the part they have played in the national rivalries of the peninsula. Miss Newbigin more than once suggests that these rivalries have been fostered under Turkish and even Austrian rule, and ignores the bitter history of the region from the fifth to the fifteenth centuries of the so-called Christian era. Her comments on Austrian control in Bosnia are no doubt suited to these troublous times; but some of the praise accorded to Serbia (p. 211) for her progress "since her liberation from Turkish rule" might surely be allowed also to Austrian Bosnia since 1878. We can hardly think that the author has experienced the hospitality of an up-country police-post, where the gendarmes were mixed Mohammedans and "orthodox," or has taken her coffee as an unwatched ordinary wayfarer at an outdoor "han" in Hercegovina. The scientific aspects of the book are so admirable that we regret the least intrusion of "Jugoslavic" controversy. Germans are apt to write thus of English ways in India.

The Edinburgh Geographical Institute provides a truly beautiful orographic map in colours, extending from Rome to Asia Minor. Its restraint in the matter of railways is compensated by a special map on p. 103, which is far more up-to-date than those generally available. Among the many inspiring passages in this most attractive book is the discussion of the Roman roads (p. 90), in which stress is laid on the Via Egnatia from Durazzo, the loss of which is so keenly felt by the western allies at the present day. The unhelped heroism of Serbia at the close of 1915 has indeed its "geographical aspects," like many another Balkan tragedy.

GRENVILLE A. J. COLE.

AGRICULTURE

Plant Breeding. By L. H. BAILEY. New Edition, revised by ARTHUR W. GILBERT. The Rural Science Series. [Pp. xviii + 474, with 113 illustrations in the text.] (New York: The Macmillan Co., 1915. Price 8*s.* 6*d.* net.)

WE welcome the appearance of this fifth edition of Bailey's *Plant Breeding* partly on account of the interest which attaches to the growth of the subject, partly from a friendly feeling to a familiar acquaintance on our shelves, and partly for the wealth of material which it contains. But it is not a book which we would put within reach of the young. Some of us may remember vividly the torments which we suffered in learning chemistry all over again, in the simple reasonableness of its modern physical treatment, after school-days spent in learning from the old catalogue-textbooks of chemistry. Some such torments are in store for students who study heredity from this volume; the only foundation which they will lay in their minds for further accumulation of understanding will be a quicksand—to wit "that plants are essentially unstable and plastic, and that variations between the individuals must everywhere be expected," or "that every individual plant is a distinct variety."

This is a form of agnosticism which seems to come amiss from a country where so much credit and enthusiasm is expended on plant-breeding. We venture to think it is unjustified, especially when it contradicts flatly the whole attitude of the authors in dealing with facts, as apart from their interpretation.

A part of this discrepancy is obviously due to the transitional stage through which the book is passing in this edition, the old philosophy and the new experiment not having been brought entirely together into their community of interest. We are reminded of an eminent physiologist who, having sat through a surfeit of papers in which one vital process after another had been represented by smooth log-curves, said wearily, "It is very beautiful, and one believes it while actually listening, but it *can't* be as simple as that." The fact is that we resent having things made easy for us. Again, we biologists are overshadowed and overawed by the giants of the past; we cannot write on heredity without citing Darwin, Lamarck, Weissman, and Mendel, and the mere mention of their names induces sub-conscious prejudices. One could wish that some insignificant David of biology might attempt a book on heredity which would carefully avoid making a single reference to any previous investigator, and would take the facts for granted, stringing the whole together into a logical sequence of interpretations, beginning at the physico-chemical basis of physiology in order to examine the phenomena of fluctuation, and passing through segregation and mutation to end in ecology. Such a book would be anathema to the multitude, but it would give to the earnest student at least one clear pathway through the wilderness. And it would lay down definitions which are badly needed, especially in the New World. In the present volume we find such elasticity of nomenclature that while "variation" includes the fluctuation in the form of successive leaves on a mulberry branch, the rigid term "mutation" is extended to include strange plants which crop up in a hybrid population. This way madness lies.

At the risk of seeming ungenerous we would emphasise the looseness of nomenclature in the present volume, but remembering that it sins in good company. How, for example, can the act of rogueing, even when done "on hands and knees," "push a variety to greater excellence"? the variety itself is unaffected (p. 263). Such an expression as "have a definite ideal" is not equivalent in significance to a conception of unit-characters, as the authors assert it to be (p. 202); and the use of the phrase "an antipathy to crossing" does not explain anything. In some places this looseness is indubitably mere carelessness, as in the legend to Fig. 63, where two forms of cabbage are described as "egg-shaped" and "oval" respectively; such identity may be a commercial distinction, but it is out of place in a book of this standing.

The detailed presentment of methods for determining statistical constants is a useful feature, but there seems to be a general tendency in America to overrate the value of statistical expression, at the expense of graphic presentment. Statistical methods are invaluable tools, and also provide a means of condensing data for publication; it is well that the student should know of them, but he should be shown very carefully that in too many cases they bundle together imperfectly understood data, which are not homogeneous. For ordinary work, and for the greater part of biological research, data should be kept in graphic form until nothing more can be made of them by inspection and dissection; smoothed curves, averages, and statistical constants should always be utilised, but rarely regarded as anything but compromises. A brilliant mathematician laid down the law on the subject to the present reviewer in the injunction "Never smooth any curve." Also, computation takes a deal of time.

A feature of the book which gives it a place in the reference library also is the bibliography, over sixty pages long, of plant-breeding references from 1905 to 1912. The value of the publications included in it is necessarily variable, but very few of importance seem to be omitted.

Some of the laboratory exercises embodied in the remaining eighty pages seem rather too comprehensive, or perhaps might develop manipulative skill rather than the understanding, but many of them are excellent.

In general, the volume is well worth reading. If we feel sometimes, in so doing, that there should be two volumes, one on the interesting facts alone and the other on their scientific interpretations, we may still hope that the artistic feat of uniting the two into one without visible discontinuity will ultimately be achieved in the sixth edition.

L. B.

ZOOLOGY

Life-Histories of African Game Animals. By THEODORE ROOSEVELT and E. HELLER. [Vol. I, pp. xxviii + 420; Vol. II, pp. x + 421–798.] (London: John Murray, 1915. Price 42*s*. net, 2 vols.)

SOME people maintain that the killing of animals is not justified on any grounds save those of self-defence or the need of food, and they are perhaps the only ones that will not read these two volumes with enjoyment. If ever the reasonable shooting of game animals, not of course the killing for the mere love of slaughter, should need a defence, a good one could be found in these two volumes. The expedition whose results are recorded in this work, commanded by Colonel Roosevelt, went to East Africa for the sake of acquiring information about the larger mammals and specimens to complete imperfect series in the United States Museum. Full use was made of all the material killed, and substantial additions were made to our knowledge of wild life in this part of the world.

Mr. Roosevelt attacks the problem with the vigour that characterises his activities in other spheres, with the result that the chapters dealing with theoretical questions make very vivid reading. Most biologists are familiar with the "Theory of Protective Coloration" and with some of the remarkable extremes to which various authors have endeavoured to carry it. The theory is evidently a *bête noire* to the writers of this book, and the attack on it is carried out with all the vigour, but it is pleasing to note, none of the rancour, that is usually associated with a political controversy. While not following the authors in all their contentions it must be recognised that they have challenged the supporters of protective coloration with a series of criticisms that they will find hard to meet and impossible to ignore. It is a great disappointment to find that they have not put forward any explanation of the fact that is bound to force itself on any naturalist, from the pure collector upwards, who has not had the good fortune to see the larger mammals in their natural surroundings, namely, that the coloration of most animals is such that it renders them far harder to see in the open than a similarly shaped but differently coloured piece of material would be. The authors themselves are forced, apparently somewhat grudgingly, to admit that there may be something of this sort, but if there is, it is "probably due to the inheritance of acquired characters." This phrase, meaningless, as they bring no arguments forward to support it, occurs more than once.

The chapter on Game Preserves is also interesting, and a well-deserved tribute is paid to our game laws and the way they are administered in British East Africa.

The major part of the volumes is devoted to a description of the different species, their varieties, habitat, and mode of life, and the authors are to be congratulated on their employment of a trinomial nomenclature to indicate regional variation instead of splitting one group of animals into a number of separate

species. The value of these accounts is much enhanced by maps indicating geographical distribution and the many beautiful illustrations that accompany them. There is perhaps one fault that can be found with this part of the book, and that is the classification. The book is professedly the work of field naturalists, but more attention might have been paid to the works of Lydekker and Pocock. The differences between the Tregelaphinæ and Antelopinæ are not made sufficiently clear, and the positions assigned to the Hartebeest and the Oryx are not in accordance with recent anatomical and palæontological research.

The volumes form valuable works of reference on many points, and are well worthy of a place on the bookshelf of naturalist and sportsman alike.

C. H. O'D.

Mimicry in Butterflies. By PROF. R. S. PUNNETT, F.R.S. [Pp. vii + 188, with 16 plates.] (Cambridge: at the University Press, 1915. Price 15*s.* net.)

GENERAL reader and biologist have long been familiar with the phenomenon of certain species of butterflies reproducing the wing pattern of other species not closely related to them, and thereby differing markedly from more nearly related species, and sometimes even from other individuals of their own species and sex. The problem of this resemblance has occupied a great deal of attention since the theory of natural selection was propounded, and, indeed, it has often been cited as a good illustration of the working of the theory itself. The explanation most usually given was that some of these strikingly coloured butterflies were distasteful or even poisonous, and that by mimicking such forms other and harmless species would obtain a certain immunity from the attacks of their enemies. In other words such cases were considered as striking instances of protective adaptation.

Prof. Punnett in the present volume has given a very useful account of this and allied phenomena with references to a number of the most striking examples, and the well-executed illustrations serve to bring home the main facts in a very clear manner. It is, as far as we know, the most concise and accessible account for the ordinary reader. The various difficulties in the way of accepting the usual explanations are fully dealt with, and it is concluded that, although natural selection may conceivably play some part in the preservation of the mimicking species when they arise, it cannot have helped to produce them—a criticism that has been levelled against natural selection in other fields than the present one; and, indeed, it is difficult to imagine how the first steps, of necessity rough ones, towards the model could have any selective value. Or, if they had, why such exact reproduction of the markings should have been required in order to preserve the mimic as we find in several species, for example, in *Alcidis agathyrsus* and *Papilio laglaizei*. Furthermore, as the author points out, although it is usually assumed that the agents exercising this selection are birds, yet satisfactory confirmation of this statement is lacking.

If natural selection is not sufficient to account for mimicry, and it must be admitted that Prof. Punnett makes out a very strong case against it, what is the explanation? Here, unfortunately, the book is not so satisfactory, for, as the author himself admits, "Though suggestions have been made as to the lines along which an explanation may eventually be sought, it is not pretended that the evidence is yet strong enough to justify more than suggestions." It is, perhaps, to be expected that solution should be sought in the direction of "Mendelism," and reference is made in this connection to the coat colours of mice, rats, and rabbits. But, as is admitted, it is a far cry from these semi-domesticated

animals with not more than a general unrelated resemblance to one another, and the very close copy obtaining in certain butterflies more or less nearly related to their regional distribution The volume forms a very useful and stimulating addition to the small number of books treating of this interesting subject available to the general reader, and is illustrated by excellent plates.

C. H. O'D.

The Investigation of Mind in Animals. By E. M. SMITH. [Pp. ix + 194, with frontispiece and 9 text-figures] (London: Cambridge University Press, 1915 Price 3*s*. net.)

THIS is a very readable account of some of the many problems that are encountered by the student of animal behaviour, the method in which they are attacked, and the experimental evidence that is forthcoming regarding them. It does not really deal with the investigation of mind in animals, but "the careful study and systematic observation of the behaviour of different organisms under varying conditions," as the author expresses it in the introduction. That is to say, it is the search for evidence pointing to the existence of a mind. Much work has been done in this field in the last two decades, and such a volume as the present is welcome, since from it the ordinary reader is able to get a general idea of the advances that have been made. It is the more useful as little of the work has been done in this country, and it is by no means so widely known as it should be.

Naturally the now famous thinking horses of Elberfeld claim a share of attention, but we are surprised to find that the two analyses of a series of experiments on these animals made by Claparède are not given in the bibliography. The second of these is by far the most critical examination of the horses that has yet been made. Of course in a book of this size it is easy to pick out omissions, but on the whole a good deal of ground has been successfully covered. The short bibliographies given at the end are very useful for the student who wishes to refer to the original works. The newness of the subject-matter is indicated by the fact that none of the references go back fifteen years, and most are within the last ten.

C. H. O'D.

Scottish National Antarctic Expedition. Scientific Results of the *Scotia*. Vol. IV, Zoology. Parts II—XX, Vertebrata. [Pp. 505, with 62 plates, 31 text-figures and 2 maps.] (Edinburgh: Oliver & Boyd, 1915. Price 50*s*. net.)

CERTAIN of the individual monographs occurring in this publication have appeared previously in the Proceedings and Transactions of various learned Societies, and in these cases full indication is given of their original appearance. Together with the papers now published for the first time they form the most complete account of the Antarctic Vertebrate Fauna that has yet been issued. It is pleasing to find that all its contributions are the work of British zoologists and anatomists.

The Seals, Whales, Antarctic birds including of course Penguins, the Fish, and the Tunicates are all dealt with in a very thorough and systematic manner, and valuable additions to zoology, in particular with regard to the anatomy of the seals, are made in the various accounts Not the least interesting paper is that dealing with the Whale Fisheries of the Falkland Islands and Dependencies by Mr. Salvesen, illustrated by splendid photographs of the whales, the whale boats, and the factories. This industry is becoming more and more important every year,

and has added enormously to the economic value of these remote colonies. Although many of the firms fishing in these seas are Norwegian, some are British, and the present report should induce other firms to do the same. It is interesting to notice also that the British firms came into being largely as a result of the investigations conducted by the *Scotia*, and so furnish a direct example of the way science can pave the way for commerce.

The original expedition and the publication of this, the first volume of its Reports, have been rendered possible largely by the munificence of the Coats family. In the editorial note an appeal is made for funds to continue the publication of the six further volumes that are ready. It is unfortunate that this appeal should be made at such a period of financial stress, for the object is well worthy of support, and if the remaining volumes attain the same high level as the present one, they will indeed form a valuable contribution to our knowledge of the Antarctic.

C. H. O'D.

Memoirs of the Indian Museum. Vol. V. Fauna of the Chilka Lake. No. 1, July 1915. [Pp. 146, with 10 plates.] (Published by order of the Trustees of the Indian Museum, Calcutta, 1915. Price Rs. 15.)

THE study of areas of water of varying salinity is of peculiar interest to biologists, and the survey of the fauna of Lake Chilka, undertaken by the zoologists on the staff of the Indian Museum at Calcutta, is a welcome addition to our knowledge on the subject.

Bengal's only lake is a shallow lagoon on the east coast of India, in length some 40 miles and about 10 miles broad. It is not quite shut off from the sea, there being a narrow opening at the end of a channel some miles in length. The water in the southern part of the lake remains slightly salt throughout the year, and at most seasons the rest of the main area is also slightly salt, while the communicating channel is practically as salt as the Bay of Bengal. Shortly after the breaking of the monsoon in June, an enormous quantity of fresh-water floods the lake, and from mid-August to mid-October the pressure is sufficient to drive out the salt-water completely. The level of the lake, which is at all times extremely shallow, rises from 6 to 8 ft. during the rains. Subsequently the water-level sinks, not gradually, but by a series of steps.

It is obvious that the animals which inhabit the lake must either migrate periodically or else be able to endure the water of varying salinity. Most adopt the later alternative. The fauna is principally of marine origin, while the river-crabs, which are abundant in the neighbouring tanks and rice-fields, and the pond-snails do not enter the lake. Fresh-water fish are found sparingly, but only in the period of and immediately after the rains.

The general environment of the lake is unfavourable to life, and consequently one finds that a few species predominate, each species being fairly well represented individually. There is an absence of brilliant colours, and of colours adapted for concealment.

The present volume consists of an introduction by Dr. Annandale and Mr. Kemp, and special sections devoted to the Sponges (Annandale), Echiuroidea (Annandale and Kemp), Cœlenterata (Annandale), Ctenophora (Annandale and Kemp), Polyzoa and Cirripedia (Annandale), and Oligochæta (Stephenson). A subsequent report will, we understand, be devoted to, *inter alia*, a section on the fish and the indigenous methods of catching fish and crustacea. The economic aspect of the fisheries of the lake, which are by no means inconsiderable, is being

dealt with by Mr. Southwell, who is publishing a separate report through the Government of Bengal.

As might have been anticipated by those who have followed the development of zoological research in recent years at the Indian Museum, the present volume is a valuable addition to our knowledge of the local faunas of India, quite apart from the very special interest it possesses from a bionomical standpoint, owing to the peculiar seasonal changes of the water in the lake. There is one matter for regret, and that is Dr. Annandale was unable to arrange for a study of the avi-fauna of the lake. From a visit the reviewer paid to the lake in 1908, the ornithology of the lake would appear to be of quite exceptional interest, and we trust that Dr. Annandale will be able to make good this deficit before the completion of the report on the fauna. The plankton may well be left for the future, and since it is certain to be of extraordinary interest, it may well form the subject of a special report. Short of the establishment of a temporary zoological station for at least twelve months on the shores of the lake, preferably at Satpara, it is to be feared that the fauna cannot be completely investigated; though one must not deduce from this that those groups already reported on have not been adequately dealt with.

J. T. JENKINS.

ANTHROPOLOGY

The Antiquity of Man. By ARTHUR KEITH, M.D., F.R.S. [Pp. xx + 519, with 190 illustrations.] (London: Williams & Norgate, 1915. Price 10*s*. 6*d*. net.)

THE appearance of a book by Prof. Keith on the antiquity of man is an event of great interest to students of this difficult subject. There exist wide differences of opinion among scientists both in regard to the antiquity of man as we know him and in regard to the age of that humanoid family of which true man is but one of several very distinct species. Now Prof. Keith is one of the leading protagonists of what we may call the extreme school, the school that attributes to man a very high antiquity, and thus a full statement of his case will be welcomed, for, whether or not one agrees with the author, all must admit that there is nobody in the English-speaking world more competent to present the extreme case than he.

The problem is studied in this book mainly from the anatomical point of view. Neolithic man, Late Palæolithic (Deutolithic) races, the Neandertal, Heidelberg, and Javan species, human remains in America and Africa, all these subjects are discussed in turn. Nearly two hundred pages are given up to the consideration of the famous Piltdown or Sussex skull, which was discovered in 1912 by Mr. Charles Dawson, and is regarded as the type of a new species and a new genus, *Eoanthropus dawsoni*. As is well known, even outside scientific circles, Prof. Keith had a keen controversy with Dr. Smith Woodward and Prof. Elliot Smith about the reconstruction of this skull from the fragmentary pieces of bone that were unearthed. The difficulties of this reconstruction are very fully discussed by the author, in chapters which are unavoidably very technical in character. Indeed, the whole book makes difficult reading, and is only suited for that portion of the public which possesses a considerable knowledge of anatomy.

Not the least interesting section of the book is that which deals with fossil man in North America—a subject often ignored by British writers. Remains of *Homo sapiens* have been found in that continent in strata believed to represent the last interglacial age, and these remains would therefore be roughly con-

temporaneous with the oldest undisputed remains of true man in Europe—to wit, the skeletons of the Aurignacian Age These North American relics indicate men of the same race as the existing Red Indians.

Now, the Aurignacian skeletons are, as already stated, the most ancient remains of *Homo sapiens* that are generally accepted by scientists, though the relics of the other species of the Hominidæ found at Piltdown, Heidelberg, Java, Neandertal and elsewhere are admitted by everybody to be much older. Prof. Keith, however, comes out in defence of certain other skeletons of the "modern" type which are alleged to be older than the Aurignacian, and incidentally he tries to push the Piltdown skull out of the Pleistocene back into the Pliocene. These disputed remains are those found at Galley Hill, Ipswich, Abbeville, Clichy, Grenelle, Denise and Olmo, all of which are supposed to be Early Pleistocene, and all of which Prof. Keith defends, though not all with equal certitude. The case of the remains found at Castenedolo, in Italy, is also discussed. At this place remains of *Homo sapiens* were found in a Pliocene deposit, but even Prof. Keith is sceptical of this instance, and is nclined to accept the explanation (which other authorities apply to the ' arly Pleistocene" specimens just mentioned) that the bones merely represent bur als of much later date.

With regard to the age of the Piltdown skull, Prof. Keith's arguments are not convincing. Geologists date a stratum by means of the *latest* fossil found in it. Now the beaver, which is Pleistocene, was unearthed in the same stratum with the skull of *Eoanthropus*. Hence the humanoid skull is inferred by most scientists to be Pleistocene also. It is true that Pliocene fossils were also found, but the natural explanation of these is that they are "derived" fossils. Derived fossils are quite a common geological phenomenon. Prof. Keith prefers to suppose that at Piltdown a beaver was discovered in a Pliocene stratum for the first time. The primitive character of the Piltdown skull does not give us its date. There can, we think, be little doubt that the genus *Eoanthropus* was in existence in the Pliocene, but the particular specimen found appears to have been a late survivor of an ancient type, just as Prof. Keith thinks that the Javan ape-man represented an unprogressive remnant of a still more ancient humanoid genus.

Whatever we may think of the Galley Hill, Ipswich, and Olmo finds, there is this much to be said. The Aurignacian races were as fully human as we are. True man must therefore have had some history somewhere before the Aurignacian Age. It is one of the half-dozen greatest problems of science to discover what that history was.

Prof. Keith thinks that the Australian aboriginals closely resemble anatomically the common ancestor of all the races of *Homo sapiens*, and he speculates that that common ancestor was already in existence before the close of the Pliocene. As for the Hominidæ, the humanoid family, he calculates that it arose in the Obligocene period.

A. G. THACKER.

An Introduction to the Study of Prehistoric Art. By ERNEST A. PARKYN, M.A. [Pp. xviii + 349, with 16 plates and 318 illustrations in the text.] (London: Longmans, Green & Co. Price 10*s*. 6*d*. net.)

THIS is an excellent work, and supplies a want, for there exists no other adequate and up-to-date treatise dealing with prehistoric art in general. Three chapters are given up to the wonderful work of paleolithic artists, one chapter to the Neolithic period, and the ten remaining chapters—constituting, however, only half the book—to the artistic products of the prehistoric Metal Ages. The illustrations

are of necessity one of the chief features of the book, and these are well selected and admirably produced.

To many persons the paleolithic art proves most fascinating, and the high degree of merit attained in those very ancient times is now well known. Mr Parkyn records thirty-five different animals as represented in Paleolithic Art, and the list is of interest to paleontologists not only for what it includes, but by reason of the omissions. The most notable absentee is the hippopotamus, a beast which would certainly have been included if he had been known to the savage artists; and there are no monkeys, although one anthropomorphic figure on the wall of the Spanish cave "Hornos de la Pĕna" is provided with a tail, a decoration which may or may not have been suggested by the presence in that region of our quadrumanous cousins. The animals depicted, and the conspicuous animals omitted, may eventually assist us in correlating the pictures and the three paleolithic ages in which they were drawn—namely, the Aurignacian, the Solutréan, and the Magdalenian,—with the later glacial and inter-glacial periods. There are, for instance, two mysterious drawings of elephants which Mr. Parkyn thinks are clearly not intended to represent the mammoth. What, then, is this brute? No tusks can be seen, and the animals bear a resemblance to the female of the existing Indian elephant; but is it possible that the originals were representatives of *Elephas antiquus*? If this be the case, the sketches must have been drawn in pre-Würmian times. We may call attention to a few slips in this part of the book. The bird called a "penguin" is not the real penguin, but the Great Auk. The term "Reindeer Period" is not usually applied to the Aurignacian, Solutréan, and Magdalenian ages collectively, but to the Magdalenian age only. The reindeer is not by any means characteristic of the Aurignacian, the fauna of that time indicating a much warmer climate than that of the Magdalenian.

The remainder of the book is on an equally high level with the chapters on Paleolithic Art, and the section dealing with the working of gold in the Bronze Age is particularly informing. Gold was one of the earliest metals to be worked, and much of it came from Ireland. The famous Late Keltic art is also well described; sword-sheaths, shields, mirrors, fibulæ, pottery, and many other objects being considered and illustrated. One of the very finest specimens of the art of the Early Iron Age is the famous bronze mirror found near Birdlip in Gloucestershire. Mr. Parkyn gives a good description of this, but he makes one rather important mistake. He says that the mirror was found in a grave "at the foot of the Cotteswold Hills." The grave was, on the contrary, on one of the very highest points of the Cotteswolds, the village of Birdlip being nearly 1,000 ft. above sea-level. The mistake is excusable, because the original description by John Bellows is somewhat ambiguous to any one not familiar with the district. As is well known to archeologists in the West of England, nearly all the prehistoric remains in Gloucestershire are found on high ground, the Severn Vale no doubt being at that time very marshy and almost uninhabitable; and the Birdlip find was no exception to the rule. The mirror and other specimens found with it are now in the Gloucester Museum.

A. G. THACKER.

ENGINEERING

Engineering. By GORDON D. KNOX. [Pp. xii + 276, with 17 illustrations.] (London: T. C. & E. C. Jack, 1915.)

THIS book is one of the volumes in the "Romance of Reality" series now being issued by the publishers, and the author has done his work remarkably well. In

the Preface the author writes: "The difficulty with which I have been confronted is that there is really no aspect of engineering that is not replete with romance," and so he has "sacrificed the more mechanical side of the work"; in consequence the majority of the examples illustrating the romance of engineering are taken from civil rather than from mechanical engineering. In this, the author is certainly not at fault, and his chapters on Canals, Railway Construction, Tunnels, Mining, Docks, Harbours and Breakwaters—just to mention a few—seem, under his pen, to be one long chapter of romance.

On the other hand the reader is never allowed to forget that he is reading the "romance of reality," and stern hard facts are introduced here and there with excellent results. In the chapter on Tunnels we read: "The Mont Cenis tunnel took thirteen years to build, and cost its promoters £3,000,000, the average price for each yard of work completed being £226 . . . . The Mont Cenis tunnel progressed at the rate of 7½ ft. a day; the Arlberg tunnel, masonry and all, was completed at the rate of just over 9 yds. a day, and at a cost of only £108 a yard." Also in the chapter on Mining there is food for serious thought in the figures given of the growth of the annual coal consumption in England. The table begins with 2,000,000 tons in 1735 and ends with 180,000,000 tons in 1910!

Apart from selecting suitable engineering examples the author is particularly happy in selecting passages from the writings of scientists and engineers. That from Mr. Edgar P. Rathbone in the chapter on Mining is particularly good, and contains an American definition of a mine—"a hole in the ground with a fool at the bottom and a rogue at the top."

Of the many examples a further one must suffice—a long quotation from the Thomas Hawksley Lecture by Mr. E. B. Ellington: "Peace and the accumulation of wealth are essential to industrial progress . . . the remarkable development of science and engineering which has characterised the last 150 years would have been anticipated by many centuries if it had not been for the waste of war and loss of wealth incurred in gratifying the ambition and the luxurious tastes of the rulers."

The author does not stint his own opinions, and these are at once vigorous and sound, although at times an engineer might be inclined to disagree with the more technical statements. On p. 3 the author says: "The aeroplane is a single application of the persistent efforts made in all directions to master the principles of mechanics." On p. 118 we read: "Without the discovery of mineral oil and its products, we should, as regards aerial flight, still be in the Icarus days of classical times" In the Preface we have: "I have omitted, too, all reference to the conquest of the air, the progress of which is now a matter of everyday interest. . . . Throughout, I have aimed at avoiding all technicality, as, though technical knowledge and skill is the basis of engineering, it is not, to my mind, in the technical side of the work that its romance really is." The majority would agree with the author; but the engineer would say that if there was any romance in the conquest of the air, it is due to the untiring efforts of the mechanical engineer to produce an engine developing an enormous amount of power in relation to its weight, and able to work for long periods at a time—an almost impossible problem.

We can heartily recommend this book to all classes of readers.

J. Wemyss Anderson.

HISTORY OF SCIENCE

Le Scuole Ionica, Pythagorica ed Eleata. (I prearistotelici, I.) By ALDO MIELI. [Pp. xvi + 503.] (Firenze: Libreria della Voce, 1916. Price 12 lire.)

THIS volume contains the first three chapters of that part of a projected general history of science down to the end of the eighteenth century which deals with the Ionian School, the Pythagorean School, and the Eleatic School and Heraclitus. It is to be noticed that the work is on the *general* history of science, in which all branches of science are treated in the same book for the sake of the light which they often throw on one another—a branch of knowledge of which the most distinguished living cultivator is M. George Sarton, who carried on the self-sacrificing work of editing *Isis* near Ghent before the war broke out. This volume is dedicated to M Sarton. The ambition of Prof. Mieli is to carry on the historical work begun in this volume down to the end of the eighteenth century, the nineteenth being excluded because "it is yet too close to us." However, it seems a pity to defer writing a history of a period until a great deal about the period has been forgotten. Indeed, it is quite possible that some important things will, unless we are careful, be forgotten even so soon as in the next few years, owing to the present spread of culture at the point of the bayonet or in an atmosphere of poisonous gas.

A fuller description of the contents is as follows: The first chapter (pp. 3–207), on the Ionian School, deals with the Greeks of Asia Minor and their relations with Egypt, Thales and his meteorological and astronomical knowledge, the introduction of mathematics into Greece, the speculations of the Ionian philosophers on "the primordial element," the astronomical and cosmogonic ideas of the Ionians and their geological, biogenetic, and anthropogenetic theories, Ionian geography, and the technical arts with the Ionians. To this and the other chapters are appended critical bibliographies, in which British works are rather incompletely listed, and indexes. The second chapter (pp 211–78), on the Pythagorean School, deals with Pythagorean arithmetic, geometry, acoustics, astronomy, etc., and Archytas and the Delian problem. From a scientific point of view the most interesting parts are on the Pythagorean theory of number and arithmetical concepts (pp. 227–58), geometry (pp. 259–80), and the relation of acoustics to the subject of the progressions (pp. 230, 256–8). The third chapter (pp. 381–503), on the Eleatics and Heraclitus, contains an account of the works of Xenophanes, Parmenides, Zeno, Melissus, and Heraclitus.

The chief point of interest in this period is the well-substantiated opinion that the Pythagoreans held that geometrical figures were composed of points—of a finite number of points in fact—and the refutation of this opinion by the Pythagorean discovery of irrationals (pp. 253–6, 267–72) and also by the wonderfully subtle arguments of Zeno (pp. 231, 240–1, 272 440–4, 447–8). Prof. Mieli follows Paul Tannery in his interpretation of all of this, and, though there is not a great deal that is new in this book, it is a careful and useful work and should serve as a foundation for future investigations.

PHILIP E. B. JOURDAIN.

The Positive Sciences of the Ancient Hindus. By BRAJENDRANATH SEAL, M.A., Ph.D. [Pp. viii + 295.] (London, New York, Calcutta, Bombay, Madras: Longmans, Green & Co., 1915. Price 12*s.* 6*d.* net.)

THESE studies in Hindu science are intended to serve as a preliminary to some studies of the author on comparative philosophy; and Dr. Seal remarks truly that

"philosophy in its rise and development is necessarily governed by the body of positive knowledge preceding or accompanying it" (p. iv). The knowledge presented in this book may be assigned to the period 500 B.C. to 500 A.D., and Colebrooke's account of Hindu algebra and Hoernle's of Hindu osteology have made it unnecessary for Dr. Seal to write separate monographs on these subjects; but he rightly remarks (p. iii) that the former account requires to be brought up to date. There are seven chapters: the first deals with the mechanical, physical, and chemical theories of the ancient Hindus; the second with Hindu ideas on mechanics; the third with their ideas on acoustics; the fourth with those about plants and plant-life; the fifth with those on the classification of animals; the sixth with those on physiology and biology; and the seventh with Hindu doctrine of scientific method.

The author gives extensive quotations from the Sanskrit text, and remarks: "I have not written one line which is not supported by the clearest texts. The ground trodden is, for the most part, absolutely new. Fortunately, the Sanskrit philosophico-scientific terminology, however difficult from its technical character, is exceedingly precise, consistent, and expressive. I may add that I have occasionally used (perhaps with a questionable freedom) scientific terms like isomeric, polymeric, potential, etc., in a broad sense, as convenient symbols to express ideas nearly or remotely allied" (p. iv). Here is indicated the first point which must be criticised. Such terms as "the conservation of energy," "the dissipation of energy," "mass," and "energy" are names which have grown up with years of exact experimental and mathematical research, and the reviewer cannot but regard it as a mistake, which is only saved from being misleading by its absurdity, to attribute what is denoted by these names to the ancient Hindus. A result of this mistake is to make parts of Dr. Seal's book read like a popular exposition of the results of modern science written by a very incompetent hand. In particular, pp. 3, 6–8 are like a pale reflection of passages of Herbert Spencer. What Dr Seal calls "the conservation of energy" (p. 12) is less of a discovery than the "discovery" that philosophers used to be so fond of talking about that "there must be some constant in phenomenal variation." When dealing with Udyotakara's remark that the gravity of a body is not the same as the sum of the gravities of the particles. Dr. Seal actually remarks that "The concept of mass in the New Mechanics of Lorenz may lend some countenance to this curious metaphysical speculation" (p. 140); and this is written of a time when "Galileo's discovery was not anticipated, as Galileo's observations and measurements of motion were wanting" (p. 141). This is a *reductio ad absurdum* of the attempt to make Hindu science an anticipation of modern science.

A second ground of criticism is the utterly exaggerated importance given to such simple remarks as those which Dr. Seal calls "the discovery of the principle of the Differential Calculus" (pp. 77–80, 150), and "the anticipation of the foundations of solid geometry" (pp. 117–18, 150).

It is interesting to notice that the traditional three "laws of thought" appear in ancient Hindu logic (p. 245), and that Mill was anticipated by the Hindus in parts of his theory of inductive method (pp. 254–62).

PHILIP E. B. JOURDAIN.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Five-Figure Mathematical Tables. Consisting of Logs and Cologs of Numbers from 1 to 40,000, Illogs (Antilogs) of Numbers from ·000 to ·9999, Lologs (Logs of Logs) of Numbers from 0·00100 to 1,000, Illologs (Antilologs) of Numbers from 6·0 to 6·5000, together with an Explanatory Introduction and Numerous Examples. Also, Trigonometrical Functions and their Logs of Angles from 0°—90° at intervals of 1 minute. With Subsidiary Tables. Compiled by E. Chappell, B Sc., A.C.G.I., Assoc. M Inst. C.E., Temporary Naval Instructor R.N., Late Lecturer in Engineering Science at the City and Guilds' (Engineering) College, Imperial College of Science and Technology, London, S.W. London: W. & R. Chambers, 38, Soho Square, W., and Edinburgh: 339 High Street, 1915. (Pp. xvi + 320.)

A Budget of Paradoxes. By Augustus De Morgan, F.R.A.S., and C.P.S. of Trinity College, Cambridge. Reprinted, with the Author's additions, from the *Athenæum*. Second Edition. Edited by David Eugene Smith. In two Volumes. London: The Open Court Publishing Company, 1915. (Vol. I., viii + 402, Vol. II. 387.) Price 30*s*. net.

Numbers, Variables and Mr. Russell's Philosophy. By Robert P. Richardson and Edward H. Landis. Reprinted from the *Monist* of July 1915. London and Chicago: The Open Court Publishing Company, 1915. (Pp. 59.)

Fundamental Conceptions of Modern Mathematics. Variables and Quantities. With a Discussion of the General Conception of Functional Relation. By Robert P. Richardson and Edward H. Landis. Chicago and London: The Open Court Publishing Company, 1916. (Pp. xxi + 216.) Price $1.25 net.

Euclid's Book on Divisions of Figures. With a Restoration Based on Woepeke's Text and on the Practica Geometriæ of Leonardo Pisano. By Raymond Clare Archibald, Ph.D., Assistant Professor of Mathematics in Brown University, Providence, Rhode Island. Cambridge: at the University Press, 1915. (Pp. vii + 88.) Price 6*s*. net.

Napier Tercentenary Memorial Volume. Edited by Cargill Gilston Knott. Published for the Royal Society of Edinburgh by Longmans, Green & Co., 39, Paternoster Row, London, 1915. (Pp. 441, with 15 Plates.) Price 21*s*. net.

The Mathematical Theory of Probabilities and its Application to Frequency Curves and Statistical Methods. By Arne Fisher, F.S.S. (London). Translated and Edited from the Author's Original Danish Notes with the Assistance of William Bonynge, B.A. (Belfast). With an Introductory Note

by F. W. Frankland, F.I.A., F.A.S., F.S.S., Examiner in Statistical Methods and in Pure Mathematics to the Government of New Zealand. Vol. I., Mathematical Probabilities and Homograde Statistics. New York: The Macmillan Company, 1915. (Pp. xx + 171.) Price 8*s.* 6*d.* net.

A Voyage in Space. A Course of Six Lectures Adapted to a Juvenile Auditory. Delivered at the Royal Institution at Christmas, 1913. By H. H. Turner, D.Sc., D.C.S., F.R.S., Savilian Professor of Astronomy in the University of Oxford. With over 130 Illustrations. London: Society for Promoting Christian Knowledge, Northumberland Avenue, W.C., 1915. (Pp. xvi + 299.) Price 6*s.* net.

Exercises in Practical Physics. By Arthur Schuster, Ph.D., Sc.D., Sec. R.S., Honorary Professor of Physics in the University of Manchester, and Charles H. Lees, D.Sc., F.R.S., Professor of Physics in the University of London (East London College). Fourth Edition, Revised. Cambridge: at the University Press, 1915. (Pp. x + 379.) Price 7*s.* net.

Elementary Applied Mechanics. By T. Alexander, C.E., M.Inst. C.E.I., M.A.I. (hon. causa), 4th Order Meiji, Japan, 1887, Kogaku Hakushi, 1915, Professor of Engineering, Trinity College, Dublin, and A. W. Thomson, D.Sc., Emeritus Professor of Engineering, College of Science, Poona. With numerous Diagrams, and a Series of Graduated Examples carefully worked out. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. xx + 512.) Price 15*s.* net.

The Universe and the Atom. The Ether Constitution, Creation and Structure of Atoms, Gravitation, and Electricity, Kinetically Explained. By Marion Erwin, C.I. Illustrated. London: Constable & Co., 10, Orange Street, Leicester Square, W.C., 1915. (Pp. 314) Price 8*s.* 6*d.* net.

An Introduction to the Physics and Chemistry of Colloids. By Emil Hatschek. Second Edition, with 17 Illustrations. London: J. & A. Churchill, 7, Great Marlborough Street, 1916. (Pp. ix + 107.) Price 3*s.* net.

An Introduction to the Principles of Physical Chemistry from the Standpoint of Modern Atomistics and Thermodynamics. A Course of Instruction for Students intending to enter Physics or Chemistry as a Profession. By Edward W. Washburn, Professor of Physical Chemistry in the University of Illinois. First Edition. New York: McGraw-Hill Book Company, Inc., 239 West 39th Street. London: 6, Bouverie Street, E.C. (Pp. xxv + 445.) Price 15*s.* net.

Aircraft in Warfare: The Dawn of the Fourth Arm. By F. W. Lanchester, M.Inst. C.E., M.Inst. A.E., Member Advisory Committee for Aeronautics. With Introductory Preface by Maj.-Gen. Sir David Henderson, K.C.B., Director-General of Military Aeronautics. With 14 Plates. London: Constable & Co., Orange Street, Leicester Square, 1916. (Pp. xviii + 222.) Price 12*s.* 6*d.* net.

Specification and Design of Dynamo-Electric Machinery. By Miles Walker, M.A., M.I.E.E., Professor of Electrical Engineering in the Faculty of Technology in the University of Manchester (Manchester School of Technology), Consulting Designer to the British Westinghouse Electric and Manufacturing Co., Ltd. With Illustrations. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. xix + 648.) Price 32*s.* net.

The Telephone and Telephone Exchanges: Their Invention and Development. By J. E. Kingsbury, M.I.E.E. With Illustrations. London: Longmans,

Green & Co., 39 Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1914. (Pp. x + 558.) Price 12*s.* 6*d.* net.

Metamorphic Geology. A Text-Book by C. K. Leith and W. J. Mead, University of Wisconsin. New York: Henry Holt & Co., 1915. With 16 Plates and 35 Figures. (Pp. xxvii + 337.)

Scientific Papers. By Sir George Howard Darwin, K.C.B., F.R.S., Fellow of Trinity College, Plumian Professor in the University of Cambridge. Volume V. Supplementary Volume, containing Biographical Memoirs by Sir Francis Darwin and Prof. E. W. Brown, Lectures on Hill's Lunar Theory, etc. Edited by F. J. M. Stratton, M.A., and J. Jackson, M.A., B.Sc. Cambridge: at the University Press, 1916. (Pp. lv + 81.) Price 6*s.* net.

The Apple. A Practical Treatise Dealing with the Latest Modern Practices of Apple Culture. By Albert E. Wilkinson, Department of Horticulture, Cornell University. London: Ginn & Co., and Boston, New York, Chicago, Atlanta, Dallas, Columbus, and San Francisco. (Pp. xii + 492.) Price 8*s.* 6*d* net.

The Mechanism of Mendelian Heredity. By T. H. Morgan, Professor of Experimental Zoology, Columbia University; A. H. Sturtevant, Cutting Fellow, Columbia University; H. J. Muller, Assistant in Zoology, Columbia University; C. B Bridges, Fellow in Zoology, Columbia University. Illustrated. London: Constable & Co. (Pp xii + 262.) Price 12*s.* net.

The Alligator and its Allies. By Albert M. Reese, Ph.D., Professor of Zoology in West Virginia University. With 62 Figures and 28 Plates. London and New York: G. P. Putnam's Sons, The Knickerbocker Press, 1915. (Pp. xi + 358.) Price 10*s.* 6*d.* net.

The Embryology of the Honey Bee. By James Allen Nelson, Ph.D., Expert, Bee Culture Investigations, Bureau of Entomology, U.S. Department of Agriculture. With Diagrams and 6 Plates. Princeton University Press, Princeton, and London: Humphrey Milford, Oxford University Press, 1915. (Pp. 282.)

Principles of General Physiology. By William Maddock Bayliss, M.A., D.Sc., F.R.S., etc., Professor of General Physiology in University College, London. With 259 Illustrations. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1915. (Pp. xx + 850.) Price 21*s.* net.

How to Keep Fit in the African Tropics. A Pamphlet for the use of Soldiers on the Insect Pests and Diseases likely to be met with by an Expeditionary Force operating in British East Africa. Printed for His Majesty's Stationery Office by Harrison & Sons, St. Martin's Lane, W.C. (Pp. 46.)

One of the very best little text-books for soldiers ever published; but it will apply not only to the African tropics but the tropics at large. The author (who is one of our most distinguished men of science, though his name does not appear in the book) has a most excellent but easy style. The pamphlet begins with a good introduction followed by biographical and climatic information. Then there is a very good brief account of insect pests, followed by a similar one of the diseases, fevers, worms and diseases of animals occurring in warm climates. Although the pamphlet could almost be put in the waistcoat pocket it contains most of the matter generally laid out more diffusely in bulky text-books of tropical medicine. It will be useful not only to soldiers but also to medical officers who have not been through a course of tropical medicine.—R. R.

Sleeping Sickness. A Record of Four Years' War against it in Principe, Portuguese West Africa. By B. F. Brutoda Costa, Licentiate First Class and Chief of the Mission, J. Firmino Sant' Anna, Licentiate First Class, A. Correia Dos Santos, Licentiate First Class, and M. G. de Araujo Alvares, Licentiate Second Class. Published in Portuguese in "Archivos de Hygiene e. Pathalogia Exoticas," vol. v., March 30, 1915. Translated by permission of the Lisbon School of Tropical Medicine by J. A. Wyllie, F.R.G S., Lieut.-Colonel, Indian Army (Retired); Hon. Associate Centro Colonial, Lisbon; Corresponding Member Lisbon Chamber of Commerce; Hon. Moderator in Portuguese, London Chamber of Commerce. With 68 Illustrations and 2 Maps. Published for the Centro Colonial, Lisbon, by Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, London, 1916. (Pp. xii + 261.) Price 7*s*. 6*d*. net.

An Inquiry into the Statistics of Deaths from Violence and Unnatural Causes in the United Kingdom; with Special Reference to Deaths from Starvation, Overlying of Infants, Burning, Administration of Anæsthetics and Poisoning. Being a Thesis approved for the Degree of Doctor of Medicine in State Medicine (and University Medal) at the University of London. By William A. Brend, M.A., Camb., M.D., B.Sc., London, of the Inner Temple, Barrister-at-Law; Lecturer on Forsenic Medicine, Charing Cross Hospital; Medical Referee under the Workmen's Compensation Act. London: Charles Griffen & Co., Exeter Street, Strand, 1915. (Pp. 80.) Price 3*s*. 6*d*. net.

The Eugenics Review. Published Quarterly by the Eugenics Education Society, Kingsway House, Kingsway, W.C. Agent for Canada and U.S.A.: B. W. Huebsch, 225 Fifth Avenue, New York. Vol. vii. No 4. January 1916. (Pp. 229 + 311.) Price 1*s*. net. Annual Subscription, 4*s*. 8*d*.

ANNOUNCEMENTS

British Science Guild. Meetings of the Executive Committee, 3 p.m., April 11, May 9.

The Royal Society. Ordinary Meetings, 4.30 p.m., April 6, 13, May 11, 18, 25, June 1, 8, 22, 29.

Royal Astronomical Society Meetings, 5 p.m., April 14, May 12, June 9.

Chemical Society. Meetings, 8.30 p.m., April 6, May 4, 18 (with Lecture by Prof. F. Gowland Hopkins), June 1, 15. Informal Meeting, 8 p.m., May 11.

Linnean Society. Meetings, 5 p.m., April 6, May 4, June 1, 15 (Council only). 3.30 p.m., May 24.

Zoological Society. Meetings, 5 p.m., April 4, 18, May 9, 23, June 6.

Institution of Mechanical Engineers. General Meeting, 8 p.m., April 14.

Society of Tropical Medicine and Hygiene. Meetings, 8.30 p.m., May 19, June 16.

The Universities Bureau of the British Empire announce with regret their inability to publish their Yearbook for 1916. They hope that in a double number to be published early in 1917 no facts of historical interest will be overlooked.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

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